Yours truly
T. C. Martin

TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS,

VOL. VI.

MEETINGS OF

November [illegible], 1888.
December [illegible], 1888.
January [illegible]th, 18[illegible].
February 12th, 1889.
March 12th, 1889.

May 22d, 1889.
June 25th, 1889.
September 10th, 1889.
October 15th, 1889.
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NEW YORK CITY U. S. A.
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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

SECRETARY'S BULLETIN.

The fifth volume of the Institute Transactions was completed with the issue dated September, 1888. A revised edition of the volume made up of printed sheets, corrected by the authors, consecutively paged and thoroughly indexed, will be ready for distribution to all members in good standing in a few weeks. Arrangements may be made in advance with the Secretary for binding the volume in half morocco with cloth or paper sides, as preferred, at an expense not exceeding $1.25. The first four volumes may also be bound in one at the same price, making a book nearly as thick, provided the numbers are returned to the Secretary for that purpose. Twenty-five cents should be enclosed for return postage, otherwise the book will be sent by express.

The Editing Committee has decided to begin the sixth volume with the year 1889, in order that the date of the volumes will hereafter coincide with the calendar year. This involves no omission of matter, the proceedings being recorded continously, and the current issue for January actually covers the apparent hiatus between the September and January issues. It was also decided that the monthly numbers be hereafter issued as the permanent and official publication of the Transactions, so that in future it will be necessary that they be carefully preserved by those who desire to keep a complete file.

At the regular monthly meeting of Council, held December 4th, a letter was read from the officers of the Institution of Civil Engineers, London, inquiring if any of the Institute members expected to visit Europe next season, in order that arrangements might be made to render their stay in England as pleasant and useful as possible. The Secretary was instructed to confer with the Secretary of the American Society of Mechanical Engineers, in order that the different engineering associations might act in unison, thus obtaining special rates and accommodations, which would be mutually beneficial.. If 150 passages are secured, the excursion rate from New York to Liverpool will be fixed at $110, with a special steamer by the Inman line. A suitable circular will be prepared in a few days, giving all necessary details. Since the meeting of Council a letter of similar tenor has been received from the Society of Arts, London.

The following gentlemen were elected to associate membership, their applications having been properly endorsed:

Victor Nicholls, Electrician and M'g'r Vancouver Electric Ill'g Co., (Ltd.) Vancouver, B. C.

H. Eugene Chubbuck, Manager and Electrician, Champion Electric Light Co., Springfield, O.

The following associate members were transferred to full membership upon recommendation of the Board of Examiners:

O. B. Shallenberger, Pittsburgh, Pa.
Dr. Leonard Waldo, Lockport, N. Y.

At the 30th meeting of the Institute, held December 18th, a paper was read by Mr. W. J. Jenks, Member; Director of the Edison Standardizing Bureau, on "Six Years Practical Experience with the Edison Chemical Meter."

At the 31st meeting of the Institute, held January 8th, a paper was read by Mr. E. G. Acheson, Member; Electrician of the Standard Underground Cable Company, on "Lightning Arresters, and the Photographic Study of Self-Induction."

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. VI. New York, January, 1889. No. 1.

Covering the months of October, November and December, 1888.

SPECIAL MEETING.—EXHIBITION EVENING.

The twenty-ninth meeting of the American Institute of Electrical Engineers met on the 13th of November, at 8 P. M., at the College of the City of New York. Mr. Ralph W. Pope, the Secretary, called the meeting to order and said:

I received a letter of regret from President Weston to-day saying that he would be unable to be present with us this evening, and trusting that we would have a pleasant and successful evening, of which there is no doubt. It will be incumbent upon you to appoint a chairman for the evening, and I am ready to receive nominations.

On motion of Mr. T. C. Martin, Captain O. E. Michaelis was chosen Chairman. Captain Michaelis, on taking the chair, said:

Gentlemen: I am very much obliged to you for this mark of your kindness. At this first meeting of the Institute for the current working year, we may especially congratulate ourselves upon having papers presented to us in such a very desirable form. We have here an opportunity for properly illustrating papers, and I am sure we are all very thankful indeed to the officers of the College who have extended these facilities to us. As is our custom, there will be no business transacted this evening except the reading of the papers and the discussion thereupon. The first paper of the evening is on "The Geyer-Bristol Meter for Direct and Alternating Currents," which the author will read. It is needless for me to introduce this gentleman to you. He is a graduate of this institution, and for the past twenty years has been pupil and coadjutor of Professor Morton of the Stevens Institute.

Professor Geyer then read the following paper:

THE GEYER-BRISTOL METER FOR DIRECT AND ALTERNATING CURRENTS.

BY PROFESSOR WILLIAM E. GEYER.

In the meter about to be described we make use of the heating effect of the current. Electric measuring instruments depending on this heat effect are not new. In the Cardew voltmeter we have an application which has found much favor. Here the current of greater or less strength traverses a long, thin wire, heats it more or less, and the direct expansion is a measure of the current, and indirectly of electromotive force.

In an ammeter it is necessary to keep down its resistance, and I therefore doubt whether direct expansion can be usefully applied for this purpose; for the actual elongation of a bar of metal even when raised through a considerable range of temperature is very small.

In the familiar compound bar we have a case where a very small actual elongation produces a relatively very great lateral displacement.

I think I shall best be able to explain to you our meter, by recalling to your minds this old device. In the simplest compound bar, two strips of metal which have different co-efficients of expansion are securely soldered flat-wise along their entire length. Brass and steel are metals frequently taken. On heating, the brass expands more than the steel, and in consequence the bar bends, becoming convex on the side of the brass. When such a bar is heated by the passage of the electric current, it will deflect, and this deflection may be made a measure of the current. The disadvantage of such an instrument would be that atmospheric changes of temperature would also cause deflection, so that troublesome corrections would have to be introduced.

In our meter we also use a sort of compound bar, but eliminate at once the effect of surrounding temperature by taking metals whose co-efficients of expansion are the same or sensibly equal; in fact we take the same metal. Our first form of construction was as follows: A wire of german silver is laid upon a strip of german silver of considerably greater cross-section and radiating surface. The wire and strip are soldered together at one end; separated for the remainder of the length by a film of mica, then tied together at frequent intervals with silk or other insulating material, and suitably supported or clamped at the unsoldered end. If now a current, either continuous or alternating, is allowed to enter the strip at one end, it runs along its length, there enters the wire, and leaves the instrument from the other end of the wire.

For a given current in the wire, on account of its greater resistance and also on account of its smaller radiating surface, the wire becomes hotter than the strip. In consequence of the difference of expansion, the bar bends, becoming convex on the side of the smaller conductor. This combination we call a differential bar.

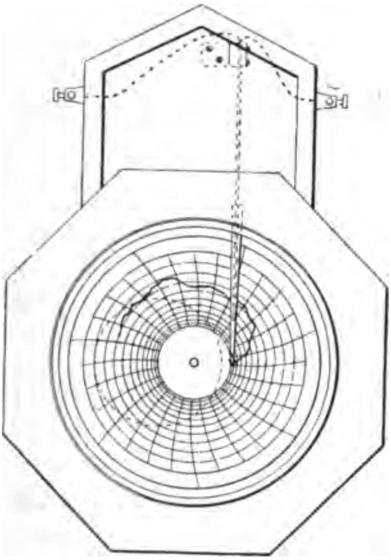


Fig. 1.

We would also state that inasmuch as the results obtained by the use of this instrument are due to the excess of the heating effect of an electric current upon one portion of the bar or its equivalent over the other, it is in a measure immaterial to the principle of the invention whether the current which produces the differences in temperature be caused to heat the two parts directly or indirectly. For example, the more expansible part, in lieu of being included directly in the circuit may be arranged in close proximity to, but insulated from, a wire or conductor which is heated by the current. The other part or element may be in the circuit or not; but in either case formed or arranged to be less sensibly heated than the other.

It will readily be seen, however, that to heat one or both of the parts of the device in this manner would be clearly equivalent to connecting them both in series in an electric circuit. Unlike the case where magnetic action is employed, the force here available is very considerable, so that to make the instrument self-recording it is only necessary to attach an inking device to the free end and move in front of it a properly ruled chart. In this respect we have no difficulty whatever. My associate, Professor Bristol, who has done the larger part of the work, has since our first experiments very much simplified the method of constructing the differential bar, and the one used in the instrument exhibited was made by placing a flat strip of german silver between a pair of dies which make alternate depressions and elevations along the length of the bar; the wire, insulated with asbestos was then slipped into the tube-like space thus formed, and the whole pressed between plates provided with grooves of the proper depth, so as to leave the wire to one side of the centre line and at the same time to insure its being held firmly by the little bridges along the length of the bar. Since the bars can be constructed essentially by these two machine operations it is evident that they may be readily reproduced and at a very small cost.

We have determined experimentally the best relation between the cross-section of the strip and wire to give maximum deflection. We believe the instrument could readily be made integrating but doubt the desirabilitity of doing so. Our reason for this could probably not be better expressed than in the words of an eminent engineer, Mr. Charles E. Emery, in his paper on "Heating Cities by Steam," before the Franklin Institute, which we quote as follows :—

"It was at first considered unfortunate that a reliable meter could not be obtained, which, like a water meter, would show by differences of reading the quantity of steam used for the interval between observations directly without calculation, and without expense of maintaining a time register at each location, and of integrating the charts afterward. This system, however, proved a blessing in disguise. The greatest difficulty in settling with consumers lies in the fact that employès waste the steam. This is particularly the case during the heating season, when steam for various uses is left on continuously during nights and Sundays, thus increasing the time of consumption from, say, 60 hours a week to 168 hours. In many cases, too, the rate of consumption keeps uniform during the night as well as during the day, so that it is an easy matter to more than double the bills. The consumers at first naturally lay the blame to the steam of the company, but the meter charts have been the means of enabling the company to satisfy consumers, when, and to what extent, the increased bills were due to mismanagement on their premises."

Substitute electricity for steam, the reasoning will apply perfectly to our case.

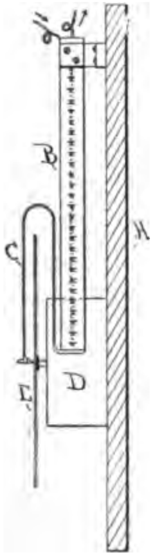


Fig 2.

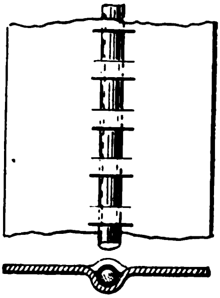


Fig. 3.

Figure 1 is a general view of the differential bar, mounted in a case with inker and recording dial.

Figure 2 is a side view of the important parts, the case being removed.

A. Supporting framework.
B. Differential bar.
C. Inking pointer attached to bar.
D. Clockwork moving dial.
E. Revolving dial for receiving record.

Figure 3 shows a small portion of the bar and a cross-section on an enlarged scale.

Professor Geyer then explained the practical operation of the meter as follows :

I will just for a moment call your attention to the instrument. You will see it has been recording for the time you have been here. By the kindness of the authorities here, I have been furnished with current as you see which here supplies a rack of lamps, and as I turn the lamps off and on, you can notice that the pointer gradually goes down or up, leaving its record. The pointer does not respond instantly to changes of current, but, as in actual use this record is spread over days, and in fact probably over weeks and months, this, I think, would not be a serious inconvenience. The clock that we have here at present, causes that dial to rotate once in twelve hours. Of course we might have a clock rotating faster or more slowly. If it had been convenient, I should have gotten one where the dial moved much more rapidly.

DISCUSSION.

THE CHAIRMAN :—Gentlemen, I am sure we are all very grateful to the Professor for his clear and able exposition, and I now declare the subject open for discussion. The gentlemen present understand that the discussion is not limited to the members of the Institute, but we would be very glad to hear from any of our friends who are with us this evening.

PROFESSOR ALFRED M. MAYER :—I ask one question merely to elucidate farther what I have heard, that is as to the linear quantity or distance that the axis of bar and wire are separated.

PROFESSOR GEYER :—I have not estimated or tried to measure exactly what that distance is. Our idea is to bring the wire of course very close to the centre line of the strip, but of course not coinciding with it. I have several bars here finished, which the gentlemen are at liberty to examine.

DR. SCHUYLER S. WHEELER :—I would like to ask Prof. Geyer the effect of changes of atmospheric temperature on the meter.

PROFESSOR GEYER :—We have experimentally tried to find out whether atmospheric changes of temperature did affect it, by taking it in the winter time from an ordinary heated room into a hall-way or passage-way where the temperature was below freezing, and then passed the same current (as indicated by another instrument through it, without being able to detect any difference in the reading. It is the difference of the temperature in the two bars, which counts, not the actual temperature of either one.

MR. FRANCIS B. CROCKER :—I would like to ask Professor

Geyer if the moisture in the atmosphere has any effect. The same question arises in regard to the Cardew volt-meter and I have never heard it answered. On a moist day for example, the wire would lose its heat more quickly than it does on a very dry day. In other words, would not the difference in temperature be greater on a dry day, than on a wet day?

Professor Geyer:—On that point we have not made any experiments. I should judge from general principles, as it is simply the difference of temperature between the two bars that the case would be analogous to the case that I mentioned before, of putting the instrument into the warm room and then into the cold room; the moisture in the atmosphere if it has any effect, being simply equivalent to increased heat or increased cold around the instrument. I should imagine the results would be negative.

Mr. Carl Hering:—I would like to ask whether it would make any difference with this instrument whether the current were direct or alternating.

Professor Geyer;—It would make no difference whether the current were direct or alternating. What we measure is the heating effect of the current. We say an alternating current is equal to a continuous current for all practical purposes when it produces the same heating effect.

In answer to a question by Dr. Vander Weyde, Professor Geyer said:

The instrument, as shown here, is intended only as an ammeter, although we believe a volt-meter can be made. In this case it is necessary to introduce very high resistance, and we think we can accomplish this by using a very fine wire, bent backward and forward a great number of times, and then lay that as a flat disc upon the other strip. I found the latter would be heated by its proximity to the heated wire, but there would be no difficulty of course in passing the current through this strip in addition to passing it through the wire.

Dr. Vander Weyde:—The objection to using the principle of expansion by heat as a volt-meter is that currents of a very small number of amperes have no heating effect.

Professor Geyer: The question as to how many volts a Cardew volt-meter will indicate is to my mind simply a question of proper adjustment of parts. Thus, for instance, at the Stevens Institute we had occasion last summer to measure directly the electromotive force of a Westinghouse alternating machine.

We wanted to measure, if possible, directly; without any multiplying devices; say from 10 to 500 volts, we therefore constructed a volt-meter sufficiently long—in fact it was two stories high, and we had no difficulty whatever in getting very large deflections for 10 volts—that is as low as we cared to go, and we made our pointer go around nine times for 500 volts. To make no mistake in counting the nine turns we had a secondary dial, which as on a common clock rotated one division for each revolution of the other dial.

Mr.. C. O. Mailloux:—I would like to ask Professor Geyer what is the rule, or law, or function, of the deflection of the strip with the rate of current.

Professor Geyer:—In some respects it may be unfortunate that the deflections are not proportional to the currents. It would hardly be expected. As we all know the heating effect varies as the square of the current, so if I double my current I have four times the heat produced, but then the bars will lose their heat much more rapidly when they are at a higher temperature. The deflections are not then as the square of the current, but as some intermediate function. I have plotted out some curves, and if I had thought of it, I might have drawn them on the board. I have not a curve here, but I might represent it, making a guess at it. Let the abscissæ (referring to a sketch, Fig. 4) represent amperes, and let the elevation represent deflec-

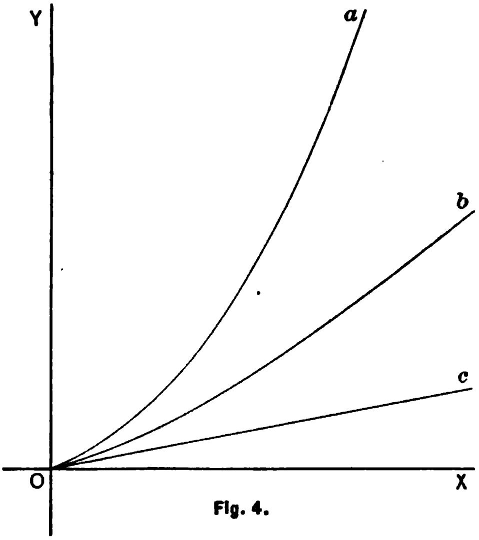


Fig. 4.

tions. If there were no increased loss of heat due to increased radiation from increased temperature, then of course our curve would be one rising quite steeply up that way as O *a*, (illustrating), and of course if the deflections are proportional to the current; our curve would be a straight line as O *c*. As a matter of fact it is a curve between this straight line and this curve which rises as steeply as the amount of heat due to the C^2 R law would require, and is represented in the figure by O *b*.

In reply to a question put by Mr. Wolcott, Professor Geyer continued:

The circles drawn on this card are ampere circles. Gentlemen can see from noting the relative distance of these concentric circles what the rate of increase is for increased current. You will at once notice at the outside of the disc the distance between two concentric curves is greater there, that the instrument grows more and more sensitive as the amount of current on it is increased.

Mr. Crooker:—I would like to ask Prof. Geyer one question, which I think has an important bearing on all meters, that is the amount of current consumed compared with the amount of current that is measured—that is to say the efficiency of the meter. I think these meters working by the heat produced—for example Professor Forbes's meter, and to a certain extent the Cardew volt-meter consume rather a large quantity of current in proportion to the current that they measure. For example, when ten lamps are in circuit, using, say ten amperes—I would like to ask Prof. Geyer what the drop over the meter would be in volts.

Professor Geyer:—I agree with the gentleman that the question of resistance is a very important one indeed, and we have aimed to keep the resistance of the ammeter low. The resistance of the bars which are there exhibited, is about .06 of an ohm. We believe it could be brought much lower, possibly by selecting other metals. Probably the length of the bar might also be much shortened. When I tell you the resistance of the instrument is so much, you can calculate at once for any given current what the loss of energy is in the meter, compared with the consumption of energy in the lamps.

Dr. Wheeler:—What is the ampere capacity of that .06 ohm strip?

Professor Geyer:—That strip will carry currents up to 20 amperes and possibly greater. We have not urged it beyond that point.

Dr. Vander Weyde:—You can make them to any capacity, of course.

Professor Geyer:—They can be made to any capacity by making the cross section of the pieces greater.

Dr. Vander Weyde:—I confess that the low resistance is a great advantage in your arrangement.

Professor Geyer:—In the Cardew instrument of course great resistance is not an objection. It has always, to my mind, been one objection to the Forbes meter, that it consumes relatively a large amount of energy. He uses a very small proportion of the heat produced to operate a little wind-mill. We might be said to use the whole heat produced.

Mr. Townsend Wolcott:—With regard to the voltmeter which was drawn on the board, the reason I asked the question was because it would require considerable length of wire, and I should judge the self-induction would be very high. That seems to be the trouble with using a straight wire.

Professor Geyer:—I think if the wire was laid zigzag the self induction would for that very cause be eliminated.

The Chairman:—Is there any further discussion, gentlemen? I see we have with us this evening our friend Dr. Moses, and I am sure we should all be pleased to hear from him in regard to this very interesting subject.

Dr. Otto A. Moses:—I regret to say, gentlemen, that I came late this evening. The subject is a very interesting one. There are one or two questions that may be a little at random, but they are simply through ignorance, not having heard the earlier part of Professor Geyer's lecture. Is there any superior advantage in having different radiating areas of the same metal over different radiating areas of different metals?

Professor Geyer:—The great point is that we have two metals whose coefficients of expansion are equal, so that simple atmospheric changes shall not cause the bar to deflect. If then you select two different metals, which, however, have the same coefficient of expansion, it will not alter the action of the instrument. The object of getting more radiating surface is to get a greater difference of temperature between the one conductor and the other conductor.

Dr. Moses:—One farther question. Do you believe it is possible to keep the surface at the same equal degree of polish, and in that way to be able to get equal ratios of radiation? Take german silver; the oxidation that will take place upon it from the increased radiating area of one side may in that way affect its accuracy after a while, may it not?

Professor Geyer:—I think it would affect the rate of variation to a slight extent. We propose to use such a cross-section that the wire shall never lose its polish from simple heat generated in it. It would thus only lose its degree of polish gradually if subjected to severe treatment from without, say strong acid fumes. Ordinary atmospheric changes do not seriously affect german silver.

Dr. Moses:—In the case of alternating currents, have you considered the electric density, so to say, of the conducting body in relation to the differences of areas. That is a question that has recently been brought forward. Have you had time since its suggestion to look into the matter?

Professor Geyer:—I must confess my ignorance and ask the Doctor to explain what he means by electric density. Then perhaps I shall be able to answer.

Dr. Moses:—I mean by that, the fact that in conductors of different areas of cross-section there have been found to be certain parts, that, by counter electromotive force set up within the body of the conductor itself, have been found to be like the central part of rods in mechanical strains, they do not have the same ratio of conductivity owing to the sudden alterations of the current, the current not seeming to be able to permeate as it were to the centre. This difference in area might in some way or other affect the amount of current that would pass over the conductor wire and surface and in that way somewhat effect the heat radiation.

Professor Geyer:—I thank the Doctor for his clear explanation. I can only answer it as far as this—that I have lit up lamps with a continuous current and noted the deflection of the instrument, then heated them up to the same degree of brightness as measured on the photometer and noticed the deflection when they were lighted by an alternating current of 16,000 alternations per minute and could notice no difference in deflections.

The Chairman:—The Chair hears no further remarks and we will therefore proceed to the next subject.

We are fortunate in having with us this evening a distinguished gentleman from abroad, a prominent mathematician, a well known contributor to that peerless European technical journal *La Lumière Electrique*, and a gentleman who has an important official connection with the electrical department of the forthcoming Paris exhibition. He will exhibit to us one of his own many inventions, an invention that I know will engage the breathless attention of a New York audience because I may call it in the vernacular an apparatus for dispensing with "cranks."

I have the pleasure of introducing to you Mr. Abdank of Paris.

Mr. Abdank :—Mr. Chairman and gentlemen ; before I begin I must crave an indulgence. I am only a beginner in knowledge of the English language, and my vocabulary has only a few words and it is the first time that I have spoken in public in your language.

Mr. Abdank then read the following paper :

THE ABDANK MAGNETIC CALL, AND THE ABDANK INTEGRAPH.

BY B. ABDANK-ABAKANOWICZ.

Permit me to speak first of a small improvement in magnetic calls. The instrument that you see here, figures 1 and 2, was constructed as long ago as 1882, when calls for telephones were wanted everywhere. The principle of this call is extremely simple. For producing electromotive force we are obliged to move a coil through a magnetic field. I fix my coil at the end of a straight spring, the other end of which is solidly held by the support. On the same support are fixed two magnets that create the magnetic field. If I move the bobbin from its neutral position, thus bending the spring, I am storing up the muscular energy of my hand in the spring. If I remove my hand this energy is restored in a series of oscillations of the bobbin through the lines of force. In this way an oscillatory current is produced in the closed conductor, and the mechanical energy reappears on

THE ABDANK MAGNETIC CALL; FIG 1.

the other side in the form of the movements of the bell striker. I have made similar calls of different sizes, beginning with half the size of this one (about four inches), up to others where the diameter of the bell was three or four feet. As the electromotive force developed is very high the working distance is very great. This small one can ring a bell to a distance of over 100 miles over an ordinary telegraph wire. We have tried successfully larger calls on lines over 600 miles.

The apparatus is an old one, and I give the short description of it only as an introduction to some theoretical remarks on its working. In this country, calls approaching to mine, based on the synchronous movement, were patented by Andrews and Watson. Mr. E. Meylen has lately made measurements in my laboratory, for the purpose of finding the curve of electromotive forces produced by the passage of the coil across the magnetic field, and this is the subject of my communication to-night.

If we admit that the instrument is symmetrically constructed, and that the magnetic fields are equal in strength, then the curve of induced electromotive forces must have a regular shape. We neglect also the retarding influence of eddy currents induced in the iron core.

We must have the greatest electromotive force in the middle, where the ratio of variation of the quantity of lines of force is the greatest.

The curve obtained by direct measurement was very different in shape, being as shown in figure 2.

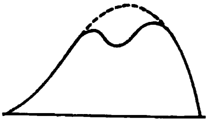


FIG 2.

This curve presents two remarkable features. First, we see a depression in the middle, where, theoretically, the electromotive force ought to be a maximum. The reason of this is to be found in the defective form of the iron core in the armature. The diagram figure 3 and the curves in figure 4 will clearly explain this.

This defect in the construction has a very bad influence on the the result, the greatest and most useful electromotive force being lost, as shown in the dotted line, figure 2, and the same defect was often found by me in different alternating current dynamos.

The second characteristic feature of the curve is in the shifting of its ordinates in the direction of the movement of the armature. Beginning from the starting point the curve is convex, and near to the other end, concave in relation to the middle ordinate.

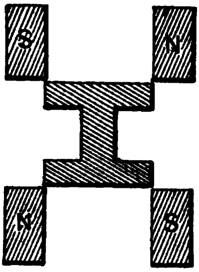


FIG 3.

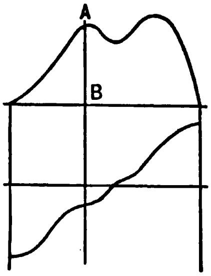


FIG 4.

When the armature moves backward the same deformation was remarked, but in opposite direction.

The electromotive force produced was always somewhat behind the time in which it ought to have been produced. We supposed at first that this was due to the retardation in the indication of the electrometer used for the measurement, but we found by different controlling methods that this was not the case, and that this retardation is probably due to the action of eddy currents induced in the armature core. As you see, the maximum points of the curve are not symmetrically placed. This proves that the magnets did not possess the same strength of magnetic field.

The method used for the measurement of the ordinates of these curves was very simple.

The variation of the magnetic field in the path of the armature was also found experimentally, and then controlled by direct integration of the curve of electromotive force.

The ballistic method was used for the measurement. This method allows of the measurement of the variation of the quantity of lines of force independent of the time. The total variation of the magnetic flux was found to be:

$$\Sigma\, d\, F = 5.1 + 10_7 \text{ (C. G. S.)}$$

The mean current was 0.021 ampere. The mean power was 0.26 watt.

The curve of the variation of the magnetic field can be found directly from the curve of E. M. F., as before alluded to.

Every ordinate of the curve of E. M. F. is proportional to the ratio of the variation of the number of lines of force.

It is equal to the differential coefficient $\frac{d\,F}{d\,x}$

Let us consider the four curves in their relation. Every ordinate, for instance, A, B, figure 4, is proportional to the ratio of variation in the corresponding point of the second curve. This ratio is measured by the ratio $\frac{d\,y}{d\,x}$ where $d\,x$ represents time. The ordinate A, B, in a given scale, is equal to the numerical value of the differential coefficient. Every ordinate of the curve E. M. F. represents a differential coefficient of the corresponding part of the magnetic curve. The magnetic curve is simply the integral of the E. M. F. It is obvious that if the equation of the E. M. F. were given, it would be possible to find the magnetic curve by integrating this equation.

This integration can be performed mechanically by an integrating machine. There is one of these machines constructed in Zurich, by the celebrated constructor, Coradi. To perform the integration it is sufficient to follow with the tracing point the given curve. The integral curve is then mechanically traced by the instrument. (M. Abdank here exhibited his integraph and explained its action.) The integration of differential equations is a problem that we meet continually in the physical sciences. We perform an integration in determining the area of a given figure; also in determining the static moments and the moments of inertia, in calculating the shape of the elastic curve.

The planimeter, as you know, gives mechanically the area and the moments. The instrument that you see before you gives much more. It traces a curve that indicates how the integral increases. The curve is the integral curve, the applications of which are extremely numerous. You have seen one of these applications for the determination of the magnetic curve.

I am glad to have had the opportunity to present it to this electrical society, and, as it were, smuggle into your presence a mathematical instrument under the cover of an electrical application. And I do so because the apparatus interests me personally, being myself the inventor of it.

I must also crave your pardon for having addressed you in

English, of which language I am not at all a complete master, and I am ashamed, because that lack of knowledge is entirely contrary to my principles. I am of the opinion that every electrician ought to be able to speak English. He cannot be a good electrician without being a complete master of that language. Without an intimate acquaintance with the works of Faraday, he is not able to draw conclusions in a simple and logical manner from experiments. He cannot, without being in direct communication with the legion of workers in electricity who speak the English language and who have advanced electricity in this country to a point where it is 50 years ahead of that in Europe, I say, that without knowing it intimately, he cannot keep track of what can be done with that power of nature which we are all attempting to harness.

DISCUSSION.

The Chairman :—I think we will all agree that Mr. Abdank speaks English well enough to have given us a great deal of information in a very brief time. The subject is now open for discussion, and as we have with us a number of distinguished college professors, I would advise that these gentlemen be requested to open the discussion. Will Professor Geyer favor us with any remarks?

Professor Geyer :—I would say that the Chairman has made it difficult for any one to reply, for he has called the gentleman distinguished and it looks very egotistic to get up first.

The Chairman :—The gentleman was called upon by name. He is relieved from the charge of immodesty in that respect.

Professor Geyer :—I would just like to ask one question, in reference to one of the experiments the picture of which is on the middle blackboard, the current being an instantaneous current, whether it was attempted to make any corrections for the self-induction of the galvanometer coil. As I understand it, the current was just a momentary current.

Mr. Abdank :—The current was momentary relative to the period of oscillation of the needle.

Professor Geyer :—What I meant was whether the current really had time to develop its strength as to its electromotive force.

Mr. Abdank :—The difference would be small.

Professor Geyer:—I was more particularly interested in the subject because I had somewhat analogous experiments made under my direction when it was attempted to measure the electromotive force of an alternating machine where the alternations took place at the rate of 16,000 a minute and it was there done by causing the condenser to be charged and then at your pleasure discharging it also through a ballistic galvanometer.

Mr. Wolcott:—Having been for some time interested in the study of integrating machines of various kinds and having invented some myself I can say that I never have seen anything which will approach this instrument which Mr. Abdank has shown us. The ordinary type of integrating machine which Mr. Abdank has spoken of will simply give a reading at the end of a given time—simply a single reading of the integral. We are all familiar, that is all who have given any study to the subject, with the apparatus of Professor James Thomson, Sir William Thomson's brother, which will integrate any expression involving a single variable. This apparatus, or rather a combination of several of them, will also integrate differential equations; but I never heard of any apparatus like this one we have seen to-night, which will trace out an integral curve, the ordinates of which are integrals of the ordinates of the other curve, and I would be very much obliged to Mr. Abdank if he could give us the principle of this machine. Has he any printed description of it?

Mr. Abdank:—I am writing out a description of the apparatus for a paper, and perhaps that will answer the gentleman's purpose. It will be published shortly.

Mr. Carl Hering:—I would like to say, in behalf of Mr. Abdank, that one of the features of that instrument besides tracing the integral curve is that it can be used for solving numerical equations which, I understand, cannot be solved algebraically—equations of a high degree, 4th, 5th and 6th degree. The instrument will trace out a curve, the dimensions of which will give the values of equations of high degrees, 2d, 3d, 4th, 5th, 6th, and so on.

Mr. Wolcott:—Does that give all the real roots in one curve?

Mr. Abdank:—Yes.

Professor Mayer:—If the machine will do that it is a marvellous production of ingenuity and science. Charles Babbage, of England, gave his whole life to making a calculating engine. After he had perfected his difference engine, and the British government would not supply him with means of bringing it out, he invented an analytical engine, of which you will find a description by the only daughter of Lord Byron, Lady Lovelace, which machine did just what this does. The construction of it would be so difficult that Babbage had not the means of bringing it out. If a machine so simple in its construction will do that, I can see that it is the most marvellous production of this age. I would like very much to understand it. Of course I only see it there, and I know nothing of the principle of it.

The Chairman:—I understand, Mr. Abdank, to put it in plain language, for instance in solving any equation of the second degree, the instrument would describe a conic section, and so on, the cissoids and higher curves according to the nature of the equation.

Mr. Abdank:—Yes. (The speaker put an equation on the black-board). We have first the equation of the first degree which represents a straight line. Then I perform successive integrations (integration with the instrument). The first curve would be a parabola, and repeating that several times I arrive at a curve that will represent the first equation, and then I have all that I want by the intersection of that curve with the axis of abscissa. I have here in my book the solution of one of these equations and the solution of the equation of 3d degree only. (The speaker copied upon the black-board the equation from his book). With respect to the calculating machine of General Babbage—I know that machine very well. My instrument acts in a different way. The machine of General Babbage is only an arithmetical machine. It has nothing to do with differentials.

Mr. Wolcott:—There is one more question I would like to ask Mr. Abdank. As has been said there are plenty of machines for performing integrations with respect to a single variable. So far as I know there has never been a double integration; that is one integration with respect to two independent variables, performed by mechanical means. I do not see how it is possible.

Mr. Abdank:—It is possible in special cases.

The Chairman:—Is there any farther discussion? If not we will proceed to the next paper on the programme for which we are also indebted to Mr. Abdank, but a description of it will be given by our fellow member Mr. Carl Hering, of Philadelphia. As I understand it, it is a combined electrical and musical stenographic recorder and type-writer. (Laughter.)

Mr. Hering:—Mr. Abdank thinks he does not speak English well enough to describe this apparatus. I believe you will all agree with me that he speaks English very well and it would be much better for him to describe it. The apparatus I will describe are the melograph and the melotrope. The melograph is an apparatus for recording what has been played on the piano, and the melotrope is an instrument for reproducing this music from the record made by the melograph. The melograph consists of a system of contact points or keys which are fastened under the key board of a piano and are so arranged that when a key is depressed, the contact is closed; these keys are then connected to an instrument like the ordinary Morse ink recorder, so that when each key is depressed a mark will be made on a strip of paper corresponding in position to the position of the note on the piano, and as each note is represented by a key and by corresponding recording apparatus each note will, on being played, be recorded on a strip of white paper like this, in the form of a dark line, which perhaps you can see. This paper is therefore a record of the notes that have been played by the player. It not only records the notes that were played, but the length of the line records the time during which that note has been held, so that it is not only a record of the note itself, but of the time of the note. This record is then passed through an intermediate apparatus called a perforator, the object of which is to perforate a piece of stiff paper with rectangular perforations corresponding to these lines of the melograph record. This is done by means of an electric apparatus which consists of a little square punch which travels up and down very rapidly. It is driven by an electric motor. I understand the lines on this paper are first run over with a puncturing pin. Then this paper is passed over a series of contacts. Wherever the paper is punctured, connection will be made, which connection will work this little punch at a place corresponding exactly to the place of this line on this record. In other words, this record on stiff paper is an exact counterpart of the other only that it is perforated with smooth rectangular holes, whereas this is originally merely a written record. This record is then ready for the melotrope—the reproducing apparatus. This melotrope is merely mechanical in its action. It would be rather difficult to explain it without models, but I will try to give a short description of the essential parts of it. The idea of the inventor in designing the reproducing apparatus was to imitate as nearly as possible the motion of the finger. In ordinary mechanical piano players, the keys are merely depressed by a force which is constant, and in short, the whole apparatus is very mechanical in its action; but in this case the inventor has tried to imitate the motion of the finger; in other words he has tried to depress the keys softly, gently or with considerable force, to hold them down a longer or shorter time. Now this is accomplished in the melotrope, which is the apparatus Mr. Abdank will exhibit, by means of a long roller which extends over the length of the key board to be played upon. This roller has cut into it a number of grooves, the sides of which are slightly inclined. In this case the instrument is arranged for three octaves. There are 37 notes to be played. There are therefore 37 of these grooves. This roller (exhibiting a model) is being turned constantly in one direction and when that string is tightened it will pull this finger down. This finger rests on the keys, so that whenever this string is tightened this finger will be pressed down. The tightening of this string is performed by a mechanism which I will not attempt to explain now. It will suffice here to say that it is a little wedge which is run into this groove. The little wedge is actuated by levers which run over the paper. Wherever they come opposite to a hole, this little lever passes through the hole. In passing through the hole that end of the lever makes a motion which is transmitted to this little wedge. The wedge is pressed into this groove. That tightens the spring and pulls down the hammer. So that wherever there is a hole in this record the corresponding wedge will be pushed into the corresponding groove which will tighten that string and will press down the key and will hold the key down as long as there is the hole in this paper. I am told by a pianist that the pressure exercised by the finger in playing is sometimes as much as five pounds, so that these little fingers must come down each with a force of five pounds, and I understand that they can come down with greater force than that, because this apparatus can play even louder than a person can play. I might add that the apparatus does not automatically produce *piano* and *fortissimo* effects. Wherever *piano* should be played and wherever *fortissimo* should be played, it is written on the music, and the effects of *piano* and *fortissimo* are produced by a little lever on the side of the instrument. This little lever does nothing more than run these wedges toward the roller or from it.

If it moves them toward the roller, it is evident that the force with which this would be moved down would be much greater. If they are pulled away, the force with which this comes down is much less. This apparatus is only a model so that you will notice the want of a full bass. It can just as well be constructed for seven octaves.

DR. VANDER WEYDE :—How does the electricity act on that written paper ?

MR. HERING :—It is punctured with a puncturing pin. It is necessary to go over this by hand.

DR. VANDER WEYDE :—It is just as easy to punch those holes mechanically as to puncture them by hand.

MR. HERING :—It is necessary to go over this by hand for this reason : Every player, however expert, will touch notes that he ought not to touch. They will all be recorded by this apparatus and it is necessary to have some one go over and erase those notes.

DR. VANDER WEYDE :—I beg your pardon, good players will not touch notes which they ought not to touch.

Mr. Abdank then played on his instrument the overture from Carmen and afterwards the Boulanger March.

MR. HERING :—There is something about this which will interest those of you who are musicians; you all understand that the notes are represented by certain positions on this paper. Therefore if you reverse this paper and pass it through the apparatus the wrong way the notes would be entirely different. A note which is three spaces above would then be three spaces below. The effect is rather curious. We will try it first and discuss it afterwards. The piece is the Minuet from Don Juan. It will be played first the right way.

Mr. Abdank then played the Minuet from Don Juan and afterwards played it by reversing the paper.

DR. VANDER WEYDE :—Now play it backwards.

MR. HERING :—It will sound like Chinese music played backwards. As just played—simply reversed—it was in minor, while it originally was in major, but the chords were perfectly correct.

Mr. Abdank then played the piece backwards; he also played a piece improvised especially for the melotrope. The improvisation was written in his labratory, and converted in one hour after he played it into the form ready for the melotrope.

THE CHAIRMAN :—Gentlemen, the subject is now open for discussion.

Dr Moses:—Would Mr. Abdank be so kind as to reverse the last piece? My object in asking is to see whether the reversal of a piece cannot be the most accurate criticism of its correctness. It is a most interesting fact that music reversed in that way will produce a pleasant impression upon the ear. There is an analogous fact which may bring some light to bear upon this point. If you take a piece of paper and write simultaneously with both hands in opposite directions, then reverse the one that looks written backwards and look at both, holding the objects between the eye and the light so that the two writings will seem to be then in the same direction, you will find that they are written by the same hand. Now the reversal of music indicates correctness of the thought and the harmony in the first direction. And it may be possible, in that way to discover an accurate method of criticising the correctness of harmonies, or in other words what should be heard. There is some music which takes a very considerable musical education to appreciate. If that music when reversed becomes harmonious, then it is worth being heard, perhaps.

Mr. Mailloux:—I understand that Mr. Abdank is a very modest person, and he tells us that he is not a performer on the piano, but I am told that he is something of a composer and that he has written pieces of more than passing interest. I should like to have him favor us with some of his own selections.

Mr. Abdank:—It is my namesake—another Abdank who is a composer.

The Chairman:—I am sure we would all be delighted to hear Mr. Abdank, but I think it would be well to defer that pleasure to a future occasion. We still have something to do. We are here on the invitation of the authorities of the college, and Professor Doremus, in whose particular department we are now has thrown open the laboratories to our inspection. Perhaps it might be well for us to avail ourselves of this opportunity and show our appreciation of the kindness extended to us by visiting the laboratories, physical and chemical, before we adjourn finally to-night.

Dr. Wheeler:—I move a vote of thanks of the Institute to Mr. Abdank and of congratulations upon his intelligent English.

Mr. T. C. Martin:—In seconding that motion I should like to say that it was our intention to have relieved Mr. Abdank from the labor to which we have put him at the piano, but we began a little too late, and we found that to apply the electric motor it would involve a reduction of about 20 to 1, the melotrope going at about a rate of 80 or 90 revolutions and the motor at the rate 1,800 or 2,000.

I think also our thanks are due to him not merely for the fact that he has given us such charming music but because also he has performed a feat which I believed to be beyond anybody's capacity. He has actually succeeded in compelling high science professors to discuss a common electric bell. That I believed to be almost beyond the range of possibility. I would like to say that Mr. Abdank has taken great pains in preparing himself for this evening and I can assure you that the work he has undertaken has been gone through by him with the utmost pleasure and I second the vote of thanks to him therefore most heartily.

THE CHAIRMAN :—The Chair thinks that it is eminently proper that a vote of thanks should be extended to our guests, for while it is not the custom to thank our own members for the pleasure and instruction given us—we deem that in a measure a sort of duty on their part if they have leisure and inclination—but for a stranger who comes here I think it is proper that we should thank him in this formal manner.

The motion was carried.

MR. MAILLOUX :—I move that the Institute tender a vote of thanks to General Webb, president of the College of the City of New York, for entertaining the Institute this evening, and also to Professor Doremus.

The motion was seconded.

THE CHAIRMAN :—You have heard the motion. I presume it is hardly necessary for me to put it. The Chair will announce its unanimous passage unless he hears a voice to the contrary. I have an announcement to make, which I make with very great pleasure indeed. We have had, I was going to say, a nibble, but I will say a very large bite at this most interesting subject of meters, and I announce that at our next meeting, which will be held on the 18th of December, and, through the courtesy of General Webb and Professor Compton and Professor Doremus, here at the college where we will have the same facilities that we had to-night for the experimental illustration of the subject, a paper will be read by Mr. W. J. Jenks, a member of the Institute, and present director of the Edison Standardizing Bureau. His subject will be "Six Years Practical Experience with the Edison Chemical Meter."

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. VI. New York, February, 1889. No. 2.

SPECIAL MEETING.

The twenty-ninth meeting of the Institute was held at the College of the City of New York, 17 Lexington Avenue, on Tuesday, December 18th, 1888. The meeting was called to order by the Secretary, who said :—

GENTLEMEN :—The subject before you this evening is a paper by Mr. W. J. Jenks, Member, Director of the Edison Standardizing Bureau, on "Six Years' Practical Experience with the Edison Chemical Meter." Before proceeding with the business of the evening, I suggest that you name a Chairman, to serve during the proceedings. I am ready to receive nominations.

On motion of Mr. Howell, Mr. George M. Phelps was chosen Chairman.

Mr. Jenks then read the following paper :

SIX YEARS' PRACTICAL EXPERIENCE WITH THE EDISON CHEMICAL METER.

BY W. J. JENKS.

"Measure for measure" has ever been the underlying principle of the trade of the world. The mess of pottage for which Esau bartered his birthright was as truly in his eyes a recompense for the distinction and the patrimony with which he parted, as the property and the necessaries of life which we acquire are in our view an equivalent for the gold and silver which we pass from hand to hand in every-day exchange.

If we start out in life with the notion, so fondly cherished some time or other by every human heart, of getting something for nothing, we shall speedily realize the truth of what somebody has happily expressed in verse:—

The motto of the world is "give and take,"
  It gives you favors, out of sheer good will;
But unless speedy recompense you make,
  You'll find yourself presented with its bill.

We are not in business matters long contented with any system of guess-work as to what the amount of this bill ought to be. Where values cannot be measured, we demand averages based upon long experience. Where it is possible to measure goods delivered, the ingenuity of man is untiring until some means is found adapted to the uses of all the traders of the world. It is only so far as we can draw from nature's limitless supply of necessaries and blessings "without money and without price" that we fail to find in these days a meter check upon our consumption. As long as people live in civilized communities, water and artificial light will represent somebody's labor, and as they come to be more and more generally used, they must be more and more accurately measured.

Perhaps it has never occurred to many of us that about the only system of measurement that has ever quite satisfied mankind is the method or device by which we reckon the passage of time, which doesn't cost anything. We look with suspicion on the scales of the butcher, and we don't believe the milkman's quart is more than two thirds as large as it was years ago, though the price has advanced several per cent. When we buy dry goods we speculate on whether it isn't best to redetermine the length of a pendulum that will beat seconds at the level of the sea, or establish a new yard on the metric basis, after we measure the distance from the equator to the poles again. We know by the ticking of the water-meter that it is away off any standard, and last and oftenest we anathemize the gas meter and the man that reads it as being alike unsanctified.

Now the electric current meter is a baby yet, but it is very likely to be considered by the great majority of mankind as a direct descendant of the gas meter—"a chip of the old block." Unfortunately we cannot record directly the light or power really delivered from any source, and an approximation to such a record through the measurement of the quantity of energy supplied, is our only practical alternative. The method which we shall examine to-night, is thus far the only commercially successful means of measuring the energy delivered to electric lamps or motors. It is doubtless the first-born of a large family which will share in the stigma which the tribe of meters of all kinds have always borne. To show how far the stigma is in this case undeserved, and how much more accurately we can measure the mysterious intangible something, which we only know as a manifestation of energy and which we call electricity, than we can the palpable forms of matter which are apparent to all the senses, is the purpose of this paper.

In the approximation which we make to the measurement of the light, heat or power secured from gas, we have three variables: quality of gas, rate of flow, and form of burner or method of consumption. In electrical work we eliminate at once the first and perhaps the most uncertain of these variable factors, for there is no difference, so far as we can discover, in the quality or commercial value of electrical energy from different sources, unless we change the method or the rate of its delivery (as, for instance, send it out in intermittent or alternating impulses).

So in order to arrive at a price at which we may profitably sell light or power, we must know the electrical horse-power demanded by the translating device for a given result, that is the efficiency of the lamp or motor and the energy actually delivered, during the time of consumption. The work done or heat generated, which bears a definite relation (in a given type of lamp or motor) to the light or power produced, is expressed in three factors—pressure, current flow, and time, and the product of these joules or units of work accomplished in a given period, is what we desire to measure. In the Edison system the light of the lamp and the speed of the motor are based upon the supply at their terminals of a constant pressure, and as the Edison meter is in its relation to the resistance of the circuit practically at the lamp or the motor, it may for all commercial purposes be regarded as always acted upon by the constant standard of E. M. F. applied to the device which transforms the electricity to the useful energy of light or motion. Hence we really make this a joule meter, even while we drop the pressure which is a constant, and make it a measurer of current and time, or a coulomb meter. Its construction is based upon the fact that a given ampere flow will deposit a given weight of metal per second, and so knowing the weight of zinc deposited on a plate, it is easy to calculate the number of ampere seconds. In practice we take the hourly deposit (1224 milligrammes of zinc by one ampere), and knowing the fraction of an ampere required for the standard lamp, we can readily arrive at the lamp hours for which the customer should be charged. It is now becoming common to charge so much for an ampere hour, and sometimes the price is the same whether supplied to lamps or motors, which are thus often included within the registration of one meter.

It is in a comprehensive system of house-to-house supply, where every unit of light or power is made separately controllable, that the necessity for such a meter appears most vital.

The records of the patent offices of almost all the civilized countries of the world bear witness to the fact that the method of connecting lamps, motors or similar translating devices in multiple arc (the only commercial method of attaining this individual current control of current actuated by safe potentials) was original with Mr. Edison. But beyond this there is no question that we owe to him the first comprehensive conception of that form of multiple arc distribution which by combining a low resistance armature, a feeder system of transmission, and a high resistance lamp and motor, has made it possible for us to secure at all lamps and motors a marvellously close approximation to uniformity of pressure, and the expenditure of the largest economical percentage of the initial pressure in overcoming the resistance of the carbon filament, or the counter electromotive force of the motor armature, and thereby producing the greatest amount of useful work.

To Mr. Edison's view each detail of such a complete system appeared full of importance, and so we find him, almost before he had a commercial lamp, working on a meter by which each customer's consumption could be accurately determined, and which could be placed on his premises and inspected at such intervals as experience had shown were reasonable in the supply of the other measurable quantities, gas and water. He foresaw that it must not only be accurate, but cheap and durable. In his study of the subject he applied several principles of motion and registration, tried a great number of experiments with each, and secured several patents. Among them are the following:

Fundamental electric motor meter.—Patent 242,901. Application filed March 3d, 1881. Fig. 1 shows an old example of a large class of meter inventions in which some kind of an electric motor driven either by part or all of the current to be measured, is employed to overcome a fluid friction, but the claims cover broadly the combination of a circuit, motor, fan, or other definite loading and registering apparatus.

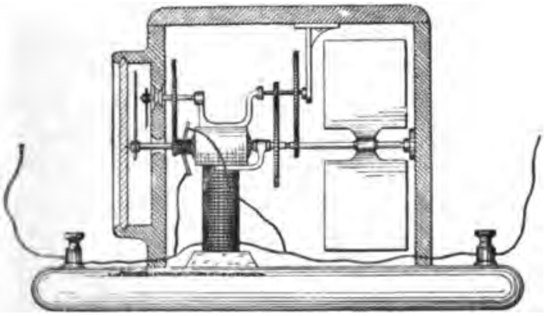


Fig. 1.

Fig. 2 shows another form of electric motor meter. Patent No. 370,123. Applied for April 17th, 1883. The form of motor used is a development of "Sturgeon's wheel," the wheel being transformed into a cylinder surrounding one pole of the magnet, itself being surrounded by the other pole. In this meter the indefinite friction is reduced to a very small factor, brushes are replaced by mercury contacts, and a very compact and simple form is possible. Probably if mercury did not have such a chronic inability to behave itself in practical continuous work, and if it really possessed the ideal character of a liquid which it commonly gets credit for, we should have seen this meter put into practical use by

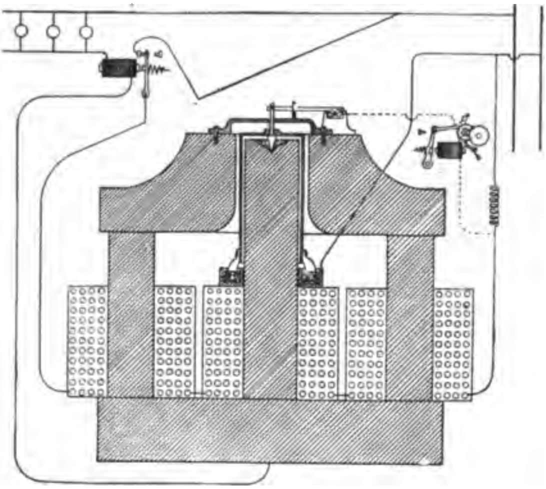


Fig. 2.

some one of the many inventors who have given it attention. This patent covers, among other things, the placing of the inductive portion in the direct circuit and the fields in multiple arc therewith. A magnet in the main circuit closes the field circuit whenever the first lamp is attached, and stops the motor whenever the current flow is arrested.

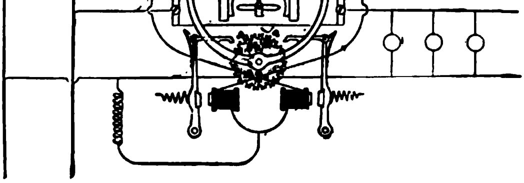


Fig. 3.

Galvanometer recorder.—Patent 307,030. Application filed Oct. 10th, 1883. Fig. 3. This covers broadly a multiple arc circuit, a galvanometer in the main line, a circuit controlled by the needle, electrically operated apparatus in this circuit, and indicating or registering devices. Preference is given to a stylus recorder, the diagram made to be measured by a planimeter.

Recording electro-mechanical meter.—No. 293,435. Applied for August 14th, 1882. Fig. 4 shows a pivoted beam oscillated by electro-magnetic coils in the main or a shunt circuit, the rapidity of motion (regulated by air dash pots of large surface) being practically proportional to the strength of the current. The recording mechanism actuated by a local circuit operated by mercury contacts.

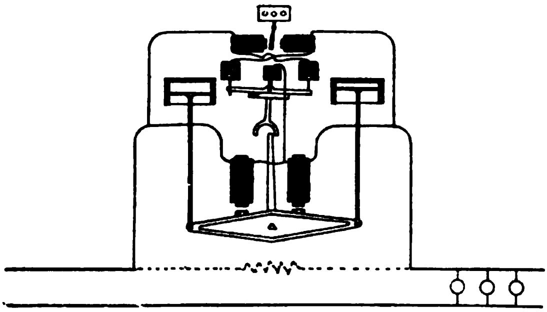


Fig. 4.

Fundamental electrolytic meter patent.—No. 251,545. Application filed March 20th, 1880. Fig. 5 shows the electrolytic meter according to the first Edison patent. An electro-magnetic cutout is shown and claimed as part of the meter. The principal claim covers the use of an electrolytic cell placed in a shunt circuit, the resistances being so proportioned that a definite fraction of the current passes through the cell.

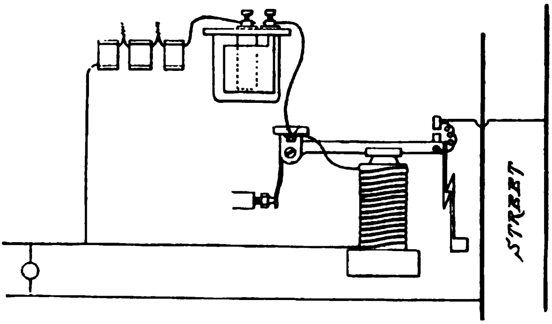


Fig. 5.

Recording electrolytic meter.—No. 304 082. Application filed August 14th, 1882. Fig. 6. At the time of the Paris Exposition of 1881 the Edison meter was exhibited in the form of an automatically recording apparatus, two electrolytic cells being used, one plate of each suspended from the beam of a sensitive balance, the circuits being so arranged that one cell only is in circuit at a time and the direction of current in that cell is such that the electrolytic action will throw the balance out of equilibrium, cause the beam to "kick" and by that action throw the current to the other cell and register one one on the counter. Whether this form was seriously intended for general use may be doubted, but it was very carefully worked up, several different forms patented, and it attracted a great deal of attention and well deserved admiration in its time. Fig. 6 shows plainly the general form and the connections; *h* is the counter operated electrically by the motion of the index of the balance; *b* is a mercury cup; *a*, dash-pot; G and G′ are reversing commutators, operated by hand once a month to keep the transfer of copper from plate to plate from going always in one direction; *i* is a simple electrolytic cell used as a check. This is probably significant of the inventor's lack of faith in mechanical meters, and is particularly interesting to look back upon, in the light of subsequent progress.

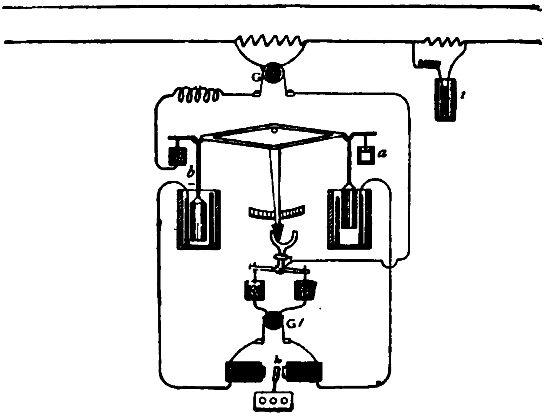


Fig. 6.

(Case 472.)

Revolving recording electrolytic meter.—(Still pending in the patent office.) Fig. 7. shows an interesting modification of an integrating electrolytic meter, in which between two electrodes immersed in the electrolyte is placed a wheel or cylinder of the same metal, free to revolve on its axis. It is apparent that if the wheel was perfectly balanced and delicately poised, the passage of a current would alter the balance, and it would revolve at a speed depending almost entirely on the friction of its bearings. But if it were first caused to revolve at a definite rate for one-half a revolution, the lower limb moving in the same direction as the current, there would be a deposit and loss on opposite halves of the periphery, causing a variation of position of the centre of gravity around the point of support, causing the revolution to continue at the same rate for the same current or at a rate proportional to the current.

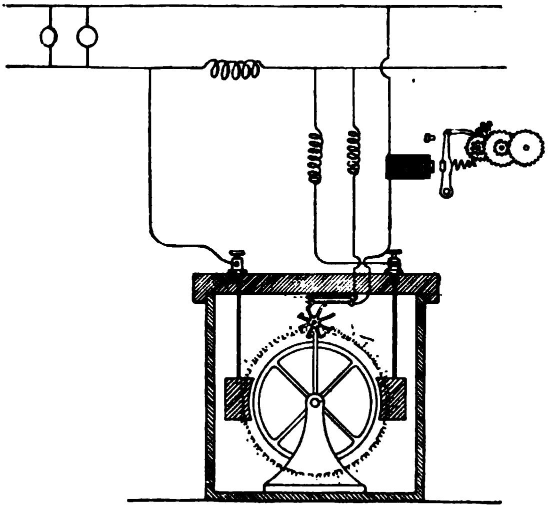


Fig. 7.

The diagram shows a dial scale for reading fractions of a turn and an electric counter recording complete revolutions.

Floating electrolytic cell.—Patent 248,565. Application filed December 15th, 1880. Fig. 8. shows a eudiometer intended to decompose water, collect the mixed gases in a bell-glass, and when a definite quantity is evolved by the raising of the glass automatically cause recomposition (and consequent falling of the glass) by closing a circuit through a platinum coil which becomes heated thereby. This operation is to be repeated continuously while the current is passing, a counter giving a record of the number of charges of gas exploded.

Among the difficulties of using such an apparatus are the comparatively high E. M. F. required, the energy wasted, and possibly like the nitro-glycerine engine, "the necessity of providing a new machine after each explosion." A very similar apparatus has been recently proposed as a laboratory standard for current measurement.

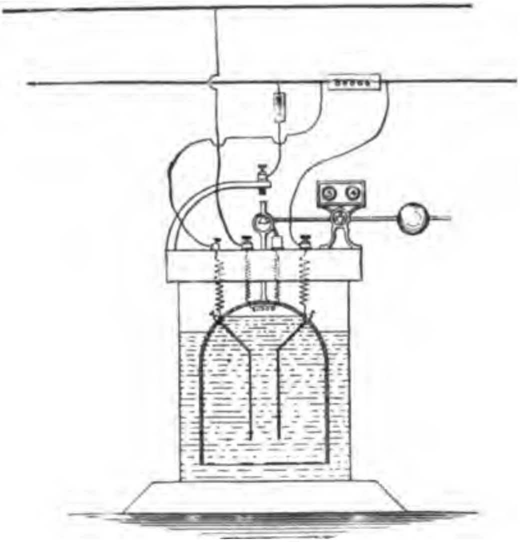


Fig. 8.

Automatic temperature regulator.—No. 251,558. Filed August 30th, 1881. This patent, illustrated in Fig. 9, covers important features of the Edison meter of to-day. Briefly, these are: (1) the resistance E acting as a source of heat to prevent freezing of the liquid in the bottles; (2) the thermostat G completing circuit through E at the proper degree of temperature; (3) distance pieces for holding electrodes a fixed distance apart.

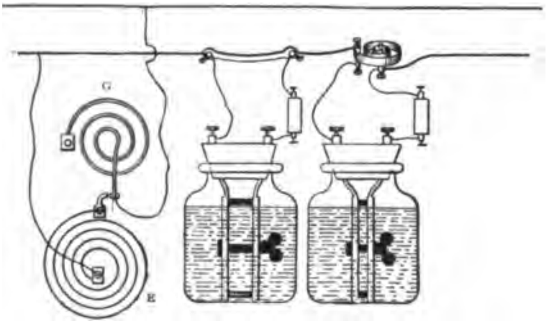


Fig. 9.

Fundamental temperature regulation patent.—No. 265,774. Application filed November 11th, 1881. Fig. 10, illustrates a method of generating heat by the action of a thermostat energizing an electro-magnet controlling a valve, which being opened permits water to flow upon quick lime. This is one of the methods illustrative of the broad idea of "causing a fall in the temperature to set in action agencies for generating heat," and thus maintaining automatic temperature regulation in an electrolytic cell.

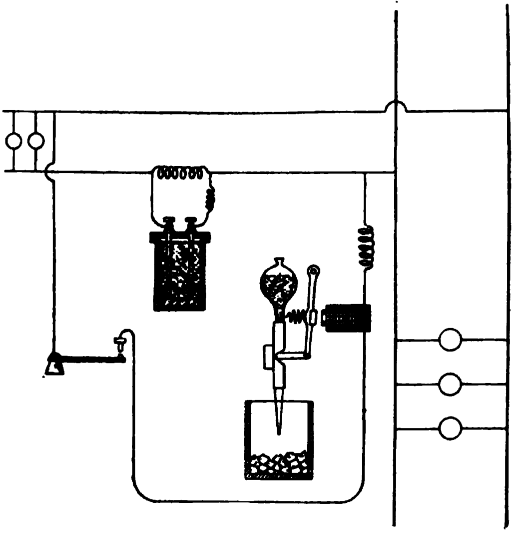


Fig. 10.

Another method here described is the one now practiced, the closing of "a circuit to an electric lamp placed in proximity to the cell." On the same date of this application, Mr Edison filed Patent No. 281,352, describing one of the most vital features of the practical apparatus, namely, amalgamated zinc electrodes in a solution of sulphate of zinc. This overcame the disadvantages of copper plates, which had formerly discouraged the inventor.

Compensating coils in electrolytic meters.—No. 251,557. Applied for May 27th, 1881. Fig. 11, illustrates certain minor claims of details, some of which apply to the Edison meter of to-day, and one feature without which probably no electrolytic meter would be practical, the "compensating spool" having a + temperature coefficient to balance the — coefficient of the bottle resistance. This patent also covers the use of two cells, depositing with unequal rapidity.

The original plan involves, of course, only a two-wire meter. This was first made with a separate resistance for each bottle, one of them being intended to register a month's consumption, the other three months. It was also proposed that the two compartments have separate keys, the inspector of the three month's bottle thus having a private check on the three readings taken in the same time from the other side. It was soon found preferable to weigh both sets of plates together, particularly as inexperience in the manipulation gave rise to errors against which the duplicate records formed a check of great usefulness. This duplication has been found unnecessary in the smaller sizes.

The student of the meter question may find interesting modifications of these general principles in patents 240,678, and others.

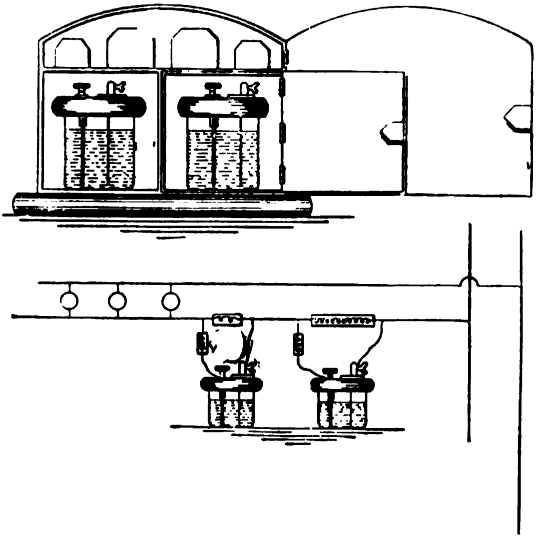


Fig. 11.

These sketches and the patents enumerated convey a faint idea of the months of patient analysis, the multitude of experiments and the scores of models and drawings which may be found described in Mr. Edison's note books, many a page written by his own hand, years before the electrical fraternity conceived of the importance of these devices for which the world now loudly calls. The electrolytic principle was finally determined upon, the details perfected, and more than six years ago the first devices were placed in the offices and stores of the first customers of the Pearl

Street Station in this city, the first station in the world to distribute current for incandescent lamps by a comprehensive system of conductors buried underground like gas pipes. The experiments thus made were so exhaustive and thorough in their character, that the meter then designed is substantially the one in use to-day in numerous stations throughout the United States and several stations in foreign countries. The Edison company has tested every form of direct current meter thus far found in any degree practicable without discussing (as vital) the question of economy either in first cost or operation, and has no knowledge of any other form which has been found to be as accurate under all working conditions, and as reliable when submitted to that tribunal before which so many carefully constructed electrical devices fail—the test of time. The Edison meter is also cheap, but this is of less vital importance. All other forms appear to be commercially impracticable. Some have inherent defects, caused by variations of permanent magnetism. Some are too large to be of any value for commercial use, and they are almost, without exception, too costly and delicate for practical service, or too wasteful of the energy they demand for their operation. We have abundant evidence of the justice of these statements when

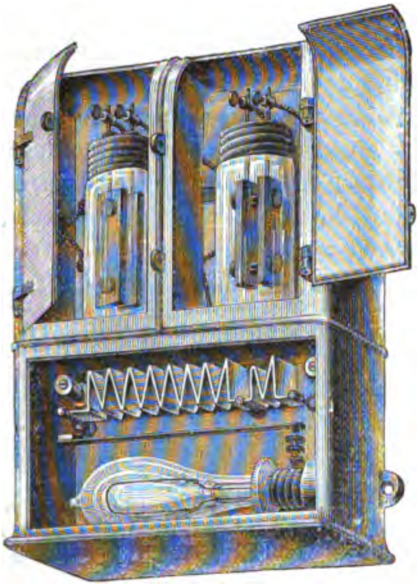


Fig. 12. Old form of two-wire Meter.

we remember that out of the large number thus far proposed, there is not another which has come into anything like extended use. The record which we can quote is therefore the only one from which any conclusions can be drawn as to future practice in this direction.

We shall be better prepared to appreciate the results obtained after glancing at the distinctive features of the meter as at present constructed.

Fig. 12 shows in an iron case, the form of meter placed at the time of starting the First District Station, September 4, 1882. The division in the G. S. shunt resistance for the long and short period bottles has given place to a method of connecting both to the same terminals. The flexible connections have been superseded by the spring clips, and in the three-wire meters two shunt resistances are placed as in Fig. 13.

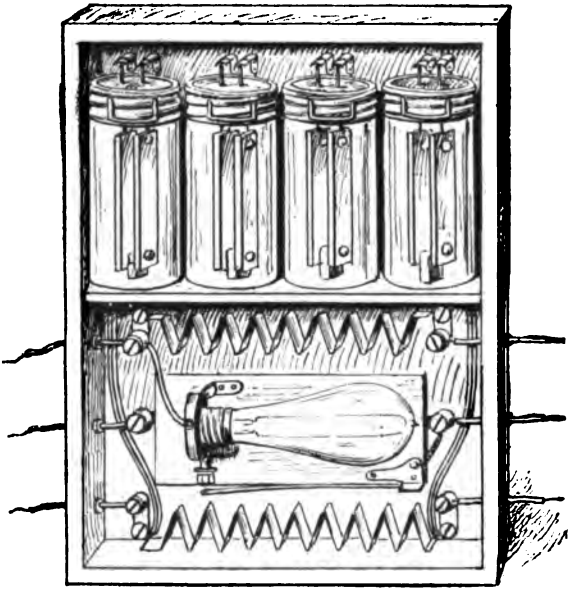


Fig. 13. Three-wire meter open.

Stripped of all complications, the connections of these types are clearly shown in the diagram:—Fig. 14.

The meter case is made of well-seasoned hard wood specially prepared to exclude air and prevent warping, and to maintain high insulation; the door of heavy sheet iron, properly lettered and numbered. This is held closed by a metallic button turning upon a small post, both passing through a vertical opening. The wire of a lead seal is passed through the button.

The conductors enter and leave the meters through holes in the side or bottom, about two or three inches apart.

The thermostat is required for all meters situated in locations where the solution is likely to freeze. It is furnished as an extra attachment and may be inserted in any size meter, and to it is attached a small contact point connected with a lamp socket. Into this socket is screwed a lamp, and when the temperature in the meter falls below a certain point, will cause the thermo strip to curve up, bringing the two contact points together, closing a circuit through the lamp and heating the interior space. As the temperature returns to normal the strip straightens and the lamp is cut out. The adjusting screw in one complete revolution changes the elevation of the contact point one forty-eighth of an inch, and being made with a six-sided head (numbered), one sixth of a turn will change the absolute temperature standard of contact about two degrees Fahrenheit. Thus the adjustment may be made sufficiently close.

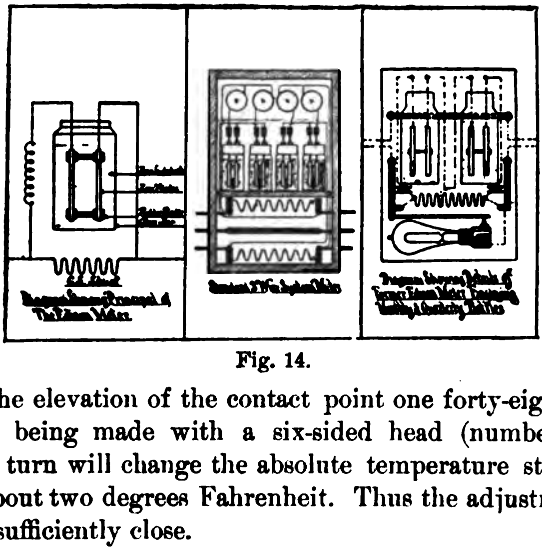


Fig. 14.

The cells are partially filled with a ten per cent. zinc sulphate solution, no special effort being made to render them air-tight, except so far as to prevent evaporation. In these the zinc plates are supported by ebonite distance pieces, screws and nuts, and connected by copper rods with spring clips fixed in the top of the space.

The resistance in the main circuit is of german silver, of the quality used by Elliot in his bridges, and so proportioned as to allow 1/975th of the current to pass through the cell and its compensating spool.

The small portion of current passing through the bottle removes from one plate and deposits upon the other metallic zinc, which when its weight is ascertained determines the current transmitted.

The two-wire meters, authorized to be used with the three-wire system are of two sizes only, five and ten amperes capacity, respectively. Anything larger than five amperes may be preferferably divided to balance the two sides and hence they are made of five amperes each side, capacity 20 of the present 16 c. p. lamps; 10 amperes, 40 lamps; 20 amperes, 80 lamps; 40 amperes, 160 lamps; 80 amperes, 320 lamps.

This will be made more clear by a

TABLE OF SIZE AND CAPACITY OF METERS.

Meter No.	Maximum ampere capacity.	
0	5	2-wire meter for 5 amperes.
½	10	" " " 10 "
1	10	3-wire meter for 5 amp. on each side.
2	20	" " " 10 " "
4	40	" " " 20 " "
8	80	" " " 40 " "
16	160	" " " 80 " "

In selecting the proper size of meters for certain customers it is borne in mind that a meter plate has, like a storage battery, a somewhat definite maximum capacity in ampere-hours per month. The standard found most desirable is 150 milligrammés deposit per month per ampere of nominal capacity.

The normal capability is therefore understood to be restricted to an average work of one or two hours per day at this maximum load, which corresponds with practical work. If steady work is to be done for an average of three to four hours daily, the load should be about 20 per cent. less; if from five to six hours, about 25 per cent. farther reduction should be made to determine the proper limit for a given plate.

The meter is, in practice, placed in all sorts of positions. The general instructions simply provide that it should be kept clean and dry, inside the service fusible cut-out, and easy of access.

The initial preparation of plates consists simply in thorough cleaning, covering the top and about an inch of the rod with a coat of asphalt varnish, amalgamating and drying (three coats when new), weighing and tagging of the positive plate, and placing in the solution.

When removed the plate is re-weighed, and where two bottles are used their weights are compared by the meter man and his record sent to the book-keeper or superintendent.

The loss in milligrammes sustained by the positive plates is then multiplied by the meter constant which gives the bill in dollars and cents. This constant is found as follows:

Let $\$$ equal price of one standard lamp hour.
C equal ampere capacity of standard lamp.

Then current through the bottle equals $\frac{C}{975}$

As 1,224 milligrammes (of zinc), is represented by one ampere-hour, one standard lamp hour is equivalent to $\frac{1{,}224\ C}{975}$ milligrammes, at the price $\$$. Hence the price for current which has removed one milligramme of zinc will be:—

$$\frac{\$}{\frac{1{,}224\ C}{975}} \text{ equals } \frac{\$ \times 975}{1{,}224\ C} \text{ equals constant.}$$

Each consumer has a meter upon his own premises, and his bill is made out and payment required, upon what the meter shows. To measure the current by means of a meter, and to do so with sufficient exactness to support a bill, the payment of which was to be insisted upon, at first seemed to many of the customers of our various companies an impossibility, and they accordingly resorted to various devices for the purpose of themselves testing the accuracy of the measurements. The most noteworthy of these, for the reason that it affords a simple and effective check, was to keep a record of the hours each lamp was in use, and by multiplying this number by the given rate of a sixteen-candle lamp per hour, to determine what the amount of the bill ought to be. There have been many instances where, in order to satisfy customers that the meters were reliable, we have taken their record at the end of a given time, during which the customer has kept an account of his lamp hours, and have presented bills upon what the meters showed, that the customers might check the amount of his bill by this simple rule.

It has been argued that the Edison chemical meter, in its best estate, is open to several very serious objections:—

1. The necessity of the expense attending the removal and replacement of the bottles, usually at monthly intervals, and the complete disconnecting and weighing of their plates. So far from being a detriment, this is seen to be in the light of practice, a positive advantage. A gas meter is adjusted once for all, and once placed is inspected only at long intervals, or when strongly suspected of inaccuracy, while the Edison meter receives thorough inspection and radical readjustment every month. The sources of error lie almost entirely within the bottle, and are thus speedily corrected.

2. The necessity of employing what objectors are pleased to term a "chemist" as a meter man. In the early history of any art, until the conditions of practice become thumb rules, the manipulator of an important device should be a man of intelligence and some originality of ideas. After a time the work becomes simply a matter of routine, and the occasional oversight of the manager or other official will detect any irregularities. Thus it has been with this branch of our work. The most of our meter men are young, and receive only moderate pay, as the statement of cost of operation elsewhere given, conclusively shows. Accuracy and caretaking in matters of detail are the prime requirements. The work has been greatly simplified by arrangements with Mr. Edison, by which he will in future produce at his laboratory electrolytic zinc plates, standard zinc sulphate solution in carboys (or salt, if preferred), distilled mercury and four simple reagents, for the testing of water for solution, by such companies as prefer to prepare it themselves, as a matter of convenience or economy. These reagents are:—

(1) Ammonia water.
(2) Ammonia sulphide.
(3) Nitrate of silver, and
(4) Sulph. cyanide of potash,

a few drops or a small crystal of each, as the case may be, to be added to separate portions of water. Any meter man can thus make the four simple tests, which, by a precipitate or by cloudy coloration, will show the water to be unfit for use.

Ordinarily, ice water is available, and where this fails, a simple apparatus for condensing and distilling, costing $25, is sometimes desirable.

Sources of error.—This brings us to the consideration of the real importance of the sources of error. Nothing that man has made is free from some of these drawbacks to accuracy, but when we except those mechanical imperfections common to all electro-mechanical devices, it is surprising how few appear, which play any important part in the result, and how far this few are neutralized. In order to point the application of the few facts and diagrams to be shown, let us refer briefly to the criticisms made on this much-misrepresented but important friend of the Edison manager.

From the English *Electrician* of December 16, 1887:—

"Edison calculates the total consumption of current by passing a known (or supposed known), fraction of the total current through the meter, and the energy supplied is calculated on the assumption that the difference of potential is constant throughout the circuit. This assumption is, of course, only approximately correct."

From the same journal, December 30, 1887:—

"In the third edition of Sir David Salomons' work on the 'Management of Accumulators,' the author (on page 105), gives the following definition of Edison's meter: 'A thing of the past, depending upon the deposit of some metal.' We are, unfortunately, a little doubtful as to the immediate accuracy of the first part of this description, inasmuch as we are under the impression that this apparatus is still extensively employed by the Edison company of the United States; but in the light of the statements alluded to above, there can be but little doubt that Sir David's description will, sooner or later, be perfectly accurate. As to the 'deposit of some metal,' which appears at present somewhat erratic, we suppose the balance is made up, on the settlement of the account. But if these accounts have any foundation, it is perfectly clear that the error is entirely against the company. As we recently had occasion to remark in our articles on the Brighton installation, the Edison plan, by which a fraction only of the current supplied is measured, can be accepted as little better than a makeshift at the best."

One more quotation (from the *Electrical World*, of a little more than a year ago—Oct. 22, 1887,) of some remarks made at one of the meetings of this Institute. It is especially desirable to bear them in mind, as an illustration of the idea that a little knowledge and a great deal of theory on any subject is a dangerous thing in the light of actual experience:—

"There has been already a great deal of time and a large amount of money spent on electric meters for direct currents, and I do not think we have had any current meters of any value whatever.

"The electro-chemical system, in use by a very prominent company here, was put out with a great deal of confidence in the results to be obtained from it. I think any one looking at the principles involved would condemn it from the start. You are to take a small fraction of the total current, and a very small fraction indeed, and pass it through an electrolytic cell, the character of which varies from time to time, so that the resistance is never fixed, and pass the bulk of the current around a resistance which is practically fixed. The change in the resistance of the metal would be but a small proportion of the change in the resistance of the electrolyte. Now, in such an instance as that, you are measuring a very small fraction of the current passing, and whatever error you have, are multiplying that error by the fraction. If you are measuring 1-1,000 you are multiplying your error by 1,000. Any one who has had much to do with electro-metallurgy knows that the cells are constantly changing; that to keep the deposit regular and uniform, to keep the resistance of the solution uniform, they must be constantly attended to. There is not an electro-plater in the country who does not once in a while go in and stir up his baths. Such an instrument is of no real value. We may, therefore, dismiss those instruments entirely from consideration, I think.

"If any one will take the record of the patent office on electric meters, he will see that there has been no small amount of time and thought and money spent on that question. Practically, we are in the same position we were before. We had nothing except the electro-chemical meter, which, as I say, I consider entirely worthless."

The lack of information on this subject thus illustrated, will be seen more clearly by an analysis of the possible value of the sources of error.

Oxidation.—In the hands of an inexperienced person almost any method or device, good and reliable itself, may be misapplied so as to gain an unenviable reputation. An Edison meter cell may be so manipulated as to measure almost any resistance and vary between wide limits. A few practical rules have, however, been applied, and the bugbear which has so disturbed the visions of those who have looked at the matter from a theoretical standpoint only, has disappeared. In other words, we are able to reduce practically to nothing this error, and it therefore appears in the results only as a slightly disturbing element at the lower end of the deposit curve, and always against the company.

The german-silver shunt.—The extract I have read specifies quite distinctly the slight error introduced by the change of resistance of the german-silver shunt with temperature variations. For each 25 degrees Centigrade or 45 degrees Fahrenheit this variation is but 1 per cent. Within a range of 40 degrees it is 0.0249 per cent. for each degree (Fahrenheit). A shunt having 0.01 ohm. at 60 degrees Fahrenheit would be at -2 degrees Fahrenheit 0.00984 and 100 degrees Fahrenheit 0.01009 ohms. The maximum error which can ever occur is about 2 per cent., due to a change from freezing, to 120 degrees Fahrenheit, the maximum reached in any of the meters at full load. As a matter of practice, meters are so placed as to vary not over 30 or 40 degrees, on the average, either from atmospheric changes or the heating of current.

Counter electromotive force.—Figure 15. It is plain that the change due to a rise of current in the meter here illustrated, from 0 to full load or 20 amperes, is 0.0017 volt. As this counter E. M. F. appears in practice as a factor of resistance, its effect is shown in the current resistance curve.

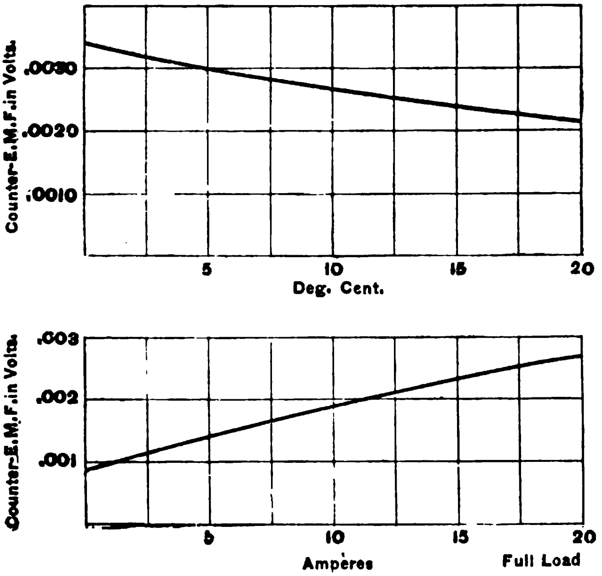


Fig. 15.

It is also clear that the error due to the rise in temperature from 0 degrees to 20 degrees Centigrade is 0.0012 volt. This is one of the factors in the temperature resistance curve, and is there expressed in ohms.

Bottle resistance and current.—The two errors, increase of counter E. M. F. and consequent rise in potential difference, and decrease of absolute resistance and consequent drop in potential difference, jointly resulting from an increase of amperes, about balance each other, forming a beautiful compensation which makes this curve almost a straight line. Fig. 16.

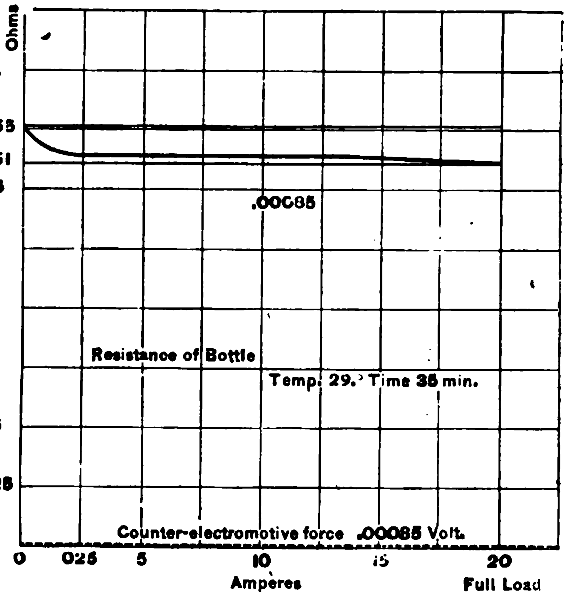


Fig. 16.

Temperature compensation.—Fig. 17. Considering the bottle resistance by itself, the fall in counter E. M. F. and consequent rise in volts with a rise in temperature, adds slightly to the rise in volts attending the fall in resistance. These two factors are together opposed by the influence of the spool, whose copper wire increases in resistance so as to make a perfect compensation at 10° and 30° Centigrade with a slight bend in the curve at intermediate points.

Rate of deposit and current.—Fig. 18. This curve, the result of the combination of the sources of error shown by the others in detail, is so close an approximation to an absolutely straight line that it is difficult to detect any material departure excepting the slight bend at the minimum load. Into the depth and sharpness of this bend the element of time enters, but after passing this point the curve runs almost absolutely straight to a maximum load.

It is especially noticeable that the curve begins at absolute zero, while with Mr. Edison's early meter of the electric motor type, and others that have followed this principle, the theoretical curve is as shown by the dotted lines. No curves deduced as are the above from long practical work have ever, so far as we can learn, been formulated of the performance of any other meter.

It is proper to say in this connection that the percentage of error from these causes is practically constant for all sizes of the Edison meter.

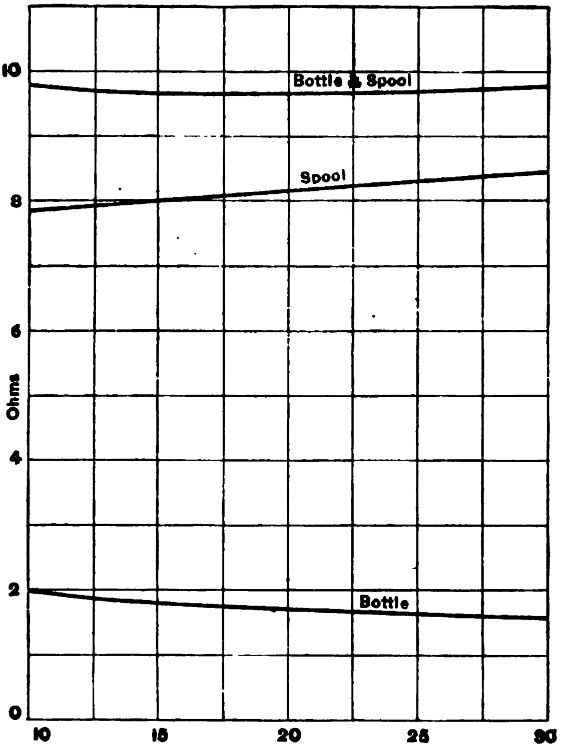


Fig. 17.

Some time ago a very careful test, covering five months, was made by the engineering department. Six Edison meters were connected in series with the meter of the Edison Electric Illuminating Company from the station by which the current is supplied to the offices occupied by the parent company. At the close of this test it was found that all of these meters registered within 1½ per cent. of each other.

An elaborate test was conducted at New Brunswick, N J., by Mr. W. S. Howell, with seven meters put in series with ten lamps. The readings showed a variation of only one and one-half cents in the amount of the bill charged, while four of them were alike to a cent. The advantage of such accuracy is manifest not only to the Illuminating Company, but must also be satisfactory to the most exacting customer. One of our most experienced and successful managers testifies that in the use of about two hundred meters he found the poorest record to be very close, and sums up his experience by declaring that "without the meter they would be swamped."

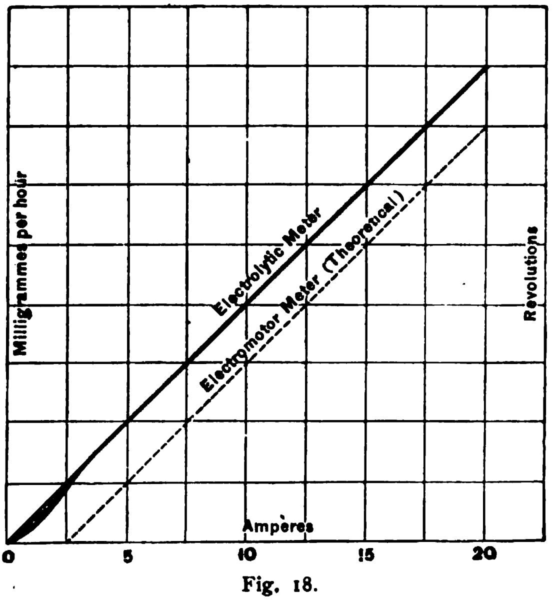


Fig. 18.

But the only real criterion is that of practical success.

In order to discover what the practical men in the business think of the meter, a circular was recently addressed to the 26 stations in the country now operating the meter system, asking for their experience and opinions. To this we have up to this time received 23 replies. That you may judge of the convictions which they express I will read the more important queries, and summarize the answers.

Have you used meters from the starting of your station?

Answers—Yes, 18; No, 5.

From your experience what reasons would you quote for a change from the contract to the meter system?

All replies to this question indicate the unanimity of the opinion that the meter is far preferable. Some of the expressions are as follows:—

(1) "There is no argument about lamp breakage. It is square dealing on both sides."

(2) "They are always superior, even to the best contract."

(3) "Less unnecessary burning; income in more fixed ratio to expenses. Customers are able to regulate their light bills. An accurate basis instead of guess work, to estimate schedule price. Life of lamps should be much longer by use of meter, by the stopping of unnecessary burning."

(4) "We should take in a great deal more money than we do now."

(5) "There are a few exceptional cases where we are getting a little more out of contracts than we would from a meter, but as we run by night only, this difference is made possible. I refer to early closing stores. If we ran by day these would use light by day on contract, while they would rarely do so if the current were measured. I find generally that dwellings on contract are constantly fully illuminated, but the day a meter is put in they find one lamp generally suffices. I believe our income would be slightly larger if everything were on contract, but our expenses would be out of all proportion. Immediately after taking charge of this station, I looked up the last ten-minute record made by outgoing current, dated December 23d and 24th. It is possibly a little unfair to take a day or night so near Christmas, as more light is used at that time than at any other. The nearest aggregate I could get out of 75 tests in money value was, at one cent per hour per ten candle lamp (and the company charged at that time 1¼ cents per hour), and after deducting station and street lighting, $2,868.90, while the income of the company for that month from all sources was about $1,100. Of course during the winter months the contract consumer gets more than he pays for, and is reckless in the summer, because he thinks he gets less than he pays for."

(6) "By all means were I to build and manage a new station I would start it on a meter basis, and carry it as far as possible."

(7) "Our object was to decrease the unpaid consumption and increase the revenue."

(8) "We believe, from our experience, we can supply more on meters with the same power and get a better return from current used."

(9) "The contract gives a regular income to the company throughout the year, nevertheless in a majority of cases it is a system of robbery; electricity should be sold like merchandise—by measurement."

(10) "The principal reason for a change to a meter system was the impossibility of making contracts which were fair to the company. Customers will burn more light than they contract for, and though under a contract system it may be easier to collect bills, at the same time the income per lamp hour will not average much more than half that of the meter system."

(11) "It prevents people from consuming an excess of current, thus unnecessarily overloading the station."

(12) "Under a contract, consumers abuse the use of the light, which abuse, while doing the customer no benefit (the light not being necessarily used), increases the cost of operating and lamp breakage to the company very largely. In other words, you cannot trust customers to cut their own cloth."

(13) "More satisfactory to the consumer if he is economical."

(14) "Maximum revenue for maximum power."

(15) "Consumers can gauge the quantity of light for which they pay, to suit the business requirements."

Question.—"Do you keep meters on contract installations for your own information?" Some of the replies are as follows:—

"We have, and find a contract consumer is using about double what he pays for. Some customers have left us on this account but almost always return."

"Yes; on all motors also."

"We run them in this way at intervals for our own satisfaction."

One of the largest of the stations keeps a meter on each contract motor.

Have you evidence of any serious error aside from accidents? If so, please state explicitly what it is.

Eight stations, embracing the large ones at New Orleans, Rochester, La Crosse, Cincinnati and New York, reply to this question—"No."

One of the most experienced station managers makes this reply. "We have none. In my judgment the greatest amount of complaints come from places where a notable lack of care is shown in properly preparing plates, weighing and handling; also where leaks and grounds are most numerous. The meter has in past years located many faults for me."

Is the feeling of your customers generally one of confidence in the accuracy of the Edison meter?

Most of the stations reply without other remark than emphatically "Yes." Some of the answers are worthy of being quoted literally.

"In some cases our customers keep count of the hours. All such customers say the meters are correct.

"Remarkably few objections by consumers to the results obtained."

"The general feeling is one of confidence."

"The confidence is fully as great as in gas meters; we have no difficulties on this account."

"Complaints are very few from customers."

"The meter to our customers is a blank. While we can persuade them that the meter is correct if properly manipulated, it becomes a matter of confidence in the meter man."

"One month ago one of my customers began keeping a record of the hours his lamps were burning. When his bill was presented it was four cents in his favor; so it is with a great many. The meter has given us good results."

"This question is rather a difficult one to answer. I have no doubt that the majority of our customers, if they were asked the question, would say that they did not believe in a meter. This is not due in a great many cases to any inaccuracy they have ever discovered, but merely to the general idea that meters are not worth anything, judged by the gas and water meter standards. We have had a number of customers test our meters, and in addition to this, have given us strong letters of recommendation, and I have never seen the test where we had to recede from our position that the meter was correct."

"They have as much confidence in the Edison meter as in the gas meter, and probably a little more."

"Those consumers who keep a careful lamp account for a few months have faith in the meter."

From the results of your experience with the meter system do you feel that you can endorse our recommendation to new companies that they adopt it as the most satisfactory method of serving the public?

"Yes. I have already advised several new companies to adopt it. Contracts are bad. They soon get the best of you."

"We believe that it is the only way to sell light. We only have contracts to satisfy certain customers."

"Yes, where practicable, which, however, is not always the case in starting stations in small towns."

"I do not believe in the contract basis at all. I feel that the meter is a good thing, and particularly because my most economical consumers, never raise any questions, and we never have any trouble with them."

"Yes, most decidedly for large stations. The desirability of putting in a meter system depends on the size of the plant. I do not consider that any plant of over 500 lights can afford to get along without it."

"Yes, but would offer contracts as well. Give them what they want, and if one does not prove satisfactory, give them something else."

THE EDISON ELECTRIC ILLUMINATING CO. OF NEW YORK,
16 and 18 Broad Street.
Spencer Trask, President.
John I. Beggs, Vice-President and General Manager.
J. B. Skehan, Secretary and Treasurer.

NEW YORK, December 27th, 1887.

E. H. JOHNSON, ESQ.,
President Edison Electric Light Co.,

Dear Sir—Replying to your inquiry as to the accuracy and degree of reliance placed upon the Edison meter in the commercial transactions between this company and its customers, it affords me pleasure to state that our experience has established confidence in the meter, not only on our part, but also on the part of our customers, in consideration of which I offer you the following facts:

The Pearl street station was started September 4th, 1882, and, with the exception of but two hours during the first year, has continued uninterruptedly to date.

The station is at present supplying current for 15,000 incandescent lamps and 150 h. p. of Sprague electric motors to 647 customers through a like number of meters.

Our bills are paid cheerfully with but an occasional exception, in which instances a verification of the meter is had, and the customer thereby thoroughly convinced of its accuracy.

Repeated tests of the meter, comparative and otherwise, made by ourselves, show a maximum variation of not greater than 2 per cent. and a variation from accuracy of not more than 1 per cent. These tests, combined with the practical results of five years actual use of several hundred meters, have demonstrated conclusively to the company and its customers the efficacy of the apparatus for the purpose intended.

Yours truly,
THE EDISON ELECTRIC ILLUMINATING CO. OF NEW YORK,
By JOHN I. BEGGS,
Vice-President and General Manager.

The following, it is hardly necessary to say, was penned in the bracing air of the Rocky Mountains, and has the ring of Western enterprise:—

"I can endorse the meter for two reasons. It is the friend of the poor man as well as the company for the following reasons: It gets action on the horny-handed son of toil this way. Our contract price for lighting houses for five lights is $4.25 per month or $51.00 a year. Now there are lots of customers who cannot afford to pay that, but can afford to have five lights installed and use the meter, which will probably not average over $2.50 per month or $30.00 for the year. At the same time if they should contract for $4.25 per month, and then run till 12 o'clock for a month, they would consume $13.50 per month (if measured by the meter), but we would only get $4.25. If all did this our load would overrun our capacity, but thanks to the little box—she holds her down. It produces the same effect with business houses. Our contract price with this class is $1.50 per month (all my figures are based on 16-candle lamps). Houses having 25 lights would be $37.50 monthly; by meter we would realize $29.10 average for year, our lighting being 6 till 9. You see we get $29.10 for 3 hours light, while with contract they could burn till 12 for $37.50 or six hours light. The meter holds the load down for families when they use only one or two lights a night, but if contracted they would 'turn her loose, Murphy,' in order to play even. It is our experience that the people always manifest a desire to play even with a light company if possible, and if they can't get action one way they will another.

Our meters here are a great deal of labor, owing to the number we have in use, but still we can't get along without them and really think they are reliable if conditions are equal. My tests here have been very flattering, have had them within 1–10 of one per cent. of bill and the highest out was 2 5–8 per cent. high.

"For customers using a large amount of light it is cheaper for them to contract here, but for small customers the meter is the correct thing, and with us produces satisfaction and good results among the customers. In the summer, bills are small, but the winter months balance them and run higher; when expenses increase, so do the meter bills, but with contracts it would be the same."

The reports referred to, represent the following aggregates and averages:—

Total number of lamps of all powers in 23 stations		117,501
" " " " on meters		87,856
" " " " on contracts		29,305
" " " motors		350
" horse-power of motors		1,000
" number of meters of all capacities		5,187
" " " two-wire meters		4,531
" " " three-wire meters		660
Average number of lamps per station, meters and contracts		5,109
" " " " on meters, 23 stations		3,820
" " " " on contracts, 20 stations		1,465
" horse-power of motors		3
" number of meters per station		226
" " " lamps per meter		17
Total cost of operation, 16 stations	$16,235.00	
Average annual cost of operation per meter	4.03	
" " " " " " lamp	0.22	

It is also of interest to note that of the twenty-three stations equipped with meters for the measurement of seventy-five per cent. of their entire lamp capacity, and relying upon these meters for the amounts of their bills to consumers, four are earning upwards of fifteen per cent. on their capital stock; three others between ten and fifteen per cent.; three others between eight and ten per cent., and eight more between five and eight per cent. on their capital stock. Others have been operating for too short a time to yield definite results.

The following extract from a letter from Mr. J. W. Lieb, Milan, Italy, written under date of November 2d, 1888, demonstrates clearly how satisfactory the action of the meter may be made, even when the urgent cares of a rapidly growing business, forbid the one engineer of the station who is thoroughly informed on this subject, from giving the details of its operation any considerable personal attention. Mr. Lieb has labored under the additional disadvantage of being at a great distance from the birthplace of the system, and the scenes of its widest usefulness.

"We have at present actually in use in Milan some 360 meters of various types (all Edison meters), the smallest for 350 ampere-hours per month, and the largest 30,000 ampere-hours, with a respective maximum capacity of from four to 400 amperes.

"All the current from the mains is paid for by the ampere-hour according to the sliding scale of charges herein enclosed.

"The temperature to which the meters are exposed varies between 5 degrees centigrade and 35 degrees centigrade. We have never had occasion to apply thermostats to the meters. The results of our experiments and our general experience with the meter have given us confidence that, if carefully treated, its indications are fairly reliable, the maximum error (low reading), occurring with light loads.

"While we have some consumers whose consumption of current is comparatively uniform throughout the year, the majority (among whom many apartments) consume little or no current during the summer months, some consumers closing their apartments and making the meters inaccessible for four or five months.

"Our consumers are as a rule very close, a number keeping careful account of their lamp hours (ampere-hours), from month to month, the larger ones having clerks specially delegated for that purpose.

"This fact, coupled with the unfavorable conditions of supply above noted, makes it necessary to apply all precautions to avoid contestations, and I am glad to say we have been fairly successful in meeting the difficulties.

"We keep a careful half-hourly register of the indications of the main station ammeters and the feeder ammeters and find them to agree with each other, and their sums with the ampere-hours given monthly by the meters within a small percentage of error which we have observed is a maximum during the summer months, the sum of the meter indications being slightly low."

Such abuses of the contract system as are shown by the foregoing expressions are so manifest to many contract stations from which we have asked for no letters, that some of them are already changing to a meter basis, and it is evidently only a matter of a short time when others will be compelled to do likewise.

At every meeting of the Association of Edison Illuminating Companies there has been a full and impartial discussion of the relative merits of the two methods. The result may be briefly and forcibly summed up in the following, unanimously passed at the meeting of the Association held at Chicago, Illinois, February 9th, 1888:—

"*Resolved*, That after a full discussion of the relative merits of the meter and contract systems in the numerous meetings of this Association, that it is the sense of the Association that the Edison meter is accurate, that the system is not too expensive for stations above 1,000 lights, and that the best financial results are invariably secured in a station selling by meter."

Recent improvements.—Nearly a year ago a series of exhaustive tests were undertaken at the laboratory of Mr. Edison, to determine whether in the light of years of experience since the initial experiments upon which the first methods were based, advantageous changes could be made.

The first result of these tests was the perfection of the general details of the three-wire meter, then being offered to the Edison stations. The present form of connecting spring clip was at once introduced. A diagonal arrangement of terminals, making it easy to connect from either side or from below, followed after. Then the production of pure chemicals by the Edison laboratory, and the elimination of the error due to oxidation (already referred to) by proper treatment of the solution.

At a meeting of the Standardizing Bureau, held December 17th, 1888, it was voted to authorize the discontinuance of the two largest sizes of plates (40 and 80 amperes), and use in their places the 20 ampere plate with a proper change in the compensating spool in each case. It was found perfectly feasible to measure the shunt resistances and weigh the plates more accurately than heretofore, thereby effecting a substantial saving in cost and care of large plates. As an illustration, a 200-light plate only needs to be weighed to one-sixteenth of the exactness in units of what is necessary in a 12-light plate to secure the same percentage of accuracy.

It was also decided to authorize the use of one bottle in place of two for the three smaller capacities, which, as appears by the average figures given, constitute a large majority of the total number in use. This will probably be followed by a change in all sizes as soon as confidence in the feasibility of this plan becomes general. One-bottle meters for two-wire uses, and two-bottle meters for three-wire, will, therefore, be immediately produced.

These important changes, which it will be observed simply follow as a natural result of the confidence established by experience, will probably be supplemented by others, none of which will, however, modify any essential features of practice, but simply apply processes which add nothing to the expense but tend to eliminate the already small sources of error. Among those indicated as likely to be recommended are a reduction in the size and change in the form of the bottles and method of closing; a slight change in the specific gravity of the solution, and a modification of the treatment of the plates by which the active surfaces shall be limited to those placed directly opposite each other in the cell.

These minor changes will aid still farther toward the result which Mr. A. E. Kennelly (under whose personal supervision the present tests are being made) predicted in a recent lecture would be realized, namely: that "The Edison electrolytic meter will in the future be recognized as the simplest and most accurate apparatus of its kind in commercial use."

An interesting application of this Edison meter principle has been made in England, and is described as follows in the *Electrician* article already quoted, relative to a multiple-series installation at Brighton:—

"The objections to the electrolytic method are absent, when it is employed in connection with a constant current distribution. In the first place, we have now to measure the 'volt-hours' across the terminals of the house connections, so that the meter is a shunt to the whole installation. By placing a high resistance in circuit with the meter in order to reduce the current in the shunt circuit, no error is introduced. Secondly, by integrating the E. M. F. and assuming constant current, we are introducing no non-existent factor; in fact, the current at Brighton is actually maintained constant within one per cent."

This suggests interesting possibilities in measurement of energy supplied to customers of series circuit systems.

I have not taken your time this evening to prove by aggregation of testimony simply that a meter basis is the proper principle, or that the electrical fraternity needs it and should be educated to its use. This is self-evident. My aim is to show by the incontestible logic of facts that it is entirely practical and economical to accomplish all that is required by a method which is widely applied, and to emphasize by varied but singularly harmonious quotations from the experience of men who are by its help, earning dividends, the result of "Six Years' Practical Experience with the Edison Chemical Meter," which is the topic of my paper.

DISCUSSION.

THE CHAIRMAN :—I am sure the Institute is to be congratulated upon the paper presented this evening; and we should be very grateful indeed to Mr. Jenks for the pains he has taken, and the elaborate preparations he has made; and for having given us all a great deal of information, which probably few, if any, of us had, as to the practical experience with these electrolytic meters. We have heard about them for a long time, and probably knew a little about them; but, I think it is the initiation of a good practice for the future that those who actually know about these things should tell us all about them, and inform us just what has been ascertained regarding them in practice. The subject is now open for discussion, and I hope you will all avail yourselves of the opportunity to speak.

MR. JENKS :—I would like to say a word just at this time. It certainly is a good practice to bring people who actually handle devices of this kind, into such a position as will enable them to tell what they know about such devices; and my regret to-night is, that for several years I have not done anything in the way of practically handling the Edison meter. I am not a chemist; and have, in a measure, gotten out of the way of handling or remembering figures and exact quantities, about which you might ask me a great many questions that I should not be able to answer off-hand. I have undertaken to show the practical results attending the operation of the meter, and any question in that line, as well as in others, that I chance to remember, I shall be very glad to answer.

MR. C. S. BRADLEY :—I would like to call Mr. Jenks's attention to the fact that he did not fully point out the smallness of the error due to the comparative resistance of the bottle and spool, compared with the shunt. The resistance of the bottle and copper spool combined being 975 times that of the shunt, there is a very small error, which is not fully explained.

THE SECRETARY :—I feel that I ought to congratulate myself somewhat, on being responsible for bringing up this subject. It was originally my idea, however, to have some gentleman take up the whole meter question, and go through it exhaustively, showing the work of different inventors. When I made this suggestion, I had no idea of the practical perfection to which the Edison meter had been brought; nor of the multitude of devices which the inventor had brought out, before arriving at the perfected form as shown this evening. We have seen, from the various diagrams which have been thrown upon the screen, that he has utilized the various properties of the electric current in many different, and I must say, very ingenious ways. Many of these experimental meters are probably not practical; but I am sure they all tend to show the remarkable fertility of Mr. Edison's inventive mind, and it has been very interesting to me, especially in comparing the experience in electric meters with those used for other purposes, which have had discredit thrown upon them, and I think rather unjustly. I have had some experience with the gas meter and I must say that I have always found it very accurate. I used to read one in the Western Union Building. True it was not used very much in the summer, but it ran on for four or five months, and the bill would come in for perhaps 25 cents. Once in a while the bill would be larger than the average and upon investigation, I would find that some one had been working at night. I have a meter in the Institute office, and frequently use gas on dark days. My bill for six months was $1.47, I do not think that is very exorbitant, and I presume the meter has recorded it very accurately. But there is one important point about this meter. It is going into a certain field where there has been nothing of the kind before, and that is in power service. All power service, as you know, is furnished under contract, so much a year, or so much a month per horse-power; but there is always a question whether a man gets six h. p. or ten h. p., whatever the understanding may be; and they judge by the width and the tightness of the belt, as to how much the customer is taking. Then again a man may pay for six h. p. and not use it at all. In printing offices the presses may be idle all day, but they have to pay for their power all the same. In this field it certainly should prove a very desirable instrument, and relieve the minds of a great many as to the amount of the bills they ought to pay. Then in domestic service, we can all see the benefit of using a meter. For a residence the contract service will not answer at all. So far as I know, the contract price at which incandescent lamps are put out precludes their use in private families, where that light is most desirable.

I could not help thinking, when I saw the pictures thrown upon the screen, as they passed one after the other, what a long road Mr. Edison had travelled over; and how much trouble he might have saved, if he had only permitted himself to be guided by the experience of John Gilpin, who rode through Cheapside "and hung a bottle on each side to make his balance true." Mr. Edison has got his meter down to two bottles and we have been shown to-night that the instrument is true.

Prof. Edward P. Roberts:—I would like, if Mr. Pope would allow me, to take one of his remarks as a text for a moment. That was about the gas meter. We had an electric light company in Cheyenne, and I blackguarded the gas meter with all my power. Then, we bought up the gas company. I had to send bills to the gas people, and the customers remembered it, and it was a difficult thing to get out of. When I went to prove up the meters, only one of them registered in favor of the company, and nearly all the rest were in favor of the consumer, about three per cent., and some, considerably more than that. As the tendency with the electric light companies, is to buy up the gas companies, and not for the gas companies to buy up the electric light companies, it is unadvisable for any man to get into that fix, if he can possibly help it. I would like to ask the lecturer what is the length of time required to weigh the plates and do all the work necessary to arrive at the figures.

Mr. Jenks:—I think, perhaps, a fair approximation to that may be reached by the averages which were given on the number of meters per station, and the average expense. The number of meters per station on an average, in 18 stations being 225; the average expense being $4 per meter per year, it would make a 225 meter station cost $900 a year, for the man and chemicals and everything else required. In other words, a station of that kind would represent just about that amount of running expense after the plant had once been installed. There are several records of one man attending to a great many meters; but I think, perhaps, experience has shown that men vary in their ability to do that sort of work just as they do in the trimming of arc lamps, or anything which requires care, and at the same time, rapid manipulation. I should be very glad to bring out somewhat more definite data in reference to that.

Prof. Roberts:—Who does that work usually, the bookkeeper?

Mr. Jenks:—That depends to a considerable extent on the size of the station. In a very small station, say a 1,000 light station, the superintendent does his own meter work, and can do it without any difficulty. There are two or three superintendents now, some of the oldest men in the Edison business, who, with 2,000 lamps installed, take in the bottles, and replace them themselves. When the station gets a little larger, a boy is employed to do that merely mechanical work. Then the time comes when the meter man, who should be an intelligent person, does simply the weighing, and all the other mechanical parts of the business are done by others. Then, instead of making out the bills himself, he simply sends the weights to the book-keeper or the superintendent; so it comes that we have all the way from, we might say, one-half to 3 and 4 or 5 men employed in the meter business, according to the size of the station.

Dr. Schuyler S. Wheeler:—Mr. Jenks, in describing the three-wire meter, omitted to explain the way in which the two sets of bottles took care of the three wires. I would like to call his attention to that. The current, in going out on the positive wire, for instance, is registered in the positive bottle. It may return through the neutral wire, and it may return through the negative. In that case it is registered in the negative bottles. After returning on the neutral wire, it is registered again. The passage of the current through this wire causes it to consume another 110 volts, so that it should be charged up twice.

Mr. Jenks:—I neglected to make it plain that the neutral wire acted as a compensation for the meter, as well as for the line service.

Mr. Francis B. Crocker:—I would like to add a word or two to what Prof. Roberts said about the general accuracy of meters, and the particular accuracy of electric meters, or, inaccuracy, if people prefer to put it that way. The word meter is synonymous with fraud in the popular mind. But I think it is more a question of human nature than anything inherently bad in the meter. Gas meters, as Prof. Roberts said, are probably actually in favor of the customer in at least three cases out of four; and any large errors would almost always be in favor of the customer, for the reason that the gas meter is a gas engine; it is worked by the gas, and the gas must flow through in order to work the meter. But the gas may flow through and not work the meter, if the meter is stuck, or the friction is great, or there is any other fact about it that prevents the proper working of the meter. Now, the same is true of this Edison meter, the electric current operates the meter, therefore the current must flow in order that the meter shall record. On the other hand, the current may flow, without fully recording its value. For instance, if the contacts of the meter circuit are poor, then the fraction of the current that should pass through the meter will not pass through it, and if the contacts are broken, it is possible that no current at all will pass through the meter, and yet a large amount of current may be used in the house. In fact, in any meter it is almost always the case that the commodity, whatever it may be, gas, water, or electricity, is much more apt to flow and not record, than that a record can be made without any flow, and I think that fact is forgotten when people condemn meters. The very nature of the apparatus requires that whatever is measured shall pass through in order to be recorded. Now, as to whether the company reads one thing from the meter and charges for another, that, of course, has nothing to do with the meters. It is entirely outside of the theory or practice of meters; it is merely a question of pen and ink. But I think the meter is as accurate as any other practical instrument, and I think that gas is measured as accurately as cloth or milk—

MR. POPE:—Or coal.

MR. CROCKER:—Or coal, or any other material that is furnished to the public. The fraud, if it occurs, and I suppose it does in many cases, is a case of deliberate fraud when the method and apparatus for making the measurements may be perfectly correct. It is after the meter has done its work that the book-keeper puts on the increment for which the meter has to take the discredit. There are so many more people who talk against meters than there are in favor of them, that I think a word said now and then on the other side is no more than fair.

THE CHAIRMAN:—Perhaps the gas companies will at least find some cause to be grateful to electricians from the way the veracity of meters in general has been maintained to-night. Has Mr. Lyne a word to say?

MR. LEWIS F. LYNE:—I do not know that I can enlighten you very much on electric meters, because I have not had anything to do with them. I have been very much interested in the paper which has been read, and also in the general subject of meters. I may say a word, though, in reference to other meters with which I have had a great deal to do; gas meters, for instance. I remember in one place where we thought the gas bills were too large, we used to put a wooden wedge under the edge of the meter, and tip it so as to increase the friction, and in that way, we proportionately reduced our bill right off. Not many months ago, I was in charge of an electric light station where we had a Crown water meter. Those of you who understand the construction of that meter, know that inside is a gutta-percha cogwheel that moves about, as the water passes through. We were bothered about getting water, and we thought that the pipe was stopped up with something, and we ran that way for several days; then we took the meter apart, and found that it was stuck; during that time, the meter had failed to record any water; but we had been running a 250 h. p. engine.

MR. J. W. HOWELL:—There is one point in the criticism which Mr. Jenks quoted from the past proceedings of the Institute, which, I think, ought to be mentioned. That criticism stated that the meter measures only a very small proportion of the current which is sold, that is a 975th part, therefore, any error in the measurement was multiplied by 1,000, practically. Well; that is true, and yet it is not true. If we measure 1,000th part of the current, and multiply by 1,000, any error in that measuring bottle or in the resistance of the circuit in which the measuring bottle is placed, is multiplied by 1,000; but the percentage of the error is always the same, and if we have an error of 10 per cent. in the circuit in which the bottle is, it means an error of 10 per cent. only in the result. No matter what kind of meter it is, whether that bottle is in the main circuit, or in the shunt circuit, a two per cent. error would be two per cent. in every case, and it would not be a 2,000 per cent. error, as the criticism would lead you to believe. There are a good many men here whom I should like to hear from about meters. I have watched the introduction of the Edison meter, in one place especially, and I have watched its growth from the very beginning. When the Edison meter was first put into stations, the first thing a man asked was, "Is the Edison meter accurate?" And, really, there was, for some time, a diversity of opinion among the Edison people as to whether it was really an accurate meter. But the experience of the users of the meter has been so gratifying, and has been so much to its credit, that you very rarely hear the accuracy of the Edison meter questioned to-day; in fact, among the Edison electric light people it is not questioned at all. It is known that the experience of the people using it for the last six years has shown beyond question, that the Edison meter is accurate. The station I spoke of is the station at New Brunswick, which started out not to use any meters. They would make a contract with a consumer, and give him such light as they thought he would use, at a fixed rate. The more they used that system, the more they were convinced that they were only getting about one-third as much for their current, as they supposed they would get. To overcome that, they introduced the meter system, and the result of changing from the contract to the meter system has been a very much diminished output in the station, and a very much increased revenue. They get twice as much in return for a given output of electricity as they did before. The accuracy of the meter there has been checked several times by the customers counting their lamp hours. I have never heard of a case there in which the meter was found to be wrong. I know of cases there, where the electric meter has checked the gas meter; I mean has differed materially from the gas meter. The customer in one case was getting his electric light very much cheaper than his gas; in the other case, the customer was paying very much more for it. In both cases, they found by going back to their gas meter, that one gas meter was charging too much, and the other was charging too little, and the electric meters, in both cases, were right. My belief, and the universal belief in the Edison meter, is the result of experience with it.

Mr. Francis R. Upton :—I would only add to what has been said here about the meter, my knowledge of the very long course of experimenting that Mr. Edison went through to obtain the results that we now have. I know that in the early days at Menlo Park, these various forms that have been shown on the screen were made and tried. I realize very fully the difficulties in making any mechanical meter, or any meter that will not stick at starting. I do not think that those who have given attention to the meter, have thought that any solution can be found; but that this trouble will come in, except with a chemical meter. In meters that use the heating effect, the sticking of the meter is an uncertain quantity, and is invariably against the company; those meters will never be used. If the error were in favor of the company, I have no doubt they would be largely sold and largely used, but they are invariably against the company. The Edison meter is formed with the zinc plates instead of copper plates, which were first proposed. That represents a large amount of work at Menlo Park, and again at the Edison Machine Works, in Goerck Street; and since then, Mr. Jenks has given an immense amount of time to it; so that it has, to-day, been built up into an instrument of most thorough accuracy.

Mr. E. T. Birdsall:—Mr. Edgar, of the Boston Edison station, is here; I am sure he could give us some data.

The Chairman:—We should be most pleased to hear from Mr. Edgar.

Mr. C. L. Edgar:—I do not know that I can add anything to what Mr. Jenks has said. We are using about 800 meters, and I have the utmost confidence in them. I have tested a great many, and I have had our customers test them for us. I have known of numbers of cases where they have proved within one per cent.; and I never have known of a case where we had to retreat from the position which we had taken, that the meter was correct. We have had a great number of meters in use where electric motors are on the circuits, and are able to determine just exactly the h. p. hours per month of each motor; thus being able to make a contract price in the next case which would be more satisfactory than if we had not made those tests.

One gentleman asked a question to-night as to the cost of a meter station. We have, as I say, 800 meters. In that department there are five men; three of them are, practically, boys, who go around town collecting bottles. We extend over a territory nearly three miles from one extreme to the other. Of course, the traveling is more than it would be for a different sized station. The fourth man does the weighing, and the fifth man makes out the bills and sends them up to the book-keeper, ready to be mailed; so that we really do more in the meter department than is ordinarily done; because, usually, the bills are made out by the book-keeper. It costs, perhaps, $2,500 a year to take care of the 800 meters; but a great many, as I say, are on motors where we are experimenting, making changes once a week, so as to get at a fair average for different classes of work.

Mr. Howell:—I suppose every one here knows that there has been a great deal of inventive energy wasted on meters. The records of the Patent Office show an enormous number of mechanical meters; and I think the Edison Company have had about as many presented to them as anybody has; and one great point in favor of the Edison meter, as compared with any other meter, is its cheapness. If we want to equip a station with 800 meters, as Mr. Edgar says his station is equipped, it makes a very great difference if we have to pay for a complicated mechanical contrivance, which is necessarily expensive; or whether we have to buy a box, with a couple of pieces of german silver and two bottles in it. The question of first cost is a very strong element in favor of the Edison meter. You can't afford to pay for your meters almost as much as for the station itself. A successful meter must have two qualities: it must be good, and it must be cheap; and I think the Edison meter has these two qualities pre-eminently.

PROF. ROBERTS:—As to the matter of mechanical meters, etc., I rather question whether we cannot some day arrive at a mechanical meter which will be satisfactory. So far as cheapness is concerned certainly a Waterbury watch is as cheap as these two bottles and two pieces of german silver, yet there is a great deal of mechanism in it. I wish to bring up one point showing how the customer, if he finds any expense against him, will want to economize. A station I know of has a 12 by 36 Corliss engine. At first the lamp renewals were paid for by the company, and the current was furnished by contract, everything ran beautifully and the engine was supposed to be developing about 65 h. p., but the indicator card showed about 100 h. p., from 6 o'clock in the evening to about 10; then it dropped down somewhat. The company began the system of charging for lamps, about 75 cents apiece. As soon as the customer got a bill for lamps the indicator card then showed that the average h. p. was reduced to 75. Therefore, they saved over 25 per cent. of power just as soon as the customer felt that the lamp breakage alone was being charged against him. I do not doubt the gentlemen who have compared the contract and the meter systems, can show even more remarkable figures than these.

MR. J. STANFORD BROWN:—There is one point which, perhaps, you will consider a side issue. Into the construction of this meter there entered one element, on which there has been considerable discussion during the last year, namely, german silver. It perhaps will not be out of place to enquire whether they have any special standard of excellence for german silver at the Edison laboratory.

MR. JENKS:—As I said in general, in the description, the best german silver that can be obtained is used; *i. e.*, an equal quality to that which is employed in making bridges. Of course, the standard, so far as the construction of each individual meter is concerned, is the resistance, and that resistance has to be determined to within a very minute fraction of an ohm, in placing the strip in the box.

Mr. Charles Wirt:—I happen to know that the german silver used in the Edison meter is 18 per cent. german silver, and it gives a little better temperature co-efficient than that quoted in books, I believe.

Dr. Wheeler:—Do you make any test of german silver before accepting it?

Mr. Jenks:—Yes, there are such tests made. Extraordinary pains are taken to have a good quality of german silver.

Dr. Wheeler:—Do you care to describe what they are?

Mr. Jenks:—I am not able, at present, to describe them to you. I think, perhaps, Mr. Wirt's statement answers the question sufficiently.

Mr. Wirt:—It is said that some electric light men are very prone to mix what has been done, with what is about to be done. I want to say that Mr. Jenks has been extremely conscientious in describing the Edison meter as it has been, and not as it is about to be. No changes of a radical nature have been made in the Edison meter since it was first introduced. I may say, that there has been practically concluded now, a series of experiments on the meter, and they are getting ready to offer a new meter, which will be a great deal better, in many respects, *i. e.*, in the cost of the meter; in the decreased cost of operating; and in the much less strain on the meter man's back. It appears to us, that, given a spool with a piece of german silver, the Edison meter depends on the laws of electrolysis and the chemical balance, and the chemical balance is probably the best, most reliable and most sensitive instrument known. So it appears to us that we do not need any large quantity of zinc to weigh or carry about, and we propose to reduce it one-half, in some instances one-quarter, and in some instances one-eighth, so that the box will be about half the size it is at present. The bottle will be half the size it is now. In place of two bottles there will be one bottle. We will call on the balance for the accuracy we can get out of it, and call upon the meter man for a little more pains.

The Chairman:—Are there any other speakers? If not, the the hour has arrived and gone by when we usually adjourn. There is an opportunity for further remarks, if anybody wishes to make them. If not, the Secretary has some announcements to make, I think.

THE SECRETARY :—Most of you are aware, probably, that there is to be an exhibition at Paris next season; and in connection with this, I would say that various engineering societies here have received invitations to participate in meetings held by different engineering societies on the other side, and the Institute has received two letters, one from the Institution of Civil Engineers, and one from the Society of Arts, in London. I wish to read these, and bring to your attention the arrangements which are being made by the Society of Mechanical Engineers, for an excursion to Europe.

THE SECRETARY then read the communications referred to as follows:

SOCIETY OF ARTS,
JOHN STREET, ADELPHI, LONDON, W. C.

December 8th, 1888.

SIR :—The Council of this Society has been given to understand that a visit of American Engineers to this country during the spring or summer of next year is in contemplation.

The Council will be very glad if the Society of Arts can in any way facilitate the visit of your members to England, or render their stay here more pleasant They will be glad to place the rooms of the Society at their disposal; and if their visit should coincide with the Society's Annual Conversazione in June, they will be very pleased to see as guests on that occasion such of your members as may be able to attend.

We have the honor to be, Sir,
Your obedient servants,
ABERCORN, Chairman of Council.
H. TRUEMAN WOOD, Secretary.

The Secretary of the American Institute of Electrical Engineers.

THE INSTITUTION OF CIVIL ENGINEEERS,
Established 1818—Incorporated by Royal Charter, 1828.
25 GREAT GEORGE ST., WESTMINSTER, S. W.,

November 23d, 1888.

To the Secretary of the American Institute of Electrical Engineers, New York City, U. S.

SIR :—It is reported that many engineers from the United States will probably visit Europe during the International Exhibition which is to be held in Paris in 1889.

In view of this, the Council of the Institution of Civil Engineers, at the first meeting of the present session, directed an inquiry to be addressed to you to

ask: 1st. Whether this report is correct, and, if so, whether your Institute can give any idea of the number of your members likely to come; 2d. Whether they will travel by way of England; and 3d. What may be expected to be the approximate date of their arrival and the duration of their stay in this country.

The object of this inquiry is to enable the Council to consider the possibility of making such arrangements as may best tend to further the objects which the visitors have in view, and to render their visit as useful and agreeable as possible.

The Council need hardly assure you of its good will towards its professional brethren in the United States, and of its desire to embrace this opportunity of manifesting its friendly feeling to the utmost of its power.

Of course, in any case, the facilities afforded by this Institution are always at the disposal of your members.

We are, yours faithfully,

GEORGE B. BRUCE,	**President.**
WILLIAM POLE,	**Hon. Secretary.**
JAMES FORREST,	**Secretary.**

THE SECRETARY:—About a week after the circulars of inquiry as to whether the members expected to go abroad this summer, were issued by Secretary Hutton, of the Am. Soc. of Mech. Eng., I was informed that they had already received affirmative responses from 75 members; so it appears that it is probable that this arrangement of a special steamer can be carried out. I am in negotiation with him now, in regard to the preparation of our own circulars in order to obtain the same facts from our own members. I bring it up to-night in order to give you this information in advance, to guide you in arranging for your vacation, and to induce you to save your money meanwhile. The next meeting of the Institute for the discussion of papers will be held on Tuesday, January 8th. The subject of the paper will be "Lightning Arresters, and the Photographic Study of Self-Induction," with numerous illustrations and experiments, by E. G. Acheson, Member; Electrician of the Standard Underground Cable Company.

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. VI. New York, March, 1889. No. 3.

The thirty-first meeting of the Institute was held at the College of the City of New York, 17 Lexington Avenue, on Tuesday, January 8th, 1889. The meeting was called to order by the Secretary at 8 P.M.

On motion of Mr. T. C. Martin, Mr. Francis W. Jones was elected Chairman of the meeting.

The Chairman:—Gentlemen, as there is no routine business to be transacted, I will not keep you waiting, but will avail myself of the pleasure of introducing Mr. E. G. Acheson, Member of the Institute, who has kindly volunteered to give us a paper upon Lightning Arresters and the Photographic Study of Self-Induction.

Mr. Acheson then read the following paper:

LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

BY E. G. ACHESON.

IF in the opinion of any an apology is required—and it is thought that such will be the case—for thus presenting a subject that is in any of its parts a reproduction of the experimental work of such authorities as Professor Hughes, Professor Lodge, Dr. Hertz and others, the excuse is offered that the original motive for entering into the series of experiments referred to in this paper was a legitimate one, namely—the clearing away of a little fog surrounding some annoying phenomena attending the working of cables of electric conductors. The investigations became so interesting, so striking and suggestive, and some of the results appeared to have such a value, that it was believed they would afford sufficient excuse for this presentation.

While the measurements made during these investigations were not so accurate as it would be possible to obtain with more refined instruments, the probable errors were not so great as to make them wholly valueless, care in the readings and many determinations checking to a great extent the errors due to the crudeness of the apparatus. Appreciating the fact that untruthful results are less desirable than none, owing to their misleading effects, it is intended to impress the fact that the determinations contained in this paper can at best only serve as a possible guide for future work of a more complete and accurate character. It is also to be hoped that they may promote the already lively interest displayed by the scientific world in discharges, and the sometimes oscillatory character of electric currents.

The mass of material and composite character of the work to be presented will necessitate considerable care in selecting the order of presentation, and it is believed that the subject can be more easily followed if the thoughts and experiments succeed each other in the order in which they occurred to the experimenter, and this plan will be followed even when the after thought is of the greater importance.

As previously stated, the primary motive from which followed this series of experiments was the clearing up of some uncertainty as to the cause of a few annoying troubles in electric cables. But, to be more precise, it was desired to determine the cause of the occasional "grounding" and failure of cables used in telegraphic and telephonic service, when to all outward appearances they were perfect in construction, protected with lightning arresters, and immediately before the moment of grounding indicated high insulation resistances. A case of this kind presented itself for solution, in which there were some features of peculiar attraction to an investigator. It was a cable containing five wires about one mile in length, and of that design known as anti-induction, the cross-section being star-shaped, each conductor having a surrounding wall of lead. It was used for telegraph work, and the conductors were connected at the ends to overhead lines, lightning arresters being provided for the protection of the cable; the terminals of the cable were on poles some twenty feet in height. The ground plates of the arresters were connected to the earth in the usual manner; that is, by wires running down the poles to plates in the ground.

This cable was continually getting into trouble, the conductors becoming solidly "grounded," and upon examination it was found that the "grounds" were caused by little kernels or spots of carbonized insulation. It was at the first moment thought that these had been introduced in the manufacture of the cable, but their repeated formation emphatically declared against this theory and made it imperative to learn the true cause. It was not probable that they were produced by the working currents, for they were of comparatively low electromotive forces, while the presence of the lightning arresters would, in accordance with the prescribed rules of their action, render it wholly unlikely that they could have been produced by lightning.

A close study of the experiments of Professor Hughes of self-induction, and of Professor Lodge on the "alternative path" had produced an unsettled condition of my ideas as to the real value of a lightning arrester. The work of these investigators, together with some few experiments of my own, had produced a feeling of interest as to the efficiency of even the best arranged arresters as connected in ordinary practice. I was prepared to believe this to be a practical live case of a well-mounted and connected lightning arrester not protecting. It even seemed possible that the cable itself was, in reality, protecting the protector, when viewed in the light of Professor Lodge's "alternative path" experiment.

In order to come to some definite and practical conclusion as to the possibility of these troubles having been produced by lightning, a series of experiments was undertaken on lines somewhat analogous to those of Professor Lodge.

In Figure 1, the general and important conditions of a cable placed in the earth and provided with lightning arresters as usually connected, are outlined. In the figure, K represents the cable, L the line wire, and W the ground wire connecting the protector C to the ground plate G.

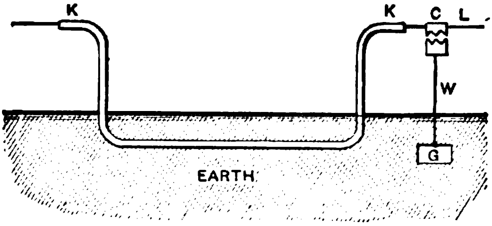


FIG. 1.

It is generally supposed that lightning—or an electrical discharge, as I prefer to call it—produced by the difference of potential between the cloud above the wire L, and the earth beneath, would, upon striking the wire, pass to the lightning arrester C. jump across the space separating the points to the wire W and thence to the earth, or the reverse, as the case may be. But while this is probably the case in many instances, it has not yet been satisfactorily demonstrated that there have not also been many cases where this plan of procedure was materially departed from, the discharge making a path for itself through the cable and the insulation, or perhaps dividing itself between both routes. In order to obtain more clearly defined ideas of the action of a discharge under these conditions, the apparatus shown in Fig. 2 was used, in which it will be seen that the fundamental features and the connections of Fig. 1 are reproduced for experimental work, the cloud being represented by the inner coating of the Leyden jar J, and the earth by the outer one, the charge being produced by a Holtz machine, of which M and N are the conductors. The long length of the cable in Fig. 1 is replaced by a short one, K. As before, L represents the line wire, and in this case is connected

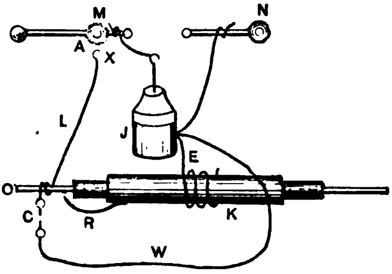


FIG. 2.

directly to the conductor, O, in the cable. The lightning arrester, is replaced by an equivalent, C, composed of two points separated an appropriate distance. The wire W is, as before, the ground wire connecting C in the former case to the earth, and in the latter to its equivalent, the outside of the jar; the lead covering of K is also connected by means of a short wire to the same outer surface. While in Fig. 1 the point on the line L which is struck by lightning is subject more or less to accidental conditions, it is in the case of Fig. 2 selected and adjusted by the ball X, as is also the value of the energy of action, by the areas of the surfaces on the jar J and the length of the striking distance, A.

With the apparatus arranged as just described and illustrated, it was quickly determined that a discharge could with great ease be caused to pass through the insulation of the cable, even when the space C was but a fraction of the distance through the insulation, and the length of W a few inches of stout copper wire. The current of discharge seemed to divide itself, a portion passing by W producing a spark at C, and another portion passing through the cable. A few more experiments also demonstrated that the wire W had apparently as much influence

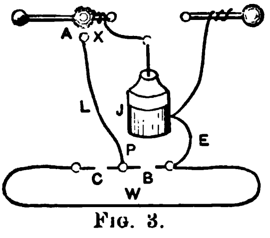


FIG. 3.

on the results as the space C, and consequently it became necessary to analyze the combination more completely to determine the relative values of c and w. With this object in view, the cable K was thrown out of the combination, and the connections simplified to those shown in Fig. 3. On close examination they will be found to be in every way similar to those in Fig. 2, with the exception of the replacing of the insulation of the cable K with a clear air-space B, and instead of connecting L to the conductor O it is here connected to a post P. The spaces C and B were both adjustable to a fairly accurate degree, B being easily readable to thousandths of an inch.

The system of experimentation was to take a certain length of wire, w, adjust A and C to known values, and then adjust B so that one spark would occur there for about every ten at A, and after having noted the length of the spark B a few inches would be cut off from w and B readjusted and again noted; this being repeated until w was wholly consumed. The values of B for these various lengths of w and a known length of C having been obtained, a wire similar to the original w was substituted for the former, and C having been removed from the circuit, as shown in Fig. 4, a similar set of tests were made, and the differences between the results, with and without C, indicated the true values of C and w in the combination, while the results with w alone show the value of each additional inch.

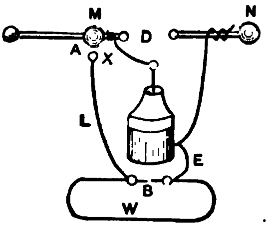


FIG. 4.

In plate A the results of these measurements have been plotted into curves. The length of w in inches is plotted horizontally, while B in thousandths of an inch is laid out vertically. The capacity of the jar J was .0025 m. f., and A was retained uniformly at .286 of an inch. The full line curve is that of w without C in the circuit, while the dotted one is that obtained with it in. The length of C was .02 of an inch, and w for both tests was composed of a copper wire having a diameter of .052 of an inch.

A brief study of these curves teaches that, when w is but a few inches in length, C plays a very important part in determining the length of B, and also that its influence rapidly vanishes as W is lengthened, and actually produces an opposite effect after a certain length of W has been arrived at. Thus, with W 20 inches long, C has a value equal to 59 per cent. of the total circuit; at 110 inches it appears to have no value, while at 180 inches the value of the compound circuit is about 7 per cent. less than W alone. These points are, however, of even less importance than the lesson to be learned from the study of the curve of the results obtained from W alone. Here is a case where a relatively small amount of energy contained on the surfaces of the jar J will, when discharged into the circuit composed of the air space A and the wires L, W and E cause the passage of energy in the form of a spark at the point B of a length of over one-third of an inch, and this when the wire W is but 20 feet in

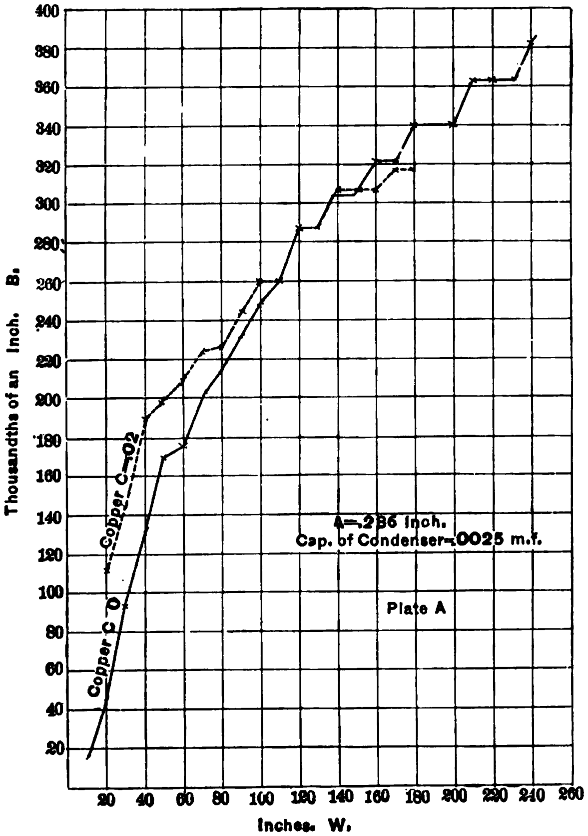


PLATE A.

length, or that of an ordinary ground wire down a telegraph pole.

This does not look very encouraging for lightning arresters with long ground connections, and rather puts to ridicule the fine adjustment of the points of these devices. The subject will, however, stand more than a little study and investigation before forming judgment, and perhaps the arrester is all right after all, in so far as the close adjustment is concerned, even if the long ground connection does have to go.

Those who are familiar with the recent investigations of Professor Lodge, investigations so exceedingly interesting and now so celebrated that few there are who have not made themselves familiar with them, will at once have recognized in Fig. 4 connections very similar to those used in the experiment of the "alternative path," by Professor Lodge. The results obtained by him and in the present experiments are in the main the same, and those differences that do exist are probably to be attributed to the following facts: Professor Lodge's work was done with instruments of great precision, the sparks were always produced between balls or rounded surfaces, and the object was to determine laws and formulate their mathematical expressions; while, on the other hand, the experiments now being described were made in some cases under the most difficult conditions, with apparatus limited to a most exceptional degree both in quality and quantity, a certain portion of the sparks were produced between points, and the object in view was to dissect, analyze and subjugate the phenomena under examination to and for the benefit of practical work, the every day affairs of the electrical profession.

It is, however, exceedingly difficult to avoid a little speculation, now and then, as the subject unfolds itself, and it is hoped the experimenter may be pardoned for any references that may be made to the theories that have been the immediate cause of some of his researches. It is even hoped that the work performed may atone for the views about to be advanced, notwithstanding the fact that they are quite contrary to those deduced by so high an authority as Professor Lodge.

After a considerable portion of this work had been done and the conclusions more or less formulated, I was much pleased and greatly strengthened in the opinions then being formed to learn through a letter from Professor Hughes to Professor Lodge, published in the London *Electrician*, issue of September 28, 1888, that Professor Hughes had, at least formerly, held the same views of the "alternative path" that I had formed, and which will now be presented in connection with what is thought to be experimental demonstration of their truthfulness.

Referring to Fig. 4, it will be seen, as has already been stated, the conditions and arrangements of W and B are such as to produce and actually constitute the "alternative path," as shown by Professor Lodge. The simple assertion that they are alternative paths does not, however, make them such, and, while they may under certain conditions and adjustments become so, it is possible to conceive of their being otherwise. It is not beyond the bounds of reason to assume that the energy that has been discharged from the jar J, traversing the wire W, produces in it a condition that may be termed *electrification;* and on the disappearance of the controlling influence of this energy, would not the wire W return with an almost infinite rapidity to its original non-electrified condition, and, in doing so, produce as the result of this enormous rapidity of motion, an electromotive force at its terminals of a greater value than that of the original energy? In other words, will there not be a restoration of the energy expended in distorting the lines of energy within the wire, and this restoration being performed in a shorter interval of time than that occupied in its production, will not a higher electromotive force be obtained even though the value of the return would be less than that of the original to an extent equal to the losses due to the internal friction of the mass?

This being the case, this current of discharge from the wire—which was first discovered by Henry and investigated by Faraday and called by him the *extra current*—would, owing to its greater electromotive force, jump the space B and thus close its circuit, even when the distance was such as to prohibit the passage of the original current as discharged from the jar J.

It seems as though this might be the case, and, if it is, then the discharge that was obtained through the insulation of the cable in Fig. 2 need not have been a part of the original current from the jar J, but the current discharged from the wire W. If it was, a shortening of W would tend towards overcoming the difficulty. To test the case a short wire R was attached to the lead and bent out over the insulation so as to present a point to the conductor O, the distance between the point and the conductor being about the same as the thickness of the insulation; W was now removed, as was also C. It will be seen that E, the lead of the cable K, the short wire R and the space separating its end from the conductor O, form an equivalent to W and C. With this arrangement it was not possible to cause a discharge to pass through the insulation.

Now, in accordance with the views which have seemed to me most probably true, this change is due to the reducing of the length of the path W, and which is now composed of R, E and the lead of the cable, this deduction resulting in a subsequent lowering of the potential of the extra or self-induced current. But if the theory of Professor Lodge's "alternative path" is correct, then the change would be due to a removal from the path W of a certain amount of resistance, which has been termed by Mr. Oliver Heaviside, *impedance.*

We here have an apt illustration of inertia under the American and English definitions, as so forcibly stated by Professor Ayrton. As I have presented it, the spark B is the result of inertia, with the American definition—a resistance to stopping. While Professor Lodge's theory would account for it by inertia with the English interpretation of that word—a resistance to motion.

Accepting the belief that the spark B, when adjusted to the critical point, was caused by the self-induction of the wire W, a variety of experiments were made with the desire of obtaining experimental proof of the theory. Repeated occasions presented themselves for mathematical work, and if time and attention could have been given in that direction, the results would unquestionably have been of interest and value, but the work was perforce restricted to the more limited confines of experiment and reasoning. The first experiment of the series was to replace the wire W in Fig. 4 with one of smaller diameter, and re-determine the value of B in the manner already used for a copper wire having a diameter of .052 of an inch, and of which the results have been plotted in plate A.

Before presenting the experimental results, it is desirable to consider what might be expected from these changed conditions. There will in all cases be practically the same amount of energy discharged into the circuit, for the areas of the condensing surfaces on the jar J remain constant, the length of the striking distance A also remains uniform, and while it is true that the resistance of W, as measured by a Wheatstone bridge, would vary inversely as the square of the diameter, the resistance of the complete circuit of discharge is, owing to the air space A, practically infinite, and hence the relatively trifling resistance of W, even when of the finest wire, may be wholly neglected.

The theory that the self-induced or extra current in a wire is a recoil action, is based primarily upon the hypothesis that there exists, before the electrifying current is discharged into the circuit, a prior state—the *electro-tonic* state of Faraday. This *electro-tonic* condition consists in the existence of lines of energy—thermal they may be—with directions coinciding with the radii of the wire; these lines or axes of energy being surrounded by circular lines of magnetic force. An electrical current being discharged through such a wire would cause, under the influence of its well-known magnetic whirls or circles, whose planes are at right angles to the line of propagation and in this case to the axis of the wire, a deflection of the planes bounded by the closed lines of magnetic force surrounding the radial lines of energy which are within the wire. The work of producing this deflection would, I think, constitute what is known as the variable resistance which meets a sudden discharge, and which was made the subject of that grand paper on "Self-Induction" by Professor Hughes before the Society of Telegraph Engineers, at their meeting of January 28, 1886. This deflecting of the radial lines of energy, while constituting a resistance to the inducing current, is also, the source of, or rather, during the act of deflecting, produces, an electromotive force contrary to that of the inducing current. This counter electromotive force exists only during the interval of deflection, and quickly vanishes from the path of the inducing current. On the moment of the interruption of the inducing current, and consequent withdrawal of its attending magnetic whirls or circles, the deflected radial lines of energy swing back to their radial positions, producing by their motion an electromotive force of such a direction as to form a current in the same direction as that of the inducing current. This return swing or recoil, as of a spring suddenly released, is of such a velocity, and overlapping, probably, what might be likened to the tail end of the disappearing inducing current, produces a momentary electromotive force of a value higher than that of the original current. These being the actions of the forces and energies, it now becomes necessary to understand what differences, if any, the size of the wire through which the original current is

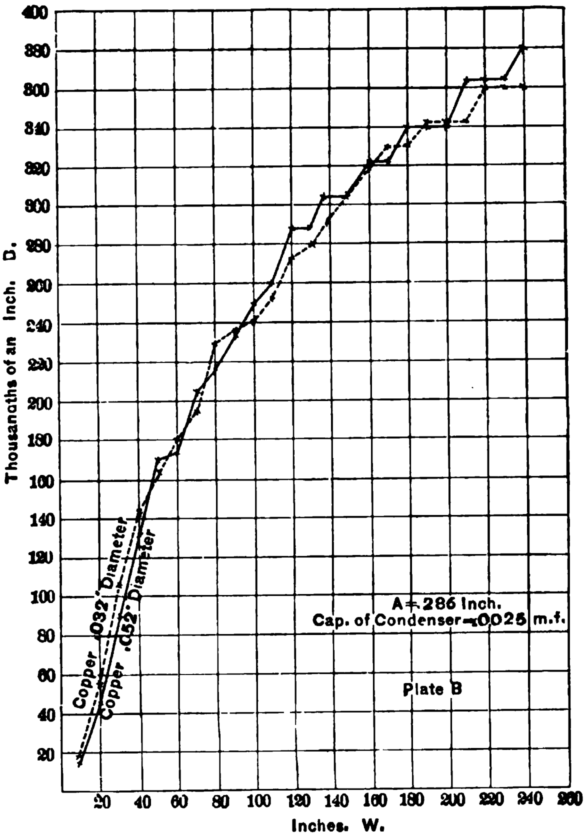


PLATE B.

discharged will have upon the value of the induced, or, as it is more generally termed, the self-induced electromotive force. All of the experiments and considerations will be made under the conditions of a constant capacity for the jar J and uniform length of A, and consequently one value for the energy discharged into the circuit. In all cases there will, of course, be a certain amount of the energy that has been discharged into the wire lost and converted into heat. This lost energy will be greater in value in the smaller wire than in the larger in a proportion inversely as their diameters. This greater loss in the small wire and resulting reduction in the available inducing energy would, of course, tend to produce a diminution of the self-induced current. This reduction is, however, exactly offset by the greater concentration of the inducing energy, its inducing value having been increased in a proportion inversely as the diameters of the wires.

It is feared this argument has become tiresome, and an immediate relief will be found in plate B, where I have plotted two curves; one, the full line curve, is the same as that shown in plate A, and was produced as formerly stated. The dotted line curve was made with a wire of copper having a diameter of .032 of an inch. The two curves are so nearly alike as to permit of the belief that they are in reality the same, and that the slight differences existing are due to errors of measurement. Here is experimental proof that the electromotive force of the self-induced current, as measured by the spark B, remains constant as long as the energy discharged into the circuit is of uniform value, and is wholly independent of the diameter of the wire, and this seems to give countenance to the argument immediately preceding.

It is more than probable that those who have been following

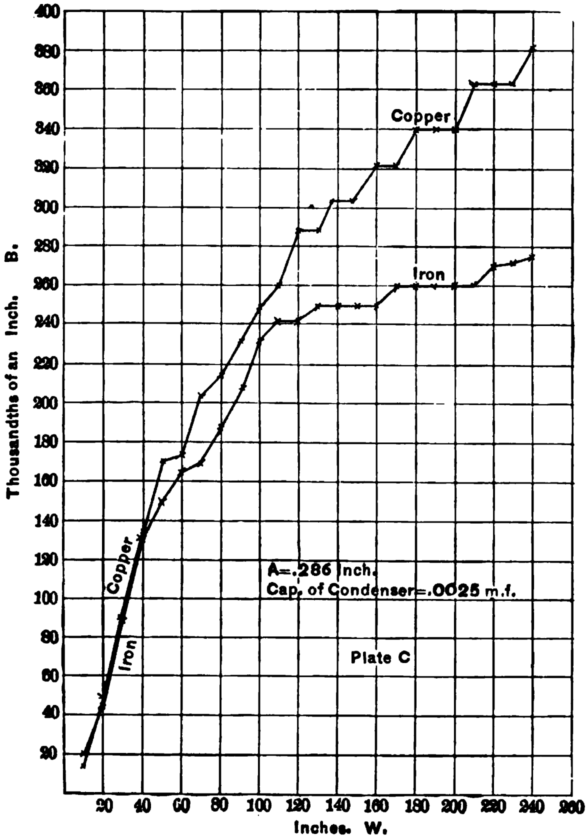


PLATE C.

the subject closely will, before this time, have had the question asked, from somewhere within: "How would these curves on Plate B have looked had one been made with an iron wire?" The answer is ready at hand on Plate C. It is not exactly an answer to the question as stated, for there are here shown two curves, one produced with an iron wire and the other with a copper wire, but both are of the same diameter, .052 of an inch. The curve of the copper wire is the one you are already familiar with.

This paper has even now attained such proportions, and there are yet so many points to present, that nothing more than a moment's glance at these curves can be offered. A study of them teaches that for lengths of 40 inches, copper and iron will, when acted upon inductively by discharges of one value, furnish self-induced currents of like electromotive forces. From 40 to 110 inches they show slightly different values, while beyond this length their efficiencies rapidly diverge; that of the iron running much lower than that of the copper, and apparently fast approaching a maximum. As a means of accounting for these different efficiencies, I would suggest this theory. Up to 40 inches of length the time occupied by the passage of the discharged current was too short to permit of any magnetic effects occurring in the iron, and consequently the wire was equivalent to its neighbor, the copper; from 40 to 110 inches small magnetic effects were produced, resulting in a lag or retardation of the velocity of the recoil deflection of the radial lines of energy, and resulting, of course, in a lowering of the electromotive force of the self-induced current; beyond 110 inches, the time of discharge rapidly increased in duration, and thereby produced the decided flattening of the curve, as shown.

There is one more set of curves to present: They are shown in Plate D. and were made with the object in view of determining the effect of a variation in the amount of energy discharged into the circuit. Experiments were made with both copper and iron wires; the curve of the copper is plotted in above that of the iron one. In the plate, the capacities of J in millionths of a microfarad are plotted horizontally, while the lengths of the B sparks in thousandths of an inch are plotted vertically. The length of the A spark was, as before, .286 of an inch. I was prepared to see the B spark increase as the energy discharged increased; but I was wholly unprepared to have it decrease as the

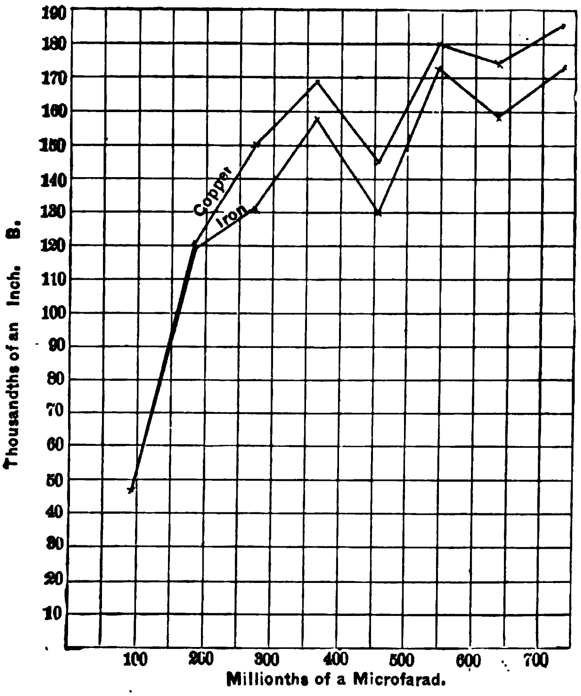


PLATE D.

result of added energy after a certain point had been arrived at, and still less prepared to obtain the wavy curves, as shown.

Time will not permit of entering into any consideration of the peculiarities of these curves.

There is a little experiment, the record of which I wish to interline at this point; it may answer a question, and perchance wet the powder of some intending questioner. Looking over my sketches and coming to Fig. 3, the thought struck me that an error might have been introduced into the experiment where the C spark formed a part of Professor Lodge's "alternative path," due to there being no connection having other than an infinite resistance between the ball X and the outer coating of J, while in all of the other experiments there was the circuit formed by W. It seemed possible that this change might have produced a different value for the energy of the discharged current, as the difference of potential between M and X might be altered. To decide the question, the connections as shown in Fig. 4 were used. The balls forming the terminals of the Holtz machine were caused to approach each other until the discharges occurred about an equal number of times between them at D and at A. Next the space B was adjusted to the critical length. If the existence or non-existence of the wire W and resulting metallic connection between X and J influenced the value of the potential discharge, its removal ought to lower the potential difference between M and X, and cause all of the discharges to occur at D. The experiment, however, did not prove this to be true, the removal or replacement of W having no apparent effect upon the number of the discharges that occurred at A and D; but, of course, when it was removed every spark at A caused one at B.

A few experiments were now made to determine the effect various portions and positions of the parts of the wire W would have upon other portions of itself. The connections shown in Fig. 4 were used. A length of 50 inches of cotton-covered copper wire, with a diameter of .033 of an inch was used for the wire W. It was arranged in the form of a circle. A was, as before, .286 of an inch, and the capacity of J was .0025 M. F. The critical point of B was found for these conditions, and W was then formed into a figure whose two sides were parallel, and B was measured for various distances between the two sides. These distances were, however, only approximate.

Position of W.		B in inches.
Circle		.182
Sides separated	3¼ inches	.170
	2½ inches	.137
	2 inches	.137
	1½ inches	.137
	1 inch	.137
	½ inch	.114
	⅛ inch	.050

The next experiment, illustrated in Fig. 5, was to take the same wire, W, and coil it upon a paper tube having a diameter of ¼ of an inch, the wire forming a spiral of one layer about two inches in length. This spiral being placed in the position W the critical point of B was found to be .197 of an inch, or over 8 per cent. greater than when the wire was in a circle. While the spiral was in the last position an iron wire one-tenth of an inch in diameter was inserted into the paper tube, and now the spark B only measured .170 of an inch, over 13 per cent. less than without the iron wire, and more than 6 per cent. less than the plain circle. Another experiment was made like the last, with the exception that the iron wire was long and bent around into a closed circuit, but it did not alter the results. The lowering of the value of the self-induced current upon the introduction of the iron wire was to me at the time startling, but a consideration of the case showed that it should be so, for a portion of the energy of the inducing current was absorbed in producing work in this mass of iron, and of a necessity weakened the self-induced current to an extent measured by the work this extracted energy could and would have done in the wire W, had it not been withdrawn. I have, however, here a confession to make. I was all of the time considering that this energy was expended in producing magnetic effects in the iron wire until a friend who had called to see some of the experiments suggested that I insert a copper wire instead of the iron one. I did so. The effect was the same as that obtained with the iron wire. The explanation was simple: It was that not magnetic effects, but electric currents, closed upon themselves within the wire, had been produced. The reduction of the length of the spark B now became a simple question of causing the inducing current to do more or less work external to the wire W, and as another means of causing it to do so, the spiral was surrounded by another spiral of wire, with the ends so arranged that they could be connected or opened as wished. With this arrangement, the spark B could be made to come and go, as desired: for by adjusting it to the critical point with the terminals of the outer spiral open, it would entirely disappear when they were united, all of which agrees perfectly with the former experiments and conclusions.

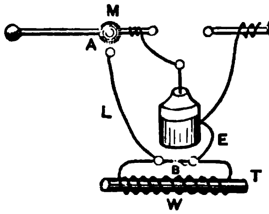


Fig. 5.

Another short series of experiments was conducted with parallel circuits, as shown in Fig. 6. The lengths of the parallel portions were 35 inches, the distance separating them being .052 of an inch. The total lengths of L and W were 130 inches. A and J were of the same values as heretofore used.

The conditions of the tests and the resulting lengths of the spark S may be briefly stated thus:

Test No. 1.	L	was	copper,	diameter	.052 in.	S = .031 in.
	W	"	"	"	"	
Test No. 2.	L	"	"	"	"	S = .024 in.
	W	"	iron,	"	"	
Test No. 3.	L	"	"	"	"	S = .020 in.
	W	"	"	"	"	
Test No. 4.	L	"	"	"	"	S = .025 in.
	W	"	copper,	"	"	

If my memory serves me right these results are the same, qualitatively, as those obtained by Professor Hughes, and clearly show the apparent sluggish action of iron, both in imparting and receiving magnetic inductive effects. Its relations to these magnetic effects resemble the actions of a polished metal surface when associated with heat; it radiates and absorbs poorly.

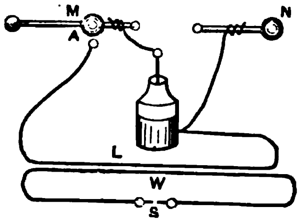


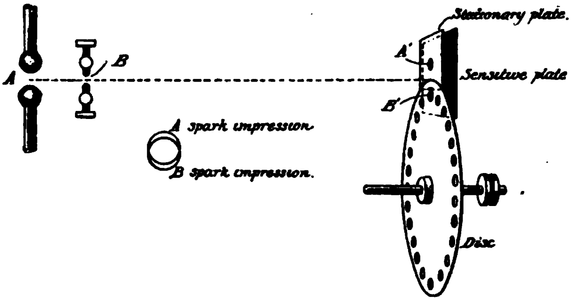
FIG. 6.

It now becomes my pleasant duty to present the concluding and, at the same time, most interesting and convincing proofs that W is not an "alternative path," but that the spark B is produced by, and consists of, the discharge of the self-induced current. I have called this series of experiments

THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

Unfortunately, at a late day, a day so late as to prevent my re pairing the loss, I met with at least two accidents, and each of them cost me one or more negatives which were a part of the record of these experiments. I shall, however, endeavor to make those remaining demonstrate the desired points.

In making these experiments the end in view was to obtain photographic records of the duration of the spark A, with and without the spark B; with and without the wire W; with various lengths of the wire W, and, if possible, to detect a difference of time between A and B. A disc of tin plate, 20 inches in diameter, was mounted on an arbor and belted to a system of pulleys of such sizes that by working the train by hand a velocity of about 75,000 inches per minute could be given to a circle of holes that had been punched in the disc at a distance of one inch from its edge. These holes were about one-eighth of an inch in diameter and one inch apart from centre to centre. The disc was so placed in relation to the positions of the A and B sparks that a line stretched from A to one of the holes in the upper edge of the disc would pass through B. Immediately back of this same hole a sensitive plate was fixed so that the hole was in front of the centre line, while the plate was removed sufficiently far to avoid being touched by the disc. One-half of the sensitive plate projected above the edge of the disc; this upper portion was covered with a tin plate in which a hole was punched of a size similar to those in the disc. The lower portion of the sensitive plate—that is the part back of the disc—was also covered with a tin plate in which a window had been cut a little over one inch square, and in such a position that the holes in the disc passed in front of it. It will be understood from this description, that while the disc revolved there could be two holes in front of the window, while there could not be less than one. With these adjustments sparks at A and B, separately or united, would produce an impression upon the sensitive plate, through the upper hole, and through one or two holes, as the case might be, in the revolving disc.



APPARATUS USED IN EXPERIMENTS.

As a result of the accidents above referred to, it will be necessary to introduce the series of experiments with photograph No. 5, where we have the spot produced through the hole in the upper stationary screen marked A, and a spot lower down on the plate marked B, and which was produced through one of the holes in the revolving disc. The object in having the spot A

was that it might offer a means of comparison between impressions made through stationary and moving apertures, and also show the effect of vibrations, if any existed, in the various parts of the apparatus. The value of the energy discharged through the circuit from J was disregarded, care, however, being taken that there should be no change in it throughout the entire series of experiments. The velocity of the apertures or holes in the disc was fairly, but not absolutely, determined to be 75,000 inches per minute. With this velocity of movement, a displacement of the apertures of one thousandth of an inch would represent an interval of time equal to $\frac{1}{75000000}$ part of a minute.

The series of experiments will be divided into four sets as follows:

Experiment 1, photograph No. 5.—The wire W was of copper, 76 feet long and .052 of an inch in diameter, the spark A was $\frac{5}{16}$ of an inch and was retained at this for the entire series.

The B spark was adjusted to a point near the critical state. but, owing to the necessity of its production with the first A spark, it could not be set to the same delicacy of action as heretofore. The photograph shows a slightly blurred or indistinct outline on the right-hand side of the spot B, which is more clearly brought out by comparison with the spot A. From this it is concluded that a measurable length of time was occupied by the spark, as the aperture in the disc through which this B spot was produced must have moved an appreciable distance in order to have produced this elongation or "ghost," as the photographic profession would term it. Owing, however, to the absence of any displacement of the distinct outlines of the spot, it cannot be assumed that there was any difference in the time of A and B, or, at least, not of sufficient value to permit of measurement with this apparatus.

Photograph No. 6.—In producing this photograph all of the conditions remained the same as those used in photograph No. 5, with the one exception of the spark B, which was removed from the circuit by separating the points beyond the striking distance. Here, by chance, two holes, or rather one and a half, were in front of the window at the moment of sparking, and produced the spots one and two. These spots exhibit displacements that, roughly measured on the negative, are one-tenth of an inch in length, and show a duration of the spark of not less than $\frac{1}{750000}$ of a minute.

From these two photographs the conclusion is drawn that the spark B shortens the duration of the spark at A.

Experiment No. 2, photograph No. 7.—The conditions of the circuit in this case were: Wire W shortened to 20 inches in length; spark B still out of circuit. This photograph shows about the same length, but a darker "ghost" following the spot B than did photograph No. 6, and indicates a corresponding increase in the work done at A.

Photograph No. 8.—The conditions are the same as the last, excepting that the B spark is again introduced. While there is still a measurable duration to the spark, the shortening of the interval due to the spark B is very decided.

Experiment No. 3, photograph No. 9.—In this experiment the entire apparatus associated with the spark B was cut out and the outer surface of the jar J was connected by a short copper wire to the ball X (refer to Fig. 4). An examination of the photograph indicates that A existed for an interval of time approximately equal to that of the last experiment (photograph No. 8) and very much less than that shown in photographs Nos. 6 and 7.

Photograph No. 10.—In this case the wire W was removed and the spark B adjusted to about $\frac{5}{16}$ of an inch. There seems to be, possibly a very slight increase in the time over that of photograph No. 9.

Experiment No. 4, photograph No. 13.—To this photograph special attention is invited. The conditions under which the negative was produced were wholly different from the foregoing. The tin plate covering the upper half of the sensitive plate, and which carried the aperture through which the spots A were made, was removed, the plate was lowered, so that a point represented by the spot marked 1 came on a line with the apertures in the disc. The wire w was composed of 50 inches of cotton-covered wire like that used in some of the earlier experiments, and was formed into a circle. The B spark was removed and was not used during this set of experiments. The A spark was, as before, $\frac{5}{16}$ of an inch, and the spot 1 in the photograph is its impression through an aperture of the disc, and, as readily seen, shows a very considerable duration of time.

The sensitive plate was now elevated a little, the wire w was coiled into a spiral, as described in a former experiment, and the spark A was once more caused to pass. Spots 2 and 2′ are the impressions produced by it, and, as will be seen by close inspection, exhibit longer "ghosts," and consequently a greater interval of time, than was shown in spot 1. Once more the sensitive plate was moved upwards, and an iron core having been inserted into the paper tube upon which w was coiled, the spot 3 was produced by another spark, the "ghost" produced being smaller than either of the former ones. These three experiments show conclusively that the presence of the wire w in the form of a loop or circle in the path of the discharged energy, tended to prolong the action upon the sensitive plate; next, that the coiling of the wire into a spiral increased the duration of that action, and lastly, that the introduction of the metallic core into the coil, and consequent stealing away of a portion of the discharged energy, shortened the time of action to an interval less than that of the loop.

The field opened up by these photographs for theorizing and speculating is a very broad and rich one, but the time is now too short to more than indicate the manner in which these results add strength to the conclusions deduced from the former experiments. Applying to the photographic experiments the theory already advanced, that the B spark was a discharge from the wire w, it is found, as would be expected, that when B is absent the self-induced current discharges into the jar J, producing in its passage a spark at A; this spark following immediately upon the heels of the original A spark. The jar being in this manner recharged, but, of course, in the opposite direction and of a slightly lower potential, will again discharge into the circuit—the discharge at a reduced potential being facilitated by the thermal and, possibly, magnetic conditions of the air space A, and which were produced by the first spark.

This second discharge will leave the jar J once more charged, and will be followed by other discharges of a continually decreasing value, until the energy has been dissipated as heat or into other forms of energy through and by the various portions of the circuit. This subject of the oscillating discharge has lately received, at the hands of Dr. Hertz, a masterly and most highly interesting treatment.

Referring once more to photograph No. 6, which was produced by an A spark with W composed of a copper wire 76 feet in length, the impression produced by the original, or first spark, is clearly defined, while in its wake is traced the track left by the sparks produced by the rapidly weakening, oscillating, self-induced currents—a track fading away like the tail of a comet, to an invisible end.

The next step in the inquiry is to determine the effect of introducing the spark B, and for this purpose reference will be made to photograph No. 5. Here we find, as would be expected, that the comet-like tail has almost disappeared, the natural result of having offered the self-induced currents from the wire W, a means of short-circuiting themselves across the points at B. The slight trace of a tail—a blurred outline—is to be attributed to the self-induced currents of the portions of the discharge circuit lying between the points of B and the surface of the jar J.

Shortening the wire W, with the spark B removed, should have the effect of increasing the amount of energy dissipated in the air space A, and further than this there should be little or no alteration. This increased amount of dissipation at A may be popularly explained thus: The total circuit into which the energy contained in J is discharged, and which consists of the space A, the wire W and the connecting wires, constitute the bodies of spaces from which the energy is radiated or dissipated, and, therefore, any contraction due to removing a portion of them will, of necessity throw more work upon the parts remaining. That this is correct is illustrated by photographs No. 6 and No. 7, where the conditions were the same other than a shortening of W from

76 inches in No. 6 to 20 inches in No. 7. A comparison shows that while the length of the "ghosts" are practically the same, that of No. 7 is heavier—more material. In the same manner in which the work was increased in the space A, so will it be increased in the wire connecting the points at B to the surfaces of the jar J, and for this reason the introduction of the spark B should not be expected to cut off the tail from the spot B to the same value obtained with a long wire at W. This is the case, as is shown in photograph No. 8, where W is 20 inches and B has been returned to the circuit. If the "ghost" shown in photograph No. 8 was due to the self-induced currents in the connecting wire, as has been assumed, then the same results should be obtained by cutting off the entire portion of the circuit represented by W and the apparatus associated with the B points, and short-circuiting these connecting wires. That this is the case is shown in photograph No. 9.

Cutting out W and replacing B did not seem to alter the results very much, if any, from those last mentioned, and this is just what would be expected.

In photograph No. 13 it is demonstrated that the coiling of a wire into a spiral tends to cause a retention of the energy within itself, and is due probably to the gathering up of a portion of the energy radiated or dissipated by one convolution by the adjoining convolution, and thereby restoring it to its own mass. This retarding of the process of dissipation will result in a prolongation of the time of A and consequent action on the sensitive plate, as is shown by the increased length of the "ghost," following spots 2 and 2′ over that following the spot 1. As would be expected, the length of the "ghost" is immediately contracted upon increasing the rate of dissipation of the energy as results from the introduction of a metallic core in the coil of W, as is illustrated in spot 3.

The final conclusions to be drawn from these experiments are:

1st. The "ground plate" of a lightning arrester should be connected to earth by as short a connection as may be possible.

2d. When an air line connects to a cable, a lightning arrester should be provided and attached to the armor of the cable by a connection not more than a few inches in length.

3d. When a short "ground wire" can not be used, one formed of a number of strands may be used, preferably not twisted into a cable.

4th. Any method by which a portion of the discharged current which passes over the "ground wire" may be withdrawn, will tend to increase the efficiency of the arrester.

5th. Under conditions similar to the tests, copper or iron may be used with equal efficiency, when the lengths are under forty inches, and over this length iron is more efficient than copper.

6th. The resistance of the "ground wire" is of little consequence, but it should have a size sufficient to carry off the charge without overheating.

7th. The insulation in a cable may be punctured by lightning, either by the direct discharge, due to the points of the arrester being too far apart, or by the current of self-induction from the "ground wire," when that wire is of any considerable length.

8th. Dissipation of the energy of discharge reduces the self-induced current.

DISCUSSION.

THE CHAIRMAN (Mr. Francis W. Jones):—Gentlemen, you have before you a very interesting subject. It is unnecessary for me to say how well and ably it has been presented. I will not take any of your time, but lay the subject open to your consideration and for any remarks the members may wish to make. It is understood, of course, that all present are at liberty to take part in the discussion. We should be pleased to hear from Dr. Moses.

DR. OTTO A. MOSES:—Gentlemen, as I have had a very good opportunity of knowing the careful methods in which Mr. Acheson directs his thoughts experimentally, I feel to a certain extent diffident upon being called upon so suddenly to express any doubts as to the correctness of his deductions. He has conferred with me occasionally on the subject in the course of his experiments. There are one or two things that might tend to start this discussion which I will suggest, so that some who are present here, who I know have taken great interest in this matter may continue it.

In the space A did you consider the difference in the size of the condensing surfaces of N and A?

MR. ACHESON:—Yes; I attempted to make a measurement of the capacity of the apparatus with the jar removed; but although I had a constant on the galvanometer—a very high constant—I failed to get any capacity, not because there was none there, for we knew that there was, but it was beyond the reach of my instruments. The size of the balls was considered, of course, and the ball x was made of a size that would allow me to obtain a sufficiently high electromotive force without spinning off, as it would do where it was very small.

Dr. Moses:—Would you be so kind as to enlighten us as to the nature of the apparatus with which you made these speed determinations? You condensed the explanation a little. Would you kindly tell us in what way the experiment was carried out?

Mr. Acheson:—You mean the spark length? I thought I had described that probably as fully as I am able to. I do not know whether I could exhibit it more fully without the actual apparatus.

Dr. Moses:—Let me ask a few questions. By what means did you cause your rotations?

Mr. Acheson:—By hand. I used, in fact, Mrs. Acheson's sewing machine as a motive power.

Dr. Moses:—In plate No 13, when you shifted the machine—

Mr. Acheson:—Well, I shifted the position of the sensitive plate, my revolving disc remaining in its former position. As the disc was rotating, immediately back of it was my sensitive plate, and in order to produce this first spot marked 1, I had the plate at a certain height and raised it higher for the succeeding ones.

Dr. Moses:—Has it ever struck you that perhaps these experiments might have great bearing on the subject of sparking in the brushes of dynamo machines?

Mr. Acheson:—Well, I don't know but that they have.

Dr. Moses:—By inserting shunts between the brushes, looping the brushes together instead of making them direct in contact so as to be able to time the sparks. It would be a very interesting thing for you to experiment upon. Your power there of avoiding the spark might enable you to avoid the spark on the brush, which comes evidently from the same cause. In the paper in which Prof. Hughes describes these experiments to which you refer, he indicated that there might be some differference in the metals that were used for brushes. There had been experiments carried on in this country before then in that direction; but it seems to me it would be quite a prolific field of investigation to find out whether by alternating brushes, perhaps of iron and copper, you might not be able to eliminate the spark from the dynamo machine.

Mr. E. T. Birdsall:—Dr. Moses speaks of sparking from the brushes of dynamos. It has been some time since we have seen much of that.

Dr. Moses:—There is a gentleman here who represents an interest in the city of New York where very considerable dynamo sparking can be seen.

Mr. Birdsall:—I represent electrical interests, and I have seen dynamos, I guess half a dozen different makes, that did not spark; also motors, which are practically the same thing.

Dr. Moses:—You have heard the anecdote of the man whose lawyer told him he could not be put in prison on account of a legal technicality and who wrote to his lawyer saying that notwithstanding that, he was actually in prison. I think if he looked, even without a microscope, at the brushes of the dynamo machines, he would find sparks.

The Chairman:—We have with us a gentleman from Boston, Mr. Hamilton, from whom we should be pleased to hear.

Mr. Leonidas Le C. Hamilton:—I have been very much pleased to hear the paper which has been read to you this evening. I do not know that I could add anything whatever to what has been said already. One question arises in my mind—whether or not the ghost, so called, in the photograph might not have been caused by a difference in the revolution of that disc. It seems to me that the experiments, with no possibility, as I can understand it, of accuracy, as to velocity of revolution, might possibly result in this ghost filling the plate.

Mr. Acheson:—I neglected to state the manner in which the disc was timed. I timed it always. Of course, I could not get an exact speed. These are not printed exactly like the negatives. While some of them exhibited a little or no ghost at all, others exhibited a length of as much as a tenth of an inch. Now, we know there is no difference in speed like that.

Mr. Hamilton:—How did you control the speed?

Mr. Achkson:—Well, it was pretty nearly the limit of my power. I had everything ready for the discharge of the jar and held that in waiting until I had the speed up, with my watch before me. I worked with a treadle so that I had perfect control of it and worked it up until I had a revolution of the band wheel of a certain value, of which my note book gives the record in each and every experiment.

Mr. Hamilton:—The photographs were each separate, were they not? For instance, in No. 5, A and B were the photographs taken at different times?

Mr. Acheson:—The A and B spots were made by one spark at the same instant of time. The spot A was produced through a stationary aperture, while the spot B was produced through the moving aperture in the disc. It was a question that I wanted to decide for my own interest to determine whether any vibration that might be introduced in the work would cause any movement of the outlines, and I always had the A spot for that object so that I might have a means of comparison with the spot produced by the moving aperture. I am sorry that these are not an exact print of the negatives, but I presume we never will have them. I do not suppose that they could be reproduced exactly. Even if the plate is good, the printing will disfigure that; it will not always be the same, and the differences between them are very slight at most.

Mr. Lemuel W. Serrell:—I would like to ask Mr. Acheson if he developed the negatives all at the same time; that is, with the same developer and gave them all an equal length of time in developing. This ghost, or tail of a comet, is a vanishing thing and if the development was applied at different lengths of time it would make a different depth to the negative and make a different length to the ghost or to the tail. I would like to know whether they were all developed at the same time.

Mr. Acheson:—I am not a photographer by profession, and in fact I never did any photographing until I commenced this. I never had an instructor. I bought my supplies in a store and used a small hand-book as a guide. I think I spent probably fifteen or twenty dollars in buying developers and plates until I accumulated quite a stock. Although I spoiled some plates, each and every one of them was developed separately, and it was all done after night. None of this work was done by daylight. Much of it was done in the neighborhood of midnight, or later; as a rule, after considerable experience, I developed them until they ceased to show any increase.

Mr. Serrell:—I would like to say that Mr. Acheson got off pretty cheap at fifteen or twenty dollars, for a beginner.

Mr. Acheson:—Well you must remember I had only a spot here to deal with.

Mr. Birdsall:—In taking pictures on glass plates, such as Mr. Acheson took, although these probably do not exhibit that defect to any extent, a glass sensitive plate, pointed at any bright object and a picture taken of the bright object, will always exhibit this halo, and taking a picture through a hole like that, if the source of light were not perpendicular to the surface of the plate, it would exhibit that halo on one side. If the spark was not far enough away from the surface of the plate so that both the holes A and B were practically in a perpendicular line, that property which is called halation of plates, and which is avoided in the paper negatives, would introduce probably an appreciable error, as in taking pictures with, say, a 10-inch focus lens, we get a picture of the sun on a plate about one-quarter of an inch in diameter and there will often be a half inch of this halo around this image on the plate. If we take a picture of the sun through a large hole, it would, in all probability, show more halation on one side than on the other similar to these pictures, although that error may not be introduced in these.

MR. JOSEPH WETZLER:—There is one point that occurs to me in connection with plate 5. Mr. Acheson remarked that when he applied an iron rod in that coil the spark was very much diminished. He attributed this at first to the magnetization, but when he inserted the copper he found the same diminution of spark. He then came to the conclusion that the retardation was due to the energy abstracted from the circuit by electric currents set up within the bar. It seems to me that that result of the equal action of the iron and the copper might have been predicated when we consider that it takes some time for iron to magnetize; in other words, the iron would not, as a rule, magnetize with a current of a static discharge on account of its great rapidity; that static discharge is practically instantaneous, while it takes some time for iron to magnetize, something in the neighborhood of one one-hundredth of a second, if I remember rightly. We might, therefore, conclude that the spark did not magnetize the iron, but was probably due to electric currents set up in the iron.

MR. ACHESON:—I quite agree with Mr. Wetzler in regard to those opinions and they are such as I finally formed. It was wholly due to the wonderful rapidity of the discharge and the want of time for it.

A gentleman brought up a question in regard to the possibility of a production of those ghosts due to the line of the ray of light not being perpendicular to the glass surface. At first I had some difficulty, and thought I had achieved success before I did, owing to the fact that my two sparks and the hole through which they were photographed were not in a direct line, and it caused a distinct overlapping of the outlines of the spark, as produced in the spot, very distinct and sharp, At first I thought it was a difference of time between the two sparks; that the B spark was a little back of A, which was what I wanted, and I was much pleased; but it was not the case; it was simply due to the fact that they were not in line. The relative positions of the A and B sparks are shown in the diagram of the apparatus (p. 91). The A and B sparks are in a direct line. Now, I divided the difference between the apertures so that the line through A and B would strike about midway between them. If I caused it to strike one, it would produce a distinct difference in the spots as can be seen on some of my negatives, although I tried to get them exactly balanced; under no conditions could I get this ghost. If that was due to the causes which were suggested, it should appear in the spot A, but it is not there.

PROF. WM. E. GEYER:—I was unfortunate in not hearing the whole of this paper, and I am not sure that I fully grasped it. I hope I may be pardoned in asking a question for my own information—to what cause, Mr. Acheson, did you ascribe the ghost? I am not sure that I quite caught that.

MR. ACHESON:—It is due to the oscillation of the current and the continuous charging and discharging of the condenser surfaces, which has to go through the space A, as illustrated in these figures, producing a spark continually diminishing in flow and continuing for a certain length of time until that energy is expended in some manner. On the negatives you can see that the spark weakens and fades away until it actually disappears. You cannot give it a definite length. You cannot set an end to it. You do not know where it ends and nothing commences, due to that diminishing of the amount of energy which is oscillating backward and forward in the circuit.

PROF. GEYER:—I know of an analogous experiment which I think proves that the position you take is perfectly correct, and that it is not necessary to find photographic causes for the ghost. The experiment, I recollect, was one made by Prof. Meyer of the Stevens Institute, where a Leyden jar was discharged through the prongs of a tuning fork, at the end of which there was a little pointer which again moved over a rotating cylinder blackened with lamp-black. When the Leyden jar was discharged there was made on that blackened paper one great blotch where the lamp-black was dissipated. At this same distance further on, this cylinder rotating with uniform velocity, there was another blotch, and then there were three and four, and then there would be a moderate interval; then there would come a great group of them, and then another group, so that taking the trouble to count them there were actually several hundred of those discharges recorded, with no possibility of any photographic disturbance on this cylinder, showing that there were at least 700 of these oscillatory discharges.

Mr. Charles Cuttriss:—I would like to ask one point from Mr. Acheson in regard to these diagrams as taken from the negative. You will observe in both of them that there is a full light circle and then a tail. Now, I would ask if that was so on the photograph or was it not a gradual broadening out without any abrupt line between the bright part and the other? It would appear to me as though there was at the first discharge a greater volume of light or a more intense passage of current just at that distance to photograph the true hole and show it across the table.

Mr. Acheson:—If I understand the gentleman correctly he wants to know whether the outline of the aperture in the disc is clearly defined. It is, if that is the question. On the negatives it is clearly defined. You can trace perfectly all those outlines. I think that you are correct in assuming that the first discharge is vastly greater than any succeeding one. That outline is shown on the negative.

The Chairman:—It is suggested that Dr. Garratt would favor us with some remark on the subject.

Dr. Allan V. Garratt:—Mr. President and Gentlemen: I have listened with a great deal of interest to this paper and discussion, and I cannot help wishing that before we go away we may get a few more facts in regard to these photographs, if possible, to clear up any doubt as to their good character, possibly. I should like to ask Mr. Acheson if the plates were all of one make.

Mr. Acheson:—Yes, sir; they were all of one make.

Dr. Garratt:—And all of the same number?

Mr. Acheson:—Yes, all number 26.

Dr. Garratt:—I understand they were developed practically until they would not develop any more?

Mr. Acheson:—Yes, sir; as near as I could tell.

Dr. Garratt:—I have occasionally monkeyed with plates myself. I see no reason under those circumstances to suppose that the trail of light, which I think a photographer would not call a ghost, is not correct. I do not see any elements of error there. In regard to the speed of rotation, some of these are only half the length of the others. I do not know with what degree of regularity Mr. Acheson can revolve a sewing machine wheel, but I should suppose that he would be able to eliminate an error of fifty per cent.; that is, if he worked it as hard as he could, and if he was in equally vigorous condition during all of his experiments, I see no reason to suppose that a very considerable degree of error was introduced there.

I cannot help feeling, gentlemen, that this paper will lead to a good deal of discussion and investigation on this extremely interesting subject. I believe that it is one that we shall hear a good deal more of.

Mr. E. T. Birdsall:—My suggestion in regard to the spot of light showing the entire circle of the hole and then fading away, is that the first spark of the continued series of sparks which formed the discharge might be the one which has the most energy in it in the form of light due to conditions of the air, such as burning the dust in between the poles, and the succeeding ones might have as much energy in them, although not so much of that energy in the form of light, but more in the form of heat or other forms of energy.

Mr. Serrell:—I would like to ask Mr. Acheson which way his wheel turned around; whether the tail was due to the wheel gradually cutting off the light on the plate and the spark being produced by duration, or whether the first bright spot was due to the first flash and the wheel went in the direction of the tail.

Mr. Acheson:—When you look at this sheet, assuming that the disc is between you and it, the disc was revolving in a direction contrary to those poles, and by the photographic process they became reversed here; the aperture moved in the same direction in which the tail faded.

Mr. Holbrook Cushman:—I would like to ask Mr. Acheson whether the photographs through his moving disc were always photographed through a permanent diaphragm.

Mr. Acheson:—It is the A spark when it was formed alone that produced both spots A and B. If the A and B sparks were together they united and formed these two spots; but the A spark always assisted at least in forming both the A and B spots. When the B spark was produced it likewise formed a portion of each.

THE CHAIRMAN:—This certainly is a subject in which gentlemen in almost all the various branches of the electrical profession have some interest, particularly in the department with which I have identified myself, where we find these antagonistic currents in our relays, in our cables and in our wires. It causes us a good deal of trouble to find just how they act and also to overcome their bad effects, particularly in relays where the signaling current, passing through the various convolutions, arouses this antagonistic current and causes relays to act very slowly. It changes the character of the signals entirely. I think probably, as suggested, it occurs in dynamo machines. This current, in fact, has a great deal to do with almost every electrical apparatus. I do not see how you can avoid it. I do not know that I am ready to express any opinion as to the basis upon which the able lecturer has given his opinion to-night. I think, however, that he is pretty nearly correct as far as I have reasoned out the thing up to this point.

If you have no further remarks to make upon the subject, the Secretary has some matter which he wishes to bring to your attention.

THE SECRETARY:—Gentlemen, it gives me pleasure to announce that the next paper to be read before the Institute in the regular series will be by Lieutenant F. Jarvis Patten, a Member of the Institute, on February 12th, entitled, "On a New System of Multiplex Telegraphy," with illustrations.

MR. CUTTRISS:—If the discussion on this paper is ended I would take pleasure in moving a vote of thanks to the author for one of the most able papers, and showing the deepest insight, I think, into recent research, that we have had before us for some time.

The motion was carried.

THE CHAIRMAN:—I have therefore very much pleasure in presenting Mr. Acheson with the unanimous thanks of the Institute, for the able and interesting paper he has presented with so much pains and so much care and accuracy of experiment.

MR. ACHESON:—I thank the Institute for this kindness in appreciating a little work.

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Vol. VI. New York, April, 1889. No. 4.

The thirty-second meeting of the Institute was held on Feb. 12, 1889, at the College of the City of New York. The meeting was called to order at 8.30 P. M. by the Secretary, who said:

"The subject first to be brought before you this evening is a paper by Lieut. F. Jarvis Patten, U. S. A., on "A New System of Multiplex Telegraphy." That will be followed by some notes on Mr. Acheson's paper, read January 8th, which will be presented by Mr. Jo. Stanford Brown, member of the Institute, and Mr. Chas. T. Child. These gentlemen have made some experiments, and prepared a brief paper based upon the same, giving reference to experiments which have been made in the past. These notes were prepared at the suggestion of Mr. Martin and myself, as the character of the paper was such that it could not be properly discussed at the last meeting. It will be necessary for you to appoint a chairman to serve for the evening."

On motion of Mr. Crocker, Mr. C. O. Mailloux was voted to the chair.

The Chairman: I do not think it is necessary for me to make a set speech to introduce to you our friend and fellow member, Lieutenant Patten. He is a man who is already very well known to the Institute by the active interest he has taken in some discussions we have had at previous meetings, and he is also very well known from several inventions which have brought him prominently into notice. The system, which he brings before you this evening, is one which I am sure will be of great interest. The subject of synchronism in telegraphy is one which has attracted attention almost from the very first discovery of the telegraph, and you will find that as early as in the "forties," some patents were taken out for methods of attaining synchronism in telegraphy. Our

American electrician, Prof. Moses G. Farmer, I believe, was one who made himself distinguished in that field, as he has in a great many others in which he has been the pioneer, and in which he has been so unfortunate as to be ahead of his time. The subject was possibly brought up too early to receive that consideration which would enable results to be obtained that would be practical. It was necessary, perhaps, that the world should advance, that new principles should be elaborated and that new discoveries of laws, etc., should be made known before we could arrive at a practical application of the methods by which synchronism could be effected in telegraphy. I think the time has now arrived for such applications, and the fact that several methods of synchronism have already been brought forward shows that the matter is forced on our consideration, and that we have a right to expect the success of such methods. You will find that the method presented to you this evening is one which is in every respect worthy of the science.

Lieut. Patten then read the following paper.

A NEW SYSTEM OF MULTIPLEX TELEGRAPHY.

BY LIEUT. F. JARVIS PATTEN.

The paper this evening has a single object, its purpose being to describe the salient features of a system of multiplex transmission by telegraph, and as the system is still in the experimental stage, I shall have more to say concerning devices thus far used wherein practical results have come near to the indications of theory, than as to the actual possibilities of the system in use. The general subject is so familiar to all present that a review of the different methods of multiplex transmission is scarcely necessary. Suffice it to say that the system under consideration falls under the general class of synchronous multiplex telegraphs which depend for their operation upon the absolute or approximately uniform rate of movement of similar apparatus placed at the different stations of a line.

In the interest of those present who may not be familiar with the fundamental principle underlying all systems of multiplex transmission, I will give a brief description, which will be understood by reference to Fig. 9. A line wire is shown connecting two distant points, at each end of which is placed a machine which carries a revolving brush or trailer, which is caused by some form of mechanism to sweep over a circular table of con. tacts; this is subdivided as shown, into a convenient number of segments insulated from each other. Each of these segments is connected to a branch circuit, which includes all the necessary instruments for a single operator sending or receiving. If we consider the trailer motionless and resting on, say, the segment No. 10, then the two operators at the end of the line would each evidently find the main line connected to his sending and receiving apparatus for the time being, and if the trailer remained in the position shown they could hold uninterrupted communication with each other. If, now, the trailers were moved to the segments No. 15, in each distributor, then operators No. 15 at each end would have the line. It will now be understood that if by any means the trailers could be revolved uniformly around the circle, the line would be given in turn to any number of operators connected, in the manner described, to the main line.

Early attempts at multiplex transmission did not go further than this. The line was given slowly first to one and then to another operator, the time that each in turn could hold communication being indicated by a common metronome. Later attempts went farther than this, the idea being to make the trailers revolve at a high rate of speed, moving, in fact, so fast that each pair of operators in turn is connected to the line so fast that he is practically unaware that the line has ever been taken from his circuit.

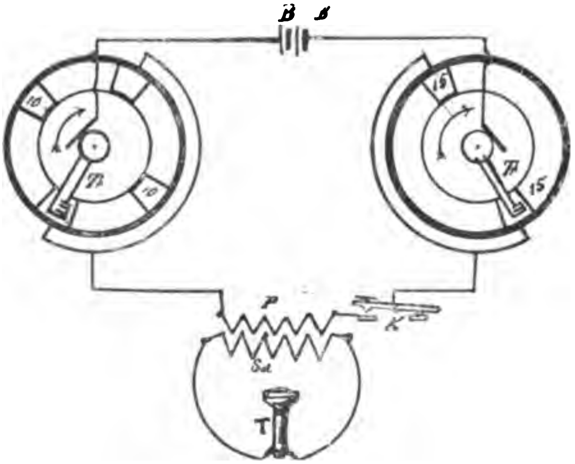


FIG. 9.

It is easy to see that this would not require a very high rate of speed. If it be assumed that the operator can make the shortest signal with his key in one-fifth of a second, it only becomes necessary to move the trailers at a speed of more than five revolutions in a second, when it would evidently be impossible for any of the operators to close a key without having the line during this interval connected to his branch circuit. In order that the signal sent by any operator shall reach the corresponding operator at the other end of the line, it becomes necessary, of course, that the trailer at the two stations should be on the same identical segments at the same instant of time. The solution of this problem will then consist in causing the trailers, however far apart, to move with absolute uniformity and precision. If this can be done at a sufficiently high rate of speed, then a single wire may be given in rapid succession to a number of operators at each end, and all can simultaneously communicate over the single wire without interruption from, or interference with each other.

Such a system has for its object the utilization of all the currents which in a given unit of time can be distinctly transmitted over a single wire. If it is assumed that in average Morse transmission five pulsations of current or makes and breaks of the circuit occur in every second, then if n represent the number of separate currents that can be made to succeed each other during this interval of time $\frac{n}{5}$ will represent theoretically the number of receivers or operators that can work the same wire at the same time.

Any system based upon the foregoing theoretical consideration, which consists practically in giving the line for a brief interval of time to a number of operators in rapid succession, rests fundamentally upon the fact that the identical revolutions in the same period of time of certain parts of the apparatus at different stations can be maintained continuously; in other words, a perfect synchronizing device of some sort must be the fundamental starting point without which any such system will remain what a noted English scientist has aptly described as a "mere figment of the intellect."

In the literature of the telegraph, already large, synchronizing devices are common, and in the books their operativeness is generally taken for granted. All such devices, with a single exception that occurs to me, are founded upon the isochronous motion of some mechanism which, under the operation of natural forces, has a fixed rate of movement, and preserves this motion unvaried so long as these forces are acting freely. The pendulum and the tuning fork perform their oscillations of great or small amplitude in the same period of time; their rate of movement is, therefore, described as being *isochronous.* Two such exactly similar devices while in motion, therefore, should be in *synchronism* with each other; but all these devices once set in motion immediately tend to a state of rest, and it becomes necessary to apply constantly some extraneous force to keep them in continued operation; they must continually receive an intermittent impulse to prevent them from coming to a state of rest. The natural rate of movement of the apparatus, which is alone the result of the uninterrupted action of the forces in the system, is thus made subject to a slight but constant disturbing effort.

In view of these considerations, which have been expressed in the most general terms, it has been my endeavor to perfect a synchronizing device that should be independent of the uniform rate of motion of any part, the general statement of the problem being to devise apparatus which shall move in unison with each other at a variable instead of a constant rate of speed. Recent electrical inventions point clearly to an ultimate solution of this problem, if it has not already been achieved. A pertinent illustration of this fact is found in the common speaking telephone, so common that variable synchronism is already in daily use among those who regard matters electrical as full of mystery. In this apparatus the diaphragm of the receiving instrument moves in perfect synchronism with that of the transmitter, and it seems by no means an exaggerated comparison to regard the receiver as an alternating current motor moving in perfect unison with the changes of potential in the generator and the line circuit. My experiments in this field were first directed to the application of this peculiar relation of the alternating current generator and motor to a synchronizing device, only to reach the conclusion,

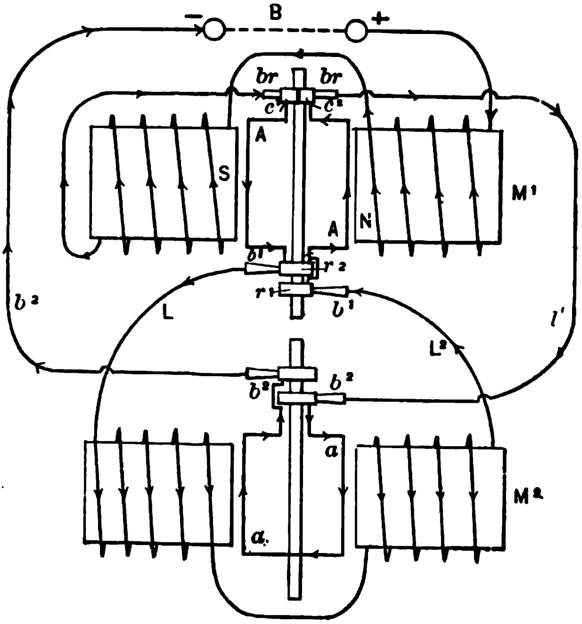


FIG. 1.

however, that it could not be made operative on a circuit of any considerable length.

Further experiments led to a simpler solution of the problem involving few uncertain elements.

I had constructed two ordinary direct current motors of the simplest form, an ordinary two pole field and single coil armature conveniently arranged for use as either direct or alternating current machines. The armature circuits were connected at one end to an ordinary two-part commutator and at the other to two insulated ring contacts. They are shown by a simple diagram in Fig. 1, where both are connected in a single circuit. The armature of the machine M^1 was normally open at the ring contacts, and could be closed either by connecting the ring or through an external circuit. Such a circuit was made to include the field coils of the machine M^2, and the armature of this machine was connected in the direct current circuit from the poles of the battery. Thus connected, the two machines moved as one; the machine M^2 following all the fluctuations of speed in the machine M^1, as if their armatures were carried by the same spindle, the speed being varied between wide limits. To convert this mechanism into a synchronizing device in such a way that the machines could be placed at a great distance from each other was another step. As there was evidently an alternating current in the external loop connected to the armature of the first machine, the reversals of current in this circuit must correspond in time to the half revolutions of the armature of this machine, and an ordinary polarized relay connected in the loop would vibrate in unison with the half revolutions of the armature in the machine M^1. The second machine could therefore be made operative at a distance by simply connecting another relay in an independent line circuit and causing the latter to reverse the current in the second machine. This arrangement, which constitutes the fundamental synchronizing device, is shown in Fig. 2. Here the first machine placed at station X has a polarized relay connected in the external armature circuit, and the relay vibrates in response to the reversals of current in the armature of this machine, which correspond accurately to the half revolution of the machine at X. A line circuit extends from the fixed point of the relay vibrator to the distant station Z, where it includes the coils of another relay and returns to the middle point of the line battery L B at X, the opposite poles of which are connected to the contact stop of the relay at this station. From these connections it results that any relays in the line circuit will be actuated by reversed currents in unison with the half revolutions of the machine at x. At any distant station, as the one shown at z, another machine has its field coils connected as follows: One terminal to the fixed point of the relay and the other to the middle point of a local battery, the opposite poles of which are connected to the contact stops of this relay, the armature of this machine being connected through ring contacts and brushes to an independent source of direct current. Connected in this manner, the two machines still move in perfect unison, even at variable speeds, though separated from each other by any line resistance through which a polarized relay can be made to act. With a single coil armature the driven machine is at times quite erratic, as it can evidently move in either direction, and I have seen such machines reverse instantly at a speed of several hundred revolutions a minute, and go apparently as fast in the opposite direction, and this even when provided with a flywheel of moderate weight.

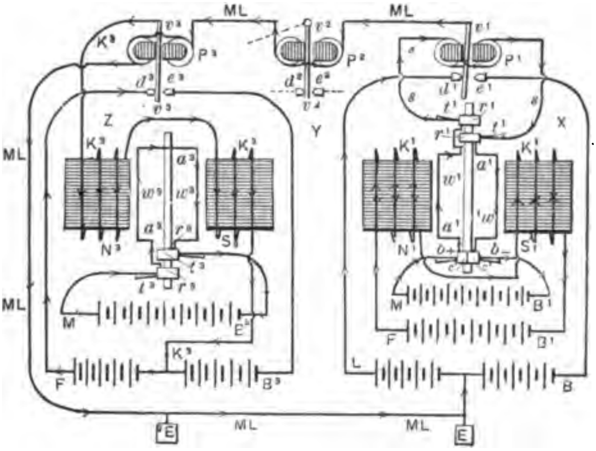


Fig. 2.

This difficulty may be overcome in a variety of ways; an effective one is shown in Fig. 3, which represents a form now given to the armature of the driven machine. It has two coils in series connection, but wound with their poles at right angles to each other. This armature, being placed in a single field, I had supposed would move at only half of the speed of the first machine; but, on the contrary, the synchronism was perfect, the motion was

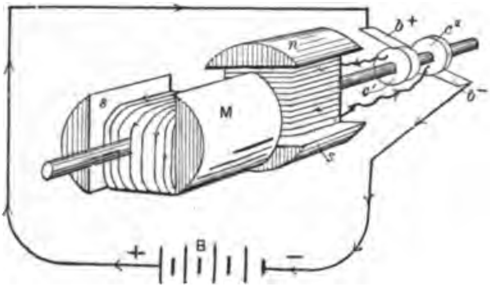


Fig. 3.

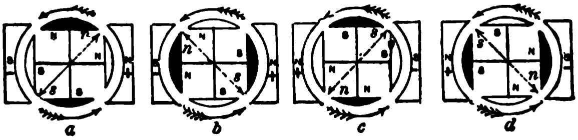


FIG. 3A.

continuous in direction and it has never been known to reverse. The analysis of its action is shown in Fig. 3A, where the armature is represented in the four successive quadrants of a single revolution, the black and the white pole pieces representing the ends of the two coils in their different positions. The dotted lines indicate the position of a pair of resultant poles which must lie diagonally between the actual poles; this diagonal line represents

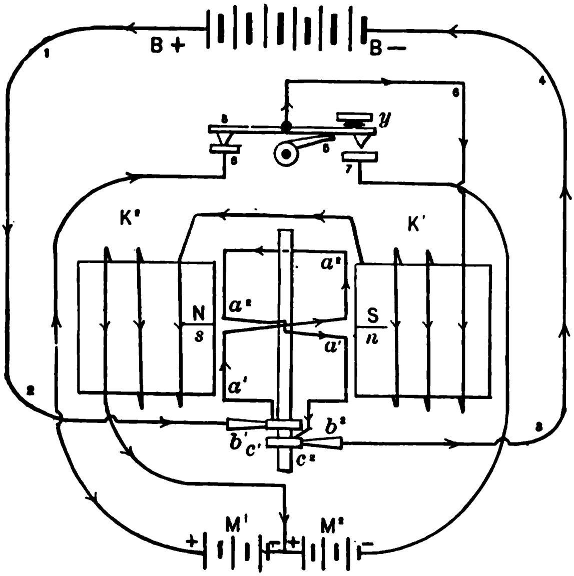


FIG. 3B.

the resultant polarity of the armature and it is evident that neither of its actual poles could be in a position of stable equilibrium while in line with the poles of the field; the resultant armature pole is therefore made to follow a changing field that moves through 180 degrees at each reversal of the current.

Fig. 3B illustrates an experiment made in determining a suitable form of armature. A two point key was used to reverse the field intermittently, the armature being supplied with an independent direct current. Even with this irregular alternation of the current the motion was still continuous. Another method of preserving absolutely steady synchronism is shown in the sketch Fig. 8. It consists in providing the driven machine with a single coil armature and a multipolar field; thus arranged the armature passes through a smaller arc of the entire circle at each reversal of the field; but while the motion is steady it has the evident disadvantage that so high a speed cannot be obtained with the same number of reversals in a given time.

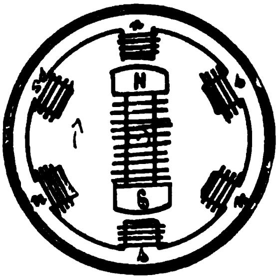


FIG. 8.

Such are the main features of the synchronizing device. There are numerous details which cannot be treated within the limits of a single paper. The uniform motion of distant machines is secured at rates of speed that may vary within considerable limits and synchronism is maintained independently of the isochronous motion of any device or moving part, all the machines moving in unison with a governing motor which sets the pace for all the rest. The controlling machine may evidently be any form of direct current motor carrying a revolving pole changer.

A practically constant speed is of course necessary in any application of this device to the telegraph, but as an illustration of the variable synchronism it may be stated that in a continuous run of twenty-five hours the speed of three machines connected by a single line circuit has varied uniformly between 100 and 1,000 revolutions per minute. Fig. 4 illustrates a modification of the original system in which induced currents of high E. M. F. are used as synchronizing currents. The armature loop of the governing machine is connected to the primary of an induction coil, the secondary to the line circuit at distant stations, and the impulses are reconverted and caused to actuate a polarized relay as before.

A simple experiment made to determine the accuracy of the synchronism will be explained, as it led directly to a separate application of the system to the transmission of signals. The connections are shown in Fig. 9. (p. 112.) From the battery B a line extends to the spindle of the two machines, each provided with segmental distributors of 48 parts. From one such segment in the one machine a wire was taken to a segment of the other machine, the primary of an induction coil and a key being connected in this line, the secondary of the coil was connected, as shown, to an or-

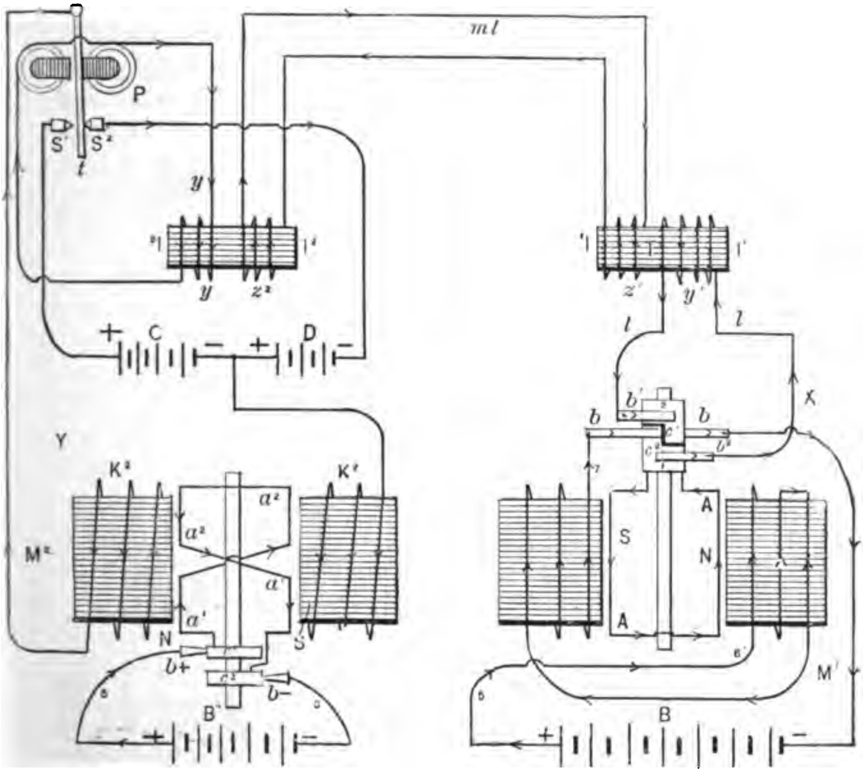


FIG. 4.

dinary telephone receiver. If the machines were moving in unison, the circuit of the primary would be completed whenever the revolving trailers were simultaneously upon the connected segments in the two machines, and the key was closed. If either trailer were slightly in advance of, or behind the other there could be no primary circuit formed on closing the key, no induced current, and no sound in the telephone. The machines were driven at variable speeds, and the wire at one machine was moved from one segment to another until the particular one was found that corresponded to the one selected in the first machine; the telephone then responded accurately to every revolution, thus proving the synchronism perfect within less than the breadth of a single segment. Other wires were then connected to other segments with a like effect.

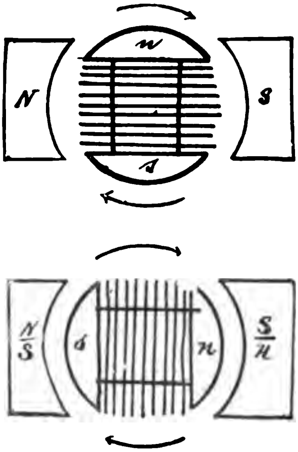


FIG 10.

A singular and unexpected result was deduced from this experiment. Evidently the one machine might start into synchronism at any point of the circumference, the trailers making any angle with each other, as the one shown in the diagram; a revolving switch had been provided which was so arranged that by turning it, a wire could be moved from one segment to another and so adjust the circuits independently of the relative positions of the trailers. The machines were repeatedly stopped and started again, but came again into synchronism always at the same point, the wires never requiring any adjustment, establishing clearly the singular fact that the driven machine always went into synchronism at the same point and could, therefore, be made to start in at any point of the distributor by a proper adjustment of the trailer on the spindle. The reason for this may be found by considering the relation of the forces as expressed in the diagram Fig. 10. Here the same machine is shown with the armature in two positions differing by 90 degrees, the first being a position of

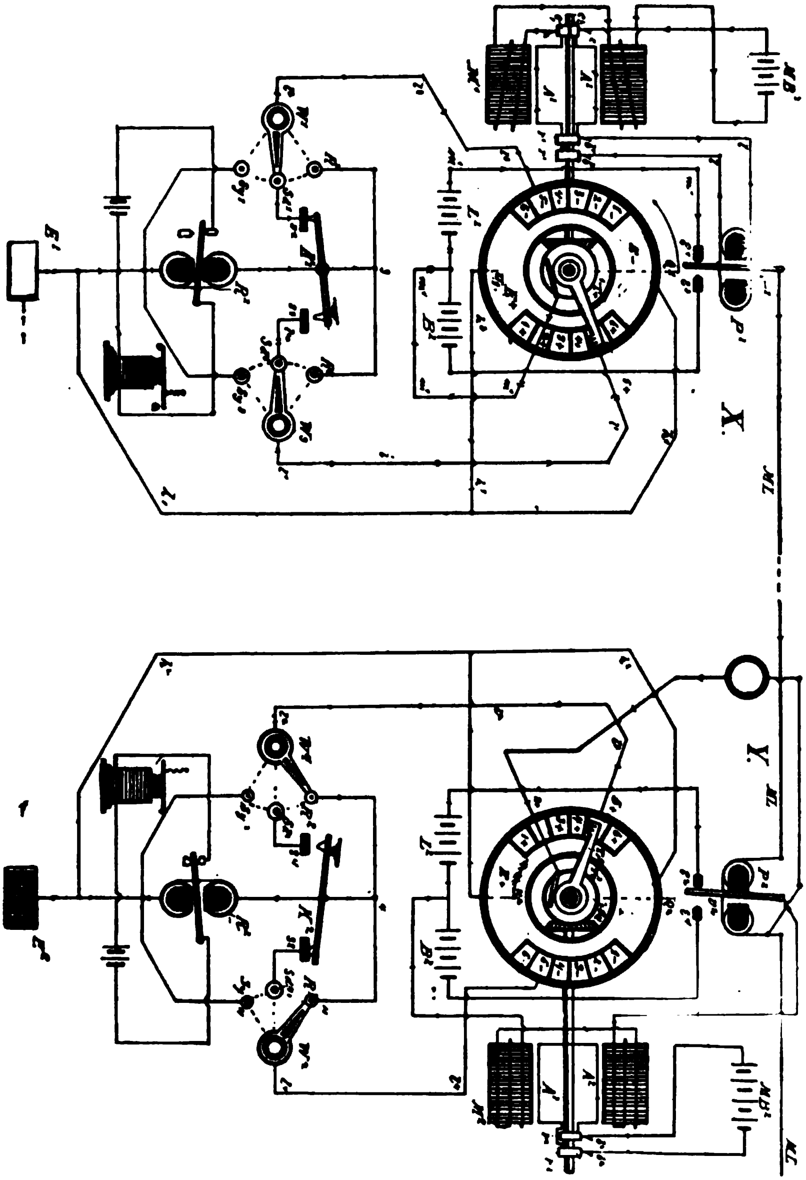


FIG. 5.

maximum effort, while the second is a minimum as the field poles are changing to the opposite designation indicated by the lower letters. As the machines are provided with fly wheels of considerable weight, the point of maximum effort shown in the upper diagram is the one at which the machines most readily come into synchronism with each other, being the point of least opposed mechanical resistance. In Fig. 5 is shown the simplest adaptation of the synchronizing device to a system of telegraphic transmission. The motors X and Y are connected to the line M L, as shown in Fig. 2. The synchronizing current is constantly maintained upon the line and is made to serve also for transmission of signals. It will also be observed that as the armature of the relay at X puts opposite halves of the line battery to earth alternately at each half revolution of the machine at X, one-half of the circumference of the segmental distributor corresponds to the transmission of positive, and the other to the transmission of negative currents to line. Thus, while the trailer is sweeping over one-half of the distributor, one part of the line battery L B is connected to line and earth, and while traversing the other half of the distributor the other part of the battery will be in circuit, each in turn sending a succession of impulses of opposite polarity. Some arbitrary lines as $Q^1 U^1$, may therefore be drawn diametrically across the distributor at X, and on one side of this line, positive, and on the other, negative, currents will be sent to the line wire, and by making the distributors movable about its axis the line $Q^1 U^1$ may be made to fall within the broad spaces $E+$ $E-$. The distributor is composed of two parts, one designated $E+$, and $E-$ is connected directly to each; the other consists of a series of insulated segments 1, 2, 3, etc., in opposite quadrants of the distributor. Whenever the trailer is on the part $E+$, $E-$, the battery has a free path to earth and line through the trailer TR^1. The insulated segments are connected to the local branches in pairs, each local branch having at least two such segments diametrically placed in opposite halves of the distributor. The connections and instruments in circuit are alike for all the branches and are therefore shown in detail for only one.

Each branch—say that of operator No. 5—has a double line to earth taken from opposite halves of the distributor as those shown at 5 + and 5 —. These branches are connected to a three point switch W^1 and W^3, which may be turned to R^1 and R^3 for receiving, or to S^1 and S^2 for sending, thus throwing the key in circuit, or sy^1 and sy^2 for synchronizing, thus leaving the sending and receiving apparatus out of circuit. An ordinary reversing key is used, the relay circuit being connected to its middle point. The drawing represents station X sending and Y receiving. Thus, if the key be depressed, as at Y, so that its front contact puts the line to earth, intermittent pulsations of one polarity will go to line, corresponding to the contacts made by the trailer with the segment pertaining to this branch on the right hand side of the distributor at X, while the negative currents, corresponding to the left hand half of the distributor, will be cut out, and the corresponding relay at both stations will be actuated by currents of constant polarity. If, now, the key be reversed, the trailer, on reaching the opposite segment, 5 —, will send an opposite current to line and earth, causing the polarized relay to reverse in accordance with the motions at the key; hence signaling may be effected with a sounder connected in a local circuit as shown. Thus, if the key were closed at both the front and back contacts at the same time, the receiving relays would all vibrate in unison with the synchronizing relay in the line circuit and transmission is effected by cutting out all the positive or all the negative impulses, according to the position of the key, whether on its front or back contact. With a slow speed the contacts connecting any branch to the main line must of course be increased to insure certainty of action; but with a sufficiently high speed few contacts will be required. In the foregoing system, the synchronizing current which is only used as such at two points during each revolution, is made to answer for signaling purposes also.

There are, of course, numerous details incident to such a system that cannot be dealt with in a superficial description. Each operator's circuit being made up of a number of rapid contacts with the main line, each one of the ordinary Morse characters is necessarily made up of a number of pulsations of current, and the serious problem has been to construct a relay that would respond to these broken currents. A single polarized relay has a tendency to leave its contact during the breaks. A number of devices have been made to correct this. Fig. 6 shows by a diagram of circuits a modification of the system of single wire transmission in which superposed currents of high tension are used to actuate receiving apparatus, thus making them independent of the synchronizing current. The key is so arranged as to open and close the primary of an induction coil, the secondary of which is connected to line. A telephone or similar receiving apparatus arranged to respond to currents of high E. M. F. is connected

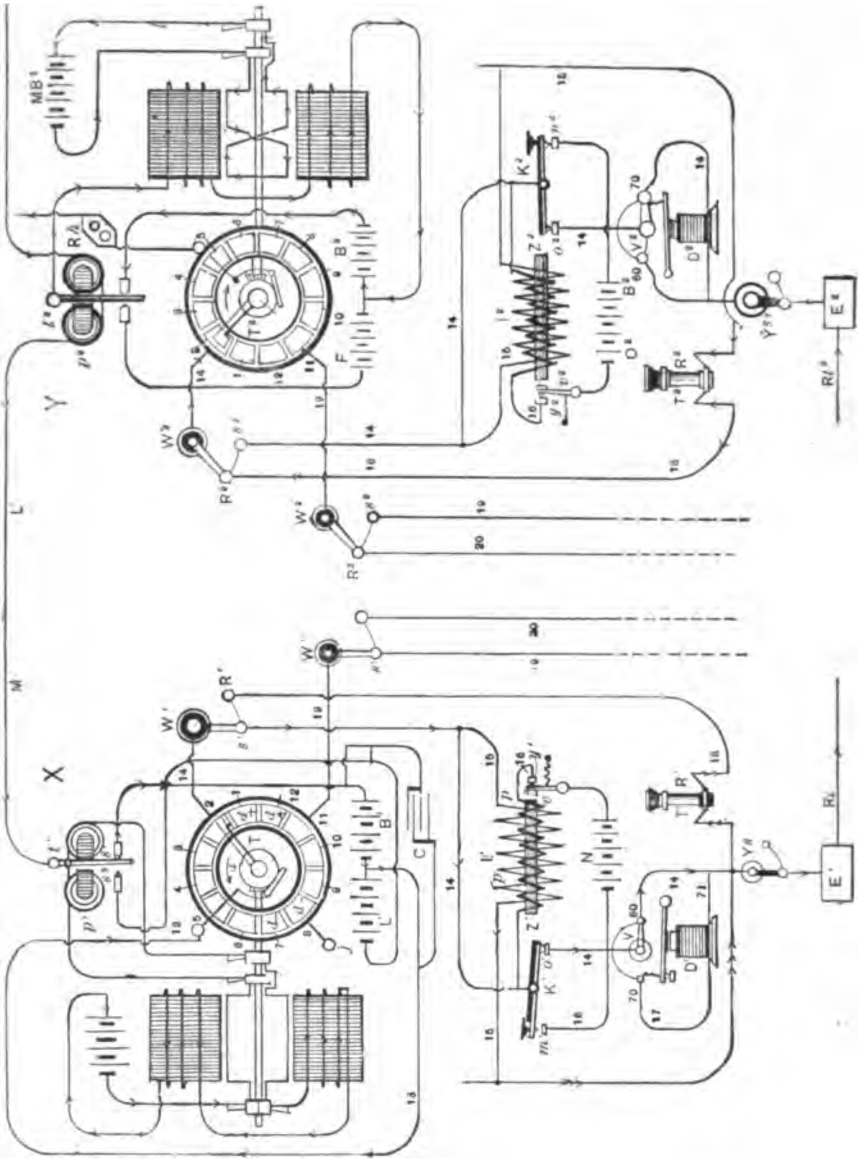


Fig. 6.

with a receiving circuit instead of the relays. With such a system of transmission a higher order of multiplex transmission can be obtained than with ordinary receiving apparatus, and it requires so little current to actuate these instruments that each operator can work a circuit with a far less number of segments. As the trailer makes contact with any particular segment there will be of course two currents sent, due to the make and the break. In Fig. 7 is shown an arrangement by which the first of these is cut out. The primary of the induction coil connected through a separate trailer and series of segments is closed by the key before the trailer connected to the line circuit has reached its segment. The induced current due to the break only, is then transmitted to line. This device is shown in Fig. 7, and as there indicated it has also been made to work polarized relays suitably wound for these high tension currents.

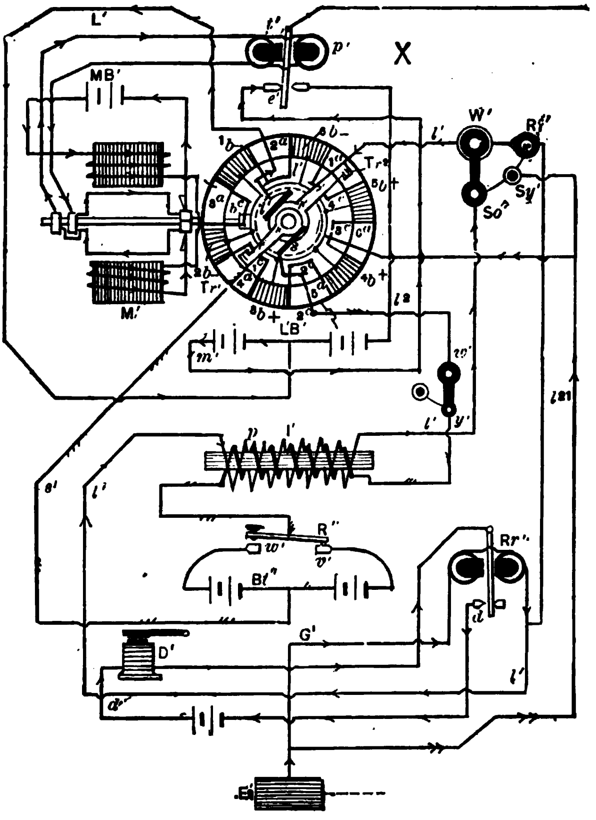


FIG. 7.

The general subject as a system of telegraphy will be recognized as a rather extensive one for a single paper. I have given the details of a few of the more decisive experiments that have been made, and I have confined the paper to what has actually been accomplished. With reference to practical results on a line test I shall have more to say at a later day. A technical paper, at its best, is uninteresting, and the present one is already long. In closing, I will answer a pertinent question that may occur to many: "Of what use is a system of multiplex telegraphy?"

The answer is this: We live in an age of industrial advancement. People are no longer satisfied with what is good enough. They want the best. The best rail for roads must be made of steel, and, this fact once established, iron was no longer suited to the requirements of travel, and iron roads have been turned into steel ones. The steel rail bears the same relation to iron in the systems of transportation, that the copper wire does to the iron one in systems of telegraphic communication, the higher rate and more reliable service required demanding already in a large degree the substitution of copper for iron wires on the main lines of business. With a good copper wire and a reliable system of multiplex transmission, a single wire from New York to our principal cities would meet all the requirements of business, and it is no exaggeration to say that with these conditions given, the existing system of intercommunication by the telegraph could be duplicated at a cost of one-tenth the present amount of capital invested by either the government or private enterprises. That is what copper wire and multiplex telegraphy mean, and it is doubtless the reason why so many find the subject worthy of their best endeavors.

DISCUSSION.

THE CHAIRMAN: The subject is now before you for discussion. It is rather to be regretted that our eminent telegraphists did not choose to put in an appearance this evening. There are a number of men in the telegraphic community who would have been able, no doubt, to give us some very interesting details and to ask a great many pertinent questions about this. It certainly is a very interesting subject. It is, perhaps, not too much to say that it is the dawn of future progress in telegraphy. For a long time, telegraph authorities have been of the opinion that the day of monotelegraphic work is past; that the cost of using the telegraph circuit and maintaining it is so great that we must be able to utilize it and multiply its use by making it answer for more messages than one at the same time. So that in bringing before us this interesting paper, Lieut. Patten has unquestionably given us something which represents the beginning of the future, and you certainly should not lose the opportunity of giving it a thorough discussion. Mr. Pope, you are an old telegrapher, can you not say something on the subject?

MR. R. W. POPE:—I was a telegrapher before we had any of these things. I notice Mr. Prescott here and Mr. Delany. Mr. Delany has done something in this line.

THE CHAIRMAN:—I must beg Mr. Delany's pardon for not having recognized him; I hope he will let us hear from him on this subject.

MR. PATRICK B. DELANY:—I have to compliment Lieut. Patten on the very clear and entertaining discourse he has given on a very interesting subject, and a subject in which I have had some little experience. I do not know that I can ask any questions that will throw any further light upon it, but as I was a telegrapher about the same time that Mr. Pope was in the business, perhaps we might agree upon some inquiries that would be interesting to us both. I would suggest one by asking Lieut. Patten, how many contacts to the line he proposes to give for each Morse circuit, so as to make the circuit sufficiently continuous for the manipulation of the average Morse operator. I didn't catch clearly the number of pulsations per second that he proposed to apply to each branch circuit with his distributors.

LIEUT. PATTEN:—The question is certainly a very pertinent one, and one which cannot yet be answered with all the definiteness I would like to give it; but, in general terms, I would say the number of segments given to each operator necessarily depends upon his speed. I would simply say that we have four contacts for a line. We have made a sounder work with clear action upon an artificial line, of course, at the rate of 30 words a minute. There are two things which determine the number of contacts to be given to any operator in the circumference of a distributor; one is the speed at which the trailer is driven and the other is the amount of time that the trailer rests in contact with the segment. Four contacts give a good reliable result; that would give about 1200 pulsations per minute.

MR. DELANY:—You say that the number of contacts that each operator would have, as a matter of course, depends on the number of revolutions of the distributor. Not necessarily so, because your distributor might have a very slow speed and make several contacts per revolution for each operator.

LIEUT. PATTEN:—I say it depends upon the rate of the trailer in a given unit of time.

MR. DELANY:—If you increase the number of segments in a circuit connected to any operator's instrument, of course, with a very slow motion of your trailer, you would get the same number of contacts. The object of my inquiry was to ascertain the number of contacts that could be given to each operator, and at the same time afford the necessary duration of contact between the distributing trailer and each segment, and enable you to transmit currents over a line of a considerable resistance.

LIEUT. PATTEN:—This is a refinement of the subject which can only be determined by prolonged experiment. I would say that with a few thousand ohms resistance we could get lower than 1-400 or 1-500 part of a second from a single contact.

MR. DELANY:—You say that with 1200 contacts a minute, you get a sufficiently continuous circuit?

LIEUT. PATTEN:—Well, so far as our experiments have indicated, which are on an artificial line.

MR. POPE:—It is my impression that Mr. Delany's experience does not quite coincide with that of Lieut. Patten, in regard to that contact, and I would like to hear what his experience has been as to the number of contacts per second necessary to permit of fast Morse manipulation on long wires.

THE CHAIRMAN:—Mr. Delany, would you kindly give us some of the results of your experience, and I think it would be particularly interesting if you would tell us your experience as to the maximum number of pulsations which can be maintained on a line of a given length, and having a given static capacity; *i. e.* the number of impulses per minute before the static charge interferes to any great extent.

MR. DELANY:—My experience would lead me to believe, not to state it exactly but in a general way, that on a circuit of, say, 100 miles, from New York to Philadelphia, for instance, with perhaps 1,200 to 1,500 ohms resistance, and an iron wire of a capacity, probably, of 1½ or 2 microfarads, that about—I will have to make a little calculation to aid me—that about 500 impulses per second can be transmitted over such a line and received on segments corresponding in position in the circle on a circuit table, provided the trailers and the distributor are in synchronism and allowing a slight moment of time for the retardation of the circuit. If the retardation amounts to one 500th part of a second then the current sent at one segment in a particular position in the circuit will be transferred to the segment next in the direction of the revolving trailer, and especially so, if the line is not cleared of its static charge. After each impulse has been sent into the line, the charge that is in the line delays the next initial impulse that goes into the line and delivers it in a segment still further removed at the receiving end. I have found that in the operation of what we call good Morse circuits, it requires at least 35 or 36 impulses per second to make a smooth circuit, so that the operator manipulating the key, as is frequently done, at the rate of 12 to 15 times in a second, may transmit to the full measure of his ability, or say, forty words a minute.

THE CHAIRMAN:—Of one polarity?

MR. DELANY:—Of one polarity, or with a reversal of polarities you would get about the same result. With that number of contacts in a second, I have seen more than forty words transmitted for a considerable period of time continuously, say, for half an hour over circuits about as I have described. In order to do that it requires a circuit that varies not more than 1-1000 part of a second. If there is variation in excess of that the currents become so weak that the relays would go out of adjustment or it would be very sensibly felt at the relays at the receiving station.

LIEUT. PATTEN:—Mr. Delany has remarked upon one feature that it might be of interest to mention in connection with this system. I might have put it into the paper, but as I have said, I did not care to go into detail. There is an essential difference between the system as shown here and any system that is founded on the isochronous motion of different moving parts at different stations. For instance, in figure 5, if the apparatus were driven by isochronous machines which were absolutely synchronous, the two trailers would be inclined at exactly the same angle of position at the same instant, and if an electric impulse were sent over the line, and it took any time to reach the other end of the circuit, it is evident that the trailers would have left the position where they were when the impulse started. Whereas, in a system of this character where variable synchronism is maintained we have what you might describe as transmitted synchronism. The trailer is just as much behind the initial one at the receiving station as the time required for the current to traverse the line. These refinements are somewhat delicate, but if there is such a distinction, it is evident that we have more chance of getting the current out upon its proper segment in a system that is variable than in one that is isochronous.

I merely bring this point out to show the difference between synchronism independently maintained and that maintained by the circuit. I would like to ask Mr. Delany what E. M. F. he used for the circuit he describes of 100 impulses, working Morse instruments.

MR. DELANY:—About 75 volts we used on the circuit that I referred to. I mark the distinction that the Lieutenant wished to draw between synchronism that is maintained by machines that are not isochronous in their movements, and that of the synchronism shown on the board maintained by transmitted impulses. I would say, that the synchronism with which I have had experience, is also maintained entirely by transmitted impulses, i. e., the synchronism is not maintained in any way by the isochronous method independent of the two machines. One machine is corrected in its movement by impulses transmitted from the other end of the line, the receiving machine is corrected by the transmitting machine, and vice versa. The impulses are sent in opposite directions at different periods of the rotation of the trailer, so that, if the current is sent over segment No. 1, we will say, at station X, and if it takes the time represented by the distance from No. 1 to No. 2 segment, to reach the receiving end, then the correcting impulses which keep the instruments in synchronism will maintain that relation and position of the trailer. We have found it necessary in practice to maintain the instruments in synchronism on corresponding segments where it was necessary or desirable to telegraph in opposite directions simultaneously. Because, if segments at each end of the line are connected to operator A, and one operator is sending to the other, the instruments being in synchronism, the time of the impulses over the line is represented by the time of the trailer from one segment to the other; it will readily be understood that if A be sending to B and the instruments are in synchronism for that condition, then, if B should turn around and send to A, A would not receive the impulses that were sent to him. Consequently, we have to synchronize on corresponding segments if we wish to work in opposite directions simultaneously. If the transmission is always done in one direction, then, as you can readily see, it makes no difference. The time of contact of the segments must correspond to the time of transmission on the line in order to transmit simultaneously in opposite directions. But where the time on the line is such as to make the transfer more than the margin allowed, (*i. e.*, the variations of the instruments on the face of corresponding segments,) you cannot work in opposite directions simultaneously.

Mr. C. J. Kintner:—I desire to call attention to the fact, which Lieut. Patten appears to have overlooked in his very good description of his apparatus, that it is, so far as I am aware, the first apparatus by which isochronous movement is effected by the apparatus itself. In the old Casselli system, which is perhaps the first isochronous telegraph with which we are familiar, the isochronous movement is brought about by the movement of a pendulum. In Mr. Delany's system, with which we are all familiar, it is done by the harmonic principle. In Lieut. Patten's system, it depends upon the peculiar phases of the alternating current generator and an alternating current motor. It was my good fortune last summer to make a somewhat extended set of tests and experiments of Lieut. Patten's apparatus as to its efficiency and as to the isochronous movements of the apparatus and I found that the apparatus moved uniformly in synchronism, and during the experiments, a very interesting feature came out. We were using six sets of keys at each end of the line and I was testing first with all the keys closed; then one key closed at each end, and successive keys and various numbers of keys, to see that there was absolute synchronism. I discovered that when key No. 1 at section No. 1 was closed at each end, that there was a faint response on section No. 2. I called Lieut. Patten's attention to it for the reason that I could not understand why it was so. At last upon close examination of the brushes I found that they were set slightly at an angle, so that as the brush was passing off section No. 1, it was at the same time also on section No. 2, so that we had a response on the other section, which was an absolute proof of the synchronism of the two sections. Lieut. Patten is making a sketch which will show this. I desire to call attention to the fact that the apparatus is capable of operation, as the Lieutenant has advised me, with an alternating current, in which it is possible, as I understand, to transmit messages over longer lines by reason of the fact that with an alternating current he is able to discharge the line more rapidly.

Mr. B. C. Batcheller:—I would like to ask how many circuits are worked at one time?

Lieut. Patten:—In answer to that question I have simply to say that our work was entirely experimental, and I am not at liberty to state what the capacity of the system is, as it has not been tried in such shape that I should be warranted in stating it.

THE CHAIRMAN:—There is one practical question, I think, which follows from the observations made by Mr. Delany, and while he was speaking I rather misapprehended his statement. Mr. Delany states that on each pair of instruments you have some 35 pulsations per second, in order to get Morse readings. Now it so happens that in Mr. Delany's system he has the control of making these impulses of either polarity, because the battery is used at either end of the line and can connect either pole to the ground. If I understand the Lieutenant's system rightly, the polarity of the impulses is alternating necessarily. Consequently, out of 35 impulses given to the pair of instruments, 17 of them will be of one polarity and the rest of them another. The question then occurs whether that number would be sufficient, or whether in reality it would not be necessary for him to have 35 of each polarity in order to effect good Morse readings?

LIEUT. PATTEN:—I have to say in reply that with our artificial line work, which is all we have done up to date, we have found that a far less number than 35 gives us very good results. In fact, the sounder acts very clear and sharp and is up to a speed of transmission of some thirty words a minute, with some 15 pulsations per second, I should judge as the average amount. It is impossible to tell accurately what number of pulsations there might be within 5 or 10. It varies from 15 to 25, I should think.

MR. FRANCIS B. CROCKER:—Lieut. Patten has spoken several times of variable synchronism. The expression is perfectly correct, but it sounds rather startling to the ordinary hearer, because one's idea naturally is that synchronism is constant. He also makes a distinction between synchronism and isochronism. There is a distinction, of course, but he did not explain it very fully. Synchronism means, for example, that the two instruments revolve together whether fast or slow, whereas isochronism means that they revolve together at a certain speed. With a variable synchronism one may start off two instruments revolving together at 100 revolutions and then raise the speed of both at the same time to 1000 revolutions, but they still run synchronously all the time, just as the two driving wheels of a locomotive which are attached to the same axle, of course revolve synchronously; *i. e.* at the same speed, but they may revolve at 100 or 1000 revolutions a minute. It is in this way that we have a variable synchronism. I considered that the confusion which might arise from this was worth pointing out. The Lieutenant passed over it quite rapidly, because, of course, he is so very familiar with the subject.

THE CHAIRMAN :—That is a very important distinction that Mr. Crocker has made. It would appear that Lieut. Patten's system works both synchronously and isochronously.

LIEUT. PATTEN :—Mr. Crocker's distinction is quite correct. I had intended to bring it out, but perhaps did not do so quite clearly enough, but the distinction he makes is in further illustration of my paper, and I thank him. In working the instruments, we try to drive them at as uniform a rate as possible. I brought out this idea of variable synchronism as a distinguishing feature of the system, not as one that would necessarily be used to a large extent. If machines are running at a good speed, a uniform 100 or 200 revolutions per minute, they are isochronous machines, and of course synchronous also. But if their speed is changed, they are no longer isochronous for the time being, they are simply synchronous.

MR. E. C. DAVIDSON :—With your permission I should like to refer to one point. Mr. Kintner has suggested that the Delany system was based on a harmonic principle. I do not so understand it. I waited for some one to inquire into that, but as no one has done so, I will take the liberty of speaking of it. As I understand it, no question of harmonics is involved. The circuit breaker is employed to control the rotation of the motor at either end of the line. That circuit breaker, it is true, may be a fork, it may be a rotating wheel; that is entirely immaterial. There is no question of harmony about it. As the circuit breaker makes and breaks the circuit at the end of the line, what is after all a small electric motor is rotated, that is, the passage of the pole of a disc or armature in front of a magnet coincides with the break in the circuit; that produces the rotation. Now the character of the impulse sent from one station to another merely modifies the circuit breaker; it slows it or accelerates it, and the two instruments are thus tied together. I do not understand that it is at all analogous to a harmonic telegraph.

THE CHAIRMAN :—I think the distinction is quite right to a certain extent, but probably the word harmonic should have been changed to rhythmic, The action of the instrument in Mr. Delany's system has reference more to maintaining a certain rhythm in the pulsations than to their harmony.

MR. KINTNER:—I accept the suggestion, I simply wanted to convey the idea that in Mr. Delany's system, like Casselli's, the synchronism was maintained by an extraneous apparatus, an apparatus extraneous to the motors themselves. Of course, we are all familiar with Mr. Delany's inventions and know exactly what they do. We are aware that his inventions have wrought a wonderful improvement in multiplex telegraphy, and most of us, I presume, have seen them in operation. They were in operation here at the Institute some years ago, and there is no doubt about their wonderful success. I had no idea of calling them harmonic telegraphs, I merely wanted to bring out that distinction that they were controlled by extraneous means.

MR. DELANY:—I beg to assure Mr. Kintner that there is no discord produced by his allusion to harmonics.

THE CHAIRMAN:—We have with us this evening a gentleman who knows something of multiplex telegraphy in Europe, our friend Mr. Abdank-Abakanowicz.

MR. DELANY:—If you will pardon me one moment, when Mr. Davidson referred to the making and breaking apparatus, I was going to make the description a little more clear, perhaps, by stating that it was an automatic circuit breaker originally, a tuning fork in connection with what is known as the phonic wheel; that is, perhaps, where Mr. Kintner inadvertently got his idea of harmony. My synchronous and my multiplex telegraph are based primarily on the very ingenious contrivances of Mr. Paul La Cour, his automatic circuit breaker in connection with a phonic wheel, an instrument that is moved by very fine increments. It moves in a little space at a time, and consequently has no opportunity for very wide variation before it is brought again to the centre of attraction by those recording impulses.

MR. ABDANK stated that he had never made a special study of telegraphy, but he had examined with great interest the synchronous devices of Lieut. Patten, and was impressed especially with the solidity and great power of the motor running in synchronism.

THE CHAIRMAN:—There is a great deal of interest to be said on this subject, but as the time seems to be passing away, I shall have to pass on to the consideration of the next topic, which is the notes on Mr. Acheson's paper, by Messrs. C. T. Child and Jo. Stanford Brown.

MR. BROWN then read the following paper:

LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

NOTES ON MR. E. G. ACHESON'S PAPER, BY JOS. STANFORD BROWN AND CHARLES T. CHILD.

Having been requested upon very short notice to re-open the discussion on the highly interesting paper on "Lightning Arresters and the Photographic Study of Self-Induction," presented at the Institute meeting held January 8th, the following hasty notes and queries are with diffidence offered for consideration. Truth alone is sought, and it is a matter of regret if any criticism should even tend to disparage the worthy endeavors of one who has so fearlessly grappled a problem fraught with such interest alike to both theorist and practical electrician.

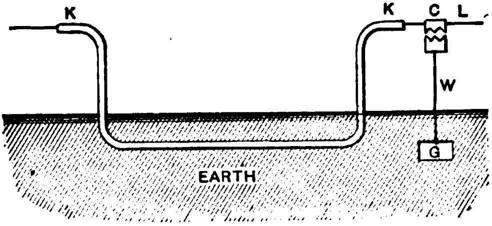


FIG. 1.

After careful reading it would seem as if the subject might be approached with advantage from more than one standpoint, and that even somewhat different conclusions were deducible simply by re-arrangement of the experimental data. We have given, a submarine or subterranean cable, to each end of which at its junction with the main line a lightning arrester is connected in the most approved manner, Fig. 1, to determine why, in spite of these precautions, the cable is not infrequently punctured when an "electrical discharge" enters the overhead system.

For experimental purposes this has been reproduced in miniature, as shown in Figs. 2, 2*a*, 2*b* and 2*c*, by the line L entering the cable K at the point P, to which point is attached the lightning arrester C grounded through the wire W. The "electrical discharge" arises from the Leyden jar J, charged from the Holtz machine M N, and strikes the line at A. The outside of J will represent the earth, to which the ground wire W and the "earthing" of the cable, represented by E, "twisted a single turn" (*Western Electrician*, IV, 47) about the cable, is connected. Is it not possible, then, to throw Fig. 2 of the previous paper into the form Fig. 2*a* for the purpose of drawing attention more directly to the relation of the circuits involved? The discharge, or momentary current, from the jar J entering at A traverses the line L to the point P, where two paths are offered; one, through the lightning arrester C to earth E; a second, in practice, by puncturing the cable insulation. C now being made but a fraction of the insulation thickness, and W a short, thick copper wire, it was found that the cable was still frequently punctured at times when a spark appeared at C, and that the length of W appeared to enter

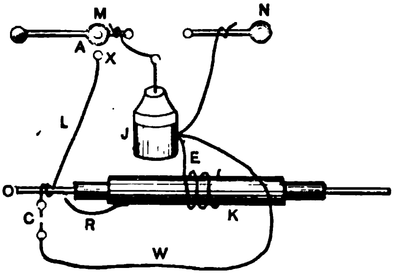


FIG. 2.

A and C constant.
B and W variable.
Capacity of J—.0025 m. f.
" " M and N not given.

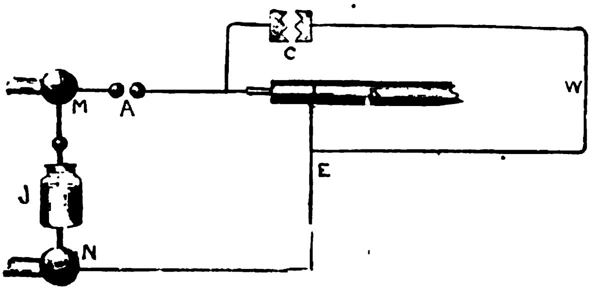


FIG. 2 *a*.

Fig. 2 redrawn.
Case of a cable protected by a lightning arrester.

the effect equally with the space variation of c. *No part of the effect*, it will be noted, *is attributed to the action of the cable as a condenser.*

The problem is now changed to that shown in Figs. 3 and 3a, presenting a divided circuit with the lightning arrester at a constant distance of .02 inch, and the earth wire w long, compared with that connecting the variable b space with the same earth point. May not this arrangement of split circuit with an air space in each branch be looked upon as the general case for which Dr. Lodge's "alternative path" represents the condition of making one of the air spaces zero? What then is the relative efficacy of changing the length of w and the distance of the c points *to prevent* a spark at b which represents the puncturing of the cable insulation, providing the b points are held equivalent to the long length of cable wire at any point of which puncturing may occur?

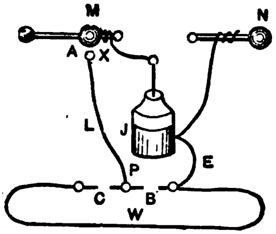


FIG. 3.

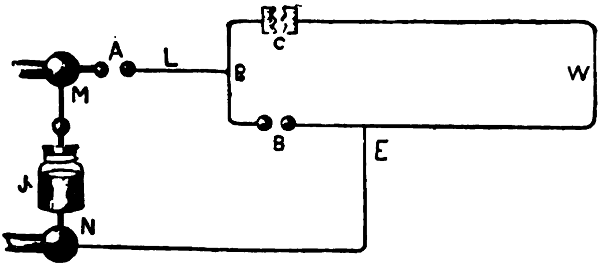


FIG. 3 *a*.

Fig. 3 redrawn.

A constant—0.286."

C " —0.020."

B variable.

W, of copper, .062" diameter, with variable length.

To decide this with a and c fixed at .286 and .02 inch, respectively, b was varied with w to keep the spark ratio of b to a constant at one to ten. Then removing c (*i. e.*, for c equals zero) the operation was repeated, and a comparison of these results is shown in the curve, Plate A. Instead of this procedure, should not b have been made non-sparking for C and A fixed, and W varied until a spark was got at B, and then W left constant for variations of C up to a point where B would spark?

That with the connections of Fig. 4 and W equal 20 feet a spark should pass at B of over $\frac{1}{3}$ inch in length might perhaps have been expected from Dr. Lodge's results (London *Electrician*, XXI, 815), shown here in Fig. 4b, when there is an alternative path in which the self-induction of W (Lodge's L$^{\circ}$ loop) comes into play. True, Fig. 4 is not an "alternative path" of necessity, for the B points may be moved out of the sparking distance.

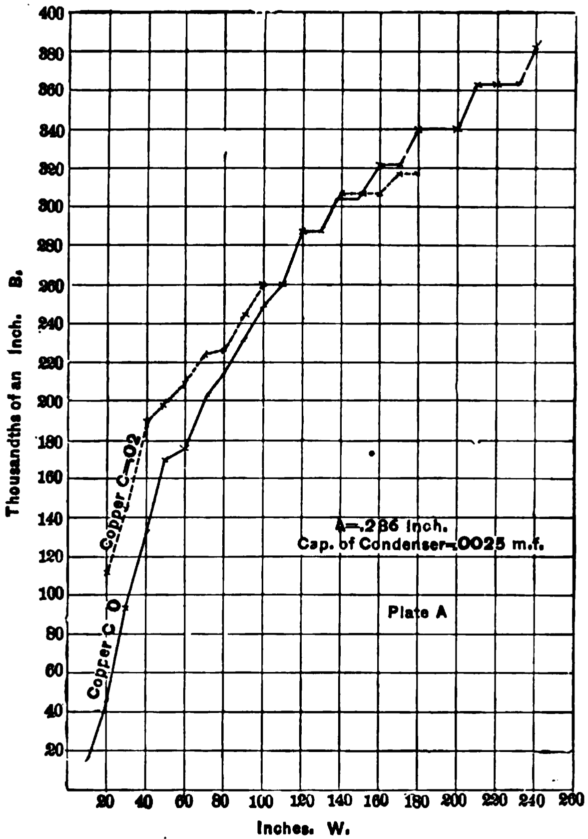


PLATE A.

W is now supposed "electrified" by the charge across A. The "inverse extra current" of making, simply cuts from the charging current; but the "direct extra current" of breaking, *i.e.*, charging impulse cessation, acts as its continuation but at a higher potential (Ganot, 7th edition, 776). This extra current then is running towards E and would naturally be supposed to equalize on the same path as the current generating it. It might therefore be expected to dissipate on reaching earth; the circuit is, however, claimed to close itself through P W E B, or, more clearly, around the closed circle of the L° loop, Fig 4b.

It would be of interest to some in this connection to know how it was ascertained that the instantaneous time duration of the extra current is shorter than that of the charging current; for if this be assumed, because of the known higher electromotive force of the former, the argument would seem to be in a circle.

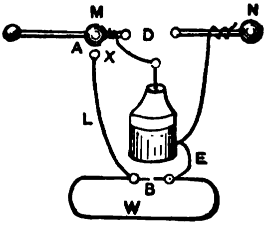


FIG. 4.

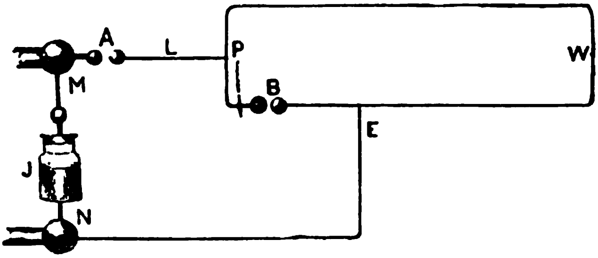


FIG. 4 *a*.

Fig. 4 redrawn.
A constant—0.286.″
B " —0.333.″
C removed.
W—20 feet of copper wire, .052″ diameter.

On the assumption that the extra current dissipates through B, and not in the earth, it is argued that shortening W would tend to decrease this tendency to puncture the cable. To test this we have B equal the insulation thickness, C and W removed, and we get Fig. 2b if the C W circuit be supposed omitted. "With this arrangement it was not found possible to puncture the cable." May not this be regarded simply as a safety short circuit for our cable considered as a Leyden jar? And what is this but a working case of a cable effectually protected by a lightning arrester

B? While the electrostatic capacity of a "short piece of cable" may be negligible compared with that of twenty feet of No. 16 B. & S. wire, it is perhaps doubtful if the capacity of a commercial cable could be so regarded in comparison with that of the ground wire from the lightning arrester.

It is not altogether plain how the direct extra current is an effect of Faraday's electro-tonic state when Faraday says (*Researches*, I, 16): "This state is altogether the effect of induction exerted, and ceases as soon as the inducing force is removed. And Jaquez ("Dictionaire d'Electricité et de Magnetisme," 1887, p. 138) defines it as "the state of the conductor during the interval of time which separates the appearance and disappearance of an induction current." Maxwell, by the way ["Electricity and Magnetism" (2d ed.) II, 174], calls it "the fundamental quantity in the theory of electro-magnetism." Would it not be possible to regard it as an electro-magnetic potential stored in the wire ready to show itself as direct extra current? With the citations quoted above, how can this electro-tonic state exist *before* the electrifying current is discharged into the circuit?

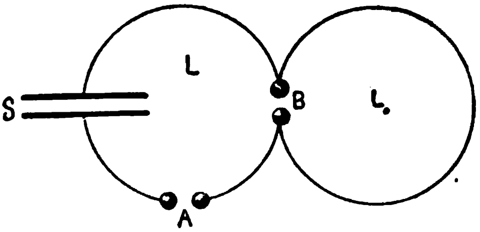


FIG. 4 *b*
Cut of Dr. Lodge's Fig. 2, *London Electrician* (vol. xxi, page 815.) showing alternative path.

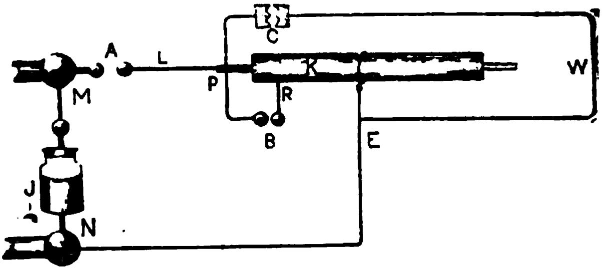


FIG. 2 *b*.

Fig. 5, 5a, 6 and 6a illustrate effects of different kinds of work introduced into the circuit. For references see Fleming's "Short

Lectures to Electrical Artisans," pp. 44, 45, and Ganot, 7 ed., 776; Maxwell, "Electricity and Magnetism," 2d ed., II, 211; Dove, Poggendorff's *Annalen*, Vol. XLIX, 1840, etc.

The methods used in the photographic part of the investigation would seem to cast some doubt on the results claimed. The effects noted deal chiefly with the prolongation or diminution of the current wave of the jar discharge by the secohmic resistance cut into the circuit. Might not a large element of uncertainty be introduced in the plates by the different degrees of illuminating power possessed by the sparks and by the irradiation of the plate?

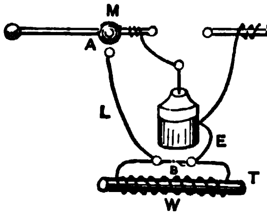


Fig. 5.
T=0.25″ diameter.

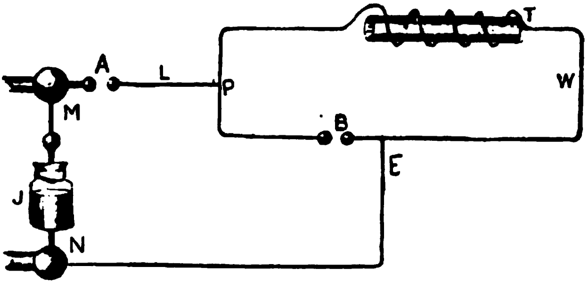


Fig. 5 *a*.
Fig. 5 redrawn.

In the cases when the discharges are through 76 and 20 inches of the same wire, respectively, hardly any difference appears in the spark. Is it not possible that the illuminating power of the quick bright spark through the short wire was so much greater than that of the necessarily more extended and consequently fainter spark through the long wire as to make the effect practically the same?

In the experiment in which two spark intervals are included in the circuit, as against one, formerly, it was found that the spark was materially lengthened. This might be expected considering the practically infinite resistance of the second air space which is introduced. The spark length, too, was found inversely proportional to any form of work introduced in the ground wire circuit. Possibly the photography of spark duration is not altogether the best method of measuring them. It is not yet by any means certain that a sensitive plate is equally affected by small equal time intervals at the beginning and end of the exposure, nor is it improbable that with sharp edged holes moving at an extremely high velocity some diffraction phenomena might intervene that would introduce serious errors in the photograph. It is, besides, very difficult to measure the faint trails of light left by the spark.

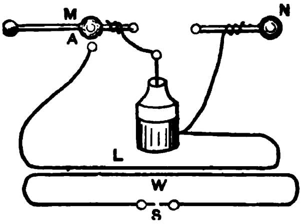


FIG. 6.
Length of parallel portions of circuits, 35″; distance separating them, 0.052.″

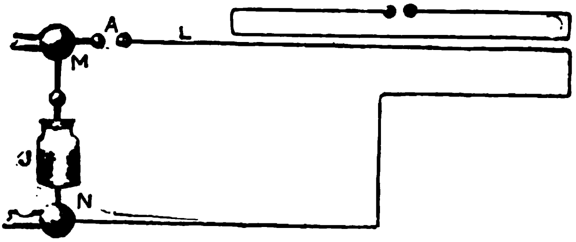


FIG. 6 *a*.
Fig. 6 redrawn.

On the negative they are practically invisible and in the prints their extreme delicacy and the fact that they fade away into the surrounding blackness without any sharp line of demarcation renders them almost impossible of measurement. On these grounds the measured spark distances of the first part of the article are probably more trustworthy as a basis for theory than the photographic results of the second part.

Returning to our original problem, why may not the cable, act- as a condenser, puncture itself in discharging, it having been charged by the current from A, Fig. 2c, which, meeting the lightning arrester, backs up its potential until able to strike across and thus to ground? This discharge occurs simultaneously with the spark at c.

If a wave of current be pictured passing along a linear conductor, it would probably appear something like Fig. 7, its front lengthened out by the self-induction effect of the cable, while the tension in the rear part is raised by the direct extra current. This high potential wave crest moves along the cable, until finding a weak spot in the insulation, it discharges rather than keeps up its strength with the impeding current ahead of it.

Fig. 7.
Form of current wave

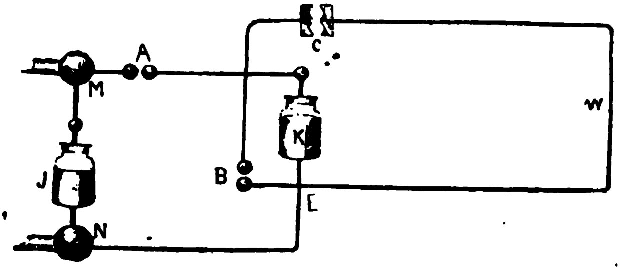


Fig. 2 *c*.
Equivalent to Fig. 2, with cable replaced by a condenser.

It is hoped to be able to present at some future meeting the results of certain experiments delayed so far by pressure of professional engagements.

DISCUSSION.

Mr. Brown:—These points are not meant to be anything in the nature of a discussion of the subject, they are merely brought up as hastily considered, and we hope that some of the gentlemen present may find them worthy of discussion. My friend, Mr. Child, is here, and will be glad to discuss any points, I am sure.

The Chairman:—Gentlemen, these interesting notes are now before you for discussion. It is to be regretted that Mr. Acheson is not here, because he is so familiar with the subject that he would have been able to say something of interest in the discussion; however, as the authors promise us a paper at a future meeting, we feel quite certain from the foretaste that they now give us, that the paper will be of great interest. The subject matter of these notes is one that is now receiving a great deal of attention all over the world, particularly in England, on account of its great bearing upon transmission through cables, and more remotely on the effect it has in all underground work. Consequently, it is a matter which is well worthy of study by electrical engineers in general, as well as of specialists.

THE SECRETARY:—I might say that these notes were prepared by these gentlemen with a view of forestalling any criticism which might arise. We felt in talking it over, that if the paper was to be criticised, it would be well to have it done at home, and not abroad, and for that reason the notes were presented in this way, in order that they might be elaborated at some future time. I might say, while I am up, that Mr. Abdank will be pleased to give some information with regard to headquarters which it is proposed to establish at the Paris Exposition. He spoke to me about it, and asked me to do it, but I would like to have him explain it himself, I think he would do it better.

THE CHAIRMAN:—If there are no remarks on this paper, we will give the floor to Mr. Abdank.

MR. ABDANK:—We have next year an exposition in Paris, as you know, in which there will be a special section devoted to electricity, and I am in charge of that section for the United States. I have collected a very nice exhibit, and I suggested having in the American section headquarters for the electricians of the United States. We will have there a collection of all the electrical papers published in the United States and the books published here, and then we will have a special attendant there, who will have in charge a register, where the members of the electrical societies can come and find necessary information, and I would suggest that the Institute here give us for the Exposition a collection of the proceedings for its headquarters, and a book wherein the members of the society arriving in Paris may inscribe their names.

THE CHAIRMAN:—We are very grateful for these remarks; the plan is one that every electrician will appreciate who visits the Exposition.

Mr. Abdank:—I forgot one thing, we have a special box for the reception of letters addressed to members, and any member intending to visit the Exposition can have his letters delivered there by simply having them addressed to the Paris Exposition, American Section, Box Electric. Our office boy will put all the letters in order, and we will distribute them to the members arriving in Paris.

The Secretary:—The Council, as you are probably aware, has arranged to supply a set of Transactions to the Exposition, and I presume they will act on Mr. Abdank's suggestion and furnish a register for the recording of names.

The Chairman:—I have no doubt that the subject will be taken up in due course by the proper authorities, and that our secretary here, with his usual eye to everything relating to the welfare of the Institute, will see that the matter is attended to in the proper manner.

Adjourned.

[*Communicated.*]

LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

A Reply to the Notes of Messrs. Jo. Stanford Brown and Chas. T. Child.

BY E. G. ACHESON.

A theory that cannot withstand the attacks incident to a general discussion, should be abandoned at the earliest moment. It is only by careful, exhaustive study, experimentation and discussion, that any theory can be proven worthy of acceptance. While our thoughts, energies and moneys are being directed to and expended for the adaptation of old, well-known principles, and the improvement of the various devices embodying those principles, it is wise to keep up a constant, restless, untiring search for new principles.

It was with such sentiments in mind that I formerly appeared before you; it is with such sentiments that I now approach this subject.

The reverence that all students of science should, and I am sure, do, feel for the opinions and theories of those who have in past years labored in our chosen field of research, ought not to be of such an all-absorbing character as to debar them from striving to add to and expand those theories; and peradventure, should occasion arise, abandon and throw aside those opinions and theories held sacred by our predecessors.

The discussion on my paper, entitled: "Lightning Arresters and the Photographic Study of Self-Induction," was reopened February 12th, 1889, by Notes from Mr. Jo. Stanford Brown and Mr. Charles T. Child. I regret that my time has been so much occupied during the last few months, that I have been unable to continue my investigations on the subject under discussion. Regrets are, however, useless, and I will, therefore, deal with the question as best I can with the material at hand.

In handling the subject, permit me to first quote each question or point as expressed in the "Notes," and then present such explanations or arguments as I may find possible or convenient.

QUOTATION:—"After careful reading it would seem as if the subject might be approached with advantage from more than one standpoint, and that even somewhat different conclusions were deducible, simply by rearrangement of the experimental data. We have given, a submarine or subterranean cable, to each end of which, at its junction with the main line, a lightning arrester is connected in the most approved manner, Fig. 1; to determine why, in spite of these precautions, the cable is not infrequently punctured when an "electrical discharge" enters the overhead system. For experimental purposes this has been produced in miniature, as shown in Fig. 2, 2*a*, 2*b*, and 2*c* *. * *"

Reply:—Figs. 2*a*, 2*b* and 2*c* have been introduced by the Authors of the "Notes," and while Figs. 2*a* and 2*b* are a close analogy to Fig. 2 and may be used to represent the case as presented in my paper, I make exceptions to Fig. 2*c*, for a condenser as there illustrated, can not be substituted for an elongated condenser as is obtained with a cable (see p. 143.)

Quotation:—"C now being made but a fraction of the insulation thickness, and w a short, thick copper wire, it was found that the cable was still frequently punctured at times when a spark appeared at c, and that the length of w appeared to enter the effect equally with the space variation of c."

Reply:—Part of this quotation contains language similar to that used in my paper, as do various other portions of the "Notes" but as no quotation marks are used, I can not, of a certainty, tell whether the authors made any of these experiments or whether they are simply restating the experiments and results obtained by me. If the latter be the case I beg to correct the results, as stated to have been obtained, in the last quotation. I found the wire w appeared to have as much influence on the results as the space c. The space c was always of a uniform length, hence no space variation of c occurred.

Quotation:—"What, then, is the relative efficacy of changing the length of w and the distance of the c points (Figs. 3 and 3*a*), to prevent a spark at B * * *"

Reply:—Owing to the fact that the authors then proceed to describe one set of my experiments and refer to the curves plotted from the results, and then suggest other, and I presume in their opinion, better, methods of determining this question; owing, I say, to these facts I am led to believe that they have not been as careful in reading my paper, or at least have not understood it as fully as might have been.

The question they propound is not the one I had presented to me. Reading from my paper, "A few more experiments also demonstrated that the wire w had apparently as much influence on the results as the space c, and consequently it became necessary to analyze the combination more completely to determine the relative values of c and w." To determine these values I did the simplest thing possible, viz: I first determined the value of w without c, and next introduced c and measured their united values.

Again, the object was not to determine their efficiency as *preventives* of the spark B, but rather their values as *producers* of that spark.

QUOTATION:—"That with the connections of Fig. 4 and w equal 20 feet, a spark should pass at B of over ⅛ inch in length, might perhaps have been expected from Dr. Lodge's results (London Electrician, XXI, 815), * * * True; Fig. 4 is not an 'alternative path' of necessity, for the B points may be moved out of the sparking distance."

REPLY:—The obtaining of this spark ⅛ of an inch in length, did not surprise me in the least, as I *did* expect it, for I had read of Dr. Lodge's experiments prior to that date. My expectations were, however, excited by his actual experimental results, and not by his theories and formulæ, as set forth in the above reference, and which were constructed to account for his experimental results.

When I remarked in my original paper, that the simple assertion that B and w were "alternative paths" did not, however, make them such; the peculiar idea contained in the latter part of the last quotation from the "Notes" had not occurred to me, excepting as it applied to the current as originally discharged from the jar J; if, however, I properly interpret the meaning in these "Notes," it is there suggested that they (the B points) might be separated to such a distance as to prevent any sparking whatever. But unfortunately they would then cease to stand as 1 to 10 compared with w, and those are the conditions we are considering, hence I fear it will be necessary to drop this suggestion regardless of its extreme originality.

"QUOTATION:—"W is now supposed 'electrified' by the charge across A."

REPLY:—I am not aware that I assumed that to be the case; but rather that w became electrified by the energy discharged from the jar J.

QUOTATION:—"The 'inverse extra current' of making simply cuts from the charging current; but the 'direct extra current' of breaking, *i. e.*, charging impulse cessation, acts as its continuation, but at a higher potential. (Ganot, 7th edition, 766)."

REPLY:—I do not believe I made any statement contrary to the above. The law is a well known, universally accepted one, and scarcely needs the support of any special authority.

QUOTATION:—"This extra current then is running towards E and would naturally be supposed to equalize on the same path as the current generating it. It might, therefore, be expected to dissipate on reaching earth."

REPLY:—With the kind permission of the authors of the "Notes," I would like to ask them what they mean by the term "equalize," and how they suppose "dissipation" takes place?

While it is well known that a charge of energy in a Leyden jar will become dissipated if left to itself and without any specially prepared discharging circuit, such a process cannot be considered in connection with the phenomena under discussion. The authors cite their authority for the generation of a current in the circuit and its flow or discharge into the jar; now, how they expect it to be dissipated I cannot see from their statement. The very name of this current—the extra current—defines it as existing distinct from the original, hence they cannot expect the original current to neutralize it, for it is the product of that current, and for it to neutralize itself requires it to either short circuit its own extremities or discharge back over the same path in which it was formed or generated.

QUOTATION:—"It would be of interest to some in this connection to know how it was ascertained that the instantaneous time duration of the extra current is shorter than that of the charging current."

REPLY:—A careful re-reading of my paper fails to bring to light any assertion that this was in any manner a demonstrated fact. I do not believe I referred to this in any other way than interrogatively—not being desirous to force my opinions, that were of a purely theoretical nature. The point being now raised however, I will take advantage of this opportunity to express my ideas more freely. It is not to be supposed that the authors of the "Notes" believe otherwise than that the "direct extra current" is less in energy *under ordinary conditions*, than the original current. They have given authority for their belief that it is of higher electromotive force. Their entire argument is on the basis that it traverses the same circuit as the original current; and hence it cannot meet a greater resistance, but possibly, everything being considered, including the fact that the circuit has already been *electrified* by the original current, the resistance may in reality be less in value than that opposed to the primary current. With these conditions given, are the authors prepared to assume that the extra current is even of the same duration as the original?

QUOTATION.—"On the assumption that the extra current dissipates through B, and not in the earth, it is argued that shortening W would tend to decrease this tendency to puncture the cable.* * * May not this be regarded simply as a safety short circuit for our cable considered as a Leyden jar? And what is this but a working case of a cable effectually protected by a lightning arrester B."

REPLY:—Once more I ask the authors of the "Notes" how they would expect the energy conveyed or represented in the extra current, to become dissipated in the earth (in this case the jar) in the infinitesimal intervals of time with which we are dealing. It would be interesting to know how they account for the oscillatory discharge of a Leyden jar, or do they disbelieve that oscillations occur? I will venture to express an opinion on this subject. The original current discharged from the jar does its equivalent of work in the circuit, and ceases to exist as current. The work performed appears as heat, light (under some conditions) and a distorting of the lines of energy which were originally within the circuit. The last work mentioned is of such a character that it causes another current, called the extra or self-induced current to be formed. The energy constituting this self-induced current is not localized into one or more places as was that forming the original current when contained on the surfaces of the jar; but it is distributed over or throughout the entire circuit. When such a circuit is "short circuited" surely the energy will be delivered more quickly, than would be the case with the energy in the jar, for there it has to traverse the entire circuit. Where no short circuiting occurs and the connections remain to the jar, the self-induced current will discharge into it.

Returning once more to the subject, I must say that under the conditions of my experiments, I cannot admit that the wire B, as shown in Fig. 2, acts simply as a safety short circuit for the cable considered as a condenser; but that it is a working case of a cable effectually protected by a lightning arrester I heartily agree; indeed, the major portion of my experiments were performed and my paper prepared to demonstrate and emphasize that fact.

Quotation:—"It is not altogether plain how the direct extra current is an effect of Faraday's electro-tonic state, when Faraday says (Researches 1, 16.): 'This state is altogether the effect of induction exerted, and ceases as soon as the inducing force is removed.'"

Reply:—The theory that the extra current, is due to the peculiar state of the circuit prior to the passage of the inducing current, was formed by me a long time ago, and was set forth in an article published in the *Electrical World*, Feb. 25th, 1888. The authors of the "Notes" are correct in so far as they go, and perhaps they are wholly so, on this point; but it is true that Faraday was never perfectly clear in his opinion as to this electro-tonic state. After having formed the opinion as above expressed in the quotation, he changed his ideas and considered there was no necessity for assuming the existence of this prior state. (Researches Vol. I, 66, 69.) Again, later on he revives the idea. (Researches, Vol. III, 367, 420.)

I do not deny, but rather confess to having attempted to add a little to Faraday's idea on this subject, for while he considered the state due to a peculiar action of the magnetic lines of force emanating from some external source, and that this state in conjunction with motion produced an electric current, I have gone back one step further and assumed a peculiar state prior to the influence of the external magnetic lines of force, and that it is this prior condition of the circuit that is the true cause of an electric current being generated when the circuit is forced to move across external lines of magnetic force. I justify my action in terming this prior state the electro-tonic state, by the fact that Faraday has, so far as I can learn, left it in more or less uncertainty, and also from these words of his. (Researches, Vol. III, 420.) "Again and again the idea of an *electro-tonic* state has been forced on my mind; such a state would coincide and become identified with that which would then constitute the physical lines of magnetic force. Another consideration tends in the same direction. I formerly remarked that the magnetic equivalent to *static* electricity was not known * * *"

If an error has been committed in thus venturing to trespass, I can only refer to the opening remarks of this communication, and add that the feelings that have inspired me in this work are there set forth.

As a passing remark I would say that I saw it stated in print, within the past few weeks, that this theory was old and contained nothing new. Evidently some one is wrong.

QUOTATION:—"Figs. 5, 5*a*, 6 and 6*a* illustrate effects of different kinds of work introduced into the circuit."

REPLY:—The authors then give a list of references to support this statement, I presume; but as I do not possess nor can I have access to any one of the volumes referred to, I will take it for granted they have sustained their statement, and which, by the way, agrees exactly with my own opinions, and which I believe I stated very clearly in my original communication. Without being able to see their references, however, I am at present unable to know whether they have referred to work producing similar results under similar conditions or not; but I am persuaded to think they have not.

QUOTATION:—"The methods used in the photographic part of the investigation would seem to cast some doubt on the result claimed."

REPLY:—That there is a possibility of the results obtained with the photographic apparatus having a limited value, owing to the uncertainty of uniformity in the speed of the disc and in other portions of the work, cannot be denied.

The results obtained do, nevertheless, correspond closely, not to say exactly, with those of the former portion of the investigations, when studied by the theories advanced. There are so many chances of error in this portion of the work that I would not myself refer to it, had not the authors of the "Notes" reviewed it and suggested various other ways and means for accounting for the peculiarities of the results as obtained.

QUOTATION:—"In the case where the discharges are through 76 feet and 20 inches of the same wire, respectively, hardly any difference appears in the spark. Is it not possible that the illuminating power of the quick, bright spark through the short wire was so much greater than that of the necessarily more extended and consequently fainter spark through the long wire as to make the effect practically the same?"

Reply :—The duration of these two sparks I found to be about the same; but with the shorter wire more work was done in the air space where the spark was produced. The fact that the spark with the short wire is brighter is an evidence of the increased work done at that point. This increase of work was indicated in the photograph by a heavier "ghost;" while the equality of their durations was shown by equal lengthed "ghosts." The simple fact of a spark being brighter, would not, I think, produce an elongation of its image through a rapidly moving aperture.

Quotation :—"In the experiment in which two spark intervals are included in the circuit, as against one formerly, it was found that the spark was materially lengthened. This might be expected considering the practically infinite resistance of the second air space which is introduced."

Reply :—I was under the impression, as indeed, I thought all others were who had been closely studying the recent advances in this line, that the question of length of circuit was of more importance in point of time, than resistance, and perhaps more especially so when the resistance was formed by an air space. Again, how can the authors account for the fact of my having obtained so small a time interval with 76 feet of wire in circuit with the air space A, when the B points were in adjustment, for they will find by referring to my paper that the second air space, which was introduced to replace the wire, was of the same length as space A. At this writing I desired to refer to the negatives and examine them, but unfortunately while all the others remain, the one produced with the two spaces in circuit, is missing, and I will not attempt to do what the authors seem to have considered doing, viz: Measure the lengths of the "ghosts" on the prints from the cuts. That they should have found the "ghosts" or trails, on the negatives practically invisible is rather strange, and I believe had I had the pleasure of being present at the time of their examination I might have been of service to them, for I have found not one who experienced difficulty in seeing the "ghosts," while none could define their limits.

Quotation :—"If a wave of current be pictured passing along a linear conductor, it would probably appear something like Fig. 7, its front lengthened out by the self induction effect of the cable, while the tension in the rear part is raised by the direct extra current. This high potential wave crest moves along the cable until, finding a weak spot in the insulation, it discharges rather than keep up its strength with the impeding current ahead of it."

Reply:—We have in this quotation and the accompanying figure all of the elements necessary either to substantiate the theory I have been advancing or else prove its utter worthlessness. The difficulty, of course, is in obtaining a correct interpretation.

May I ask how the authors determined that the direct extra current, occupied a shorter interval of time, as shown in the figure, than that of the original current? A fuller explanation of the discharging, through the insulation, of the high potential wave crest of the extra current would be interesting. Is it supposed to break through and rush out, like water through a bung-hole? We are here dealing with a complete electrical current, generated within the wire and having its high and low potentials within the wire. In accordance with the accepted theories, a current is supposed to discharge itself into or through a closed circuit, and this would necessitate both ends of the wave breaking through the insulation, if the insulation be broken at all. Perhaps this is the way in which they assume dissipation occurs. It is evident that the authors of the "Notes" are here confounding self-induced currents with static discharges.

It is probable that the gradual increase of the current as shown in Fig. 7, is more or less a correct representation of the facts.

What causes this cutting away of the earlier portion of the current wave? The authors say correctly, that it is produced by the self-induction effect of the conductor.

It is due to an inverse current having been raised in the path of the charging current. The work of producing or creating this current constitutes the momentary resistance that has been termed *impedance* by Mr. Oliver Heaviside, and it is this resistance that Dr. Lodge assumes bars the path of the discharging current by the way of the wire w and causes it to take the path offered by the B points. (Fig. 4.)

For the *impedance* of a wire to offer a resistance, or rather, for the *impedance* to exist prior to the expenditure of more or less of the current that it impedes, is exceedingly difficult, in fact impossible, to comprehend; for the existence of the resistance would be wholly unknown, did not the current energy exert itself against it; but the moment the first portion of the current wave meets and, speaking figuratively, combats with the resisting force, that portion of the current dies, but in dying it overcomes as much of the opposing resistance as is measured by its own energy; in fact they die together. This is followed by the overcoming of yet another portion of the *impedance* by another portion of the current energy, and so on to the complete annihilation of the total *impedance.*

But this process requires the loss of our original current energy; and if this be the case, and it must be so in order that the *impedance* have even an existence, how are we to discharge this energy across the B points, in the manner described by Prof. Lodge? Perhaps it may be said that it is a surplus quantity that was not required for this formation of *inpedance* that jumped the space at B; but this can hardly be so, for any energy remaining after the down-fall of the *impedance,* would have an open road through the low resisting wire.

Out of the dead body of the *impedance,* comes forth the energy expended at the B points.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. VI. New York, May, 1889. No. 5.

The thirty-third meeting of the Institute was held on March 12th, at the College of the City of New York. Vice-President T. C. Martin called the meeting to order at 8 o'clock and said:

In the absence of our Secretary, Mr. Ralph W. Pope, who is unfortunately detained by sickness in his family, it devolves upon me to introduce the business of the evening, and as I happen also to be one of the officers, it also becomes my duty to elect myself to the chair. I have much pleasure in announcing as the subject of the evening, "The Efficiency of Methods of Artificial Illumination," the paper upon which will be read by our well-known fellow member, Prof. E. L. Nichols of Cornell University, and I have much pleasure in calling upon the Professor to read his paper.

Prof. Nichols read the following:

THE EFFICIENCY OF METHODS OF ARTIFICIAL ILLUMINATION.

BY EDWARD L. NICHOLS.

Of human industries of the present day none perhaps, save those which have to do with providing the race with food, clothing, shelter and fuel, is more important to the cause of material welfare than that which deals with the production of artificial light, and he who seeks a means of measuring the material civilization reached by a nation might find an excellent criterion in the progress which it exhibits in the art of illumination.

The problem of comparing the various methods of artificial lighting which are in vogue at the present day is one of great interest, whether we view it from the commercial standpoint, taking the cost of production as our controlling factor, or from the broader basis of relative usefulness, cost being relegated to a secondary position, or, finally, from the purely scientific point of view. From whichever side we approach this question, we are led inevitably to the consideration of certain elements which are alike of scientific, utilitarian and commercial importance.

At first sight the attempt to determine the relative value of two sources of light would seem to be a simple matter. The number of factors, however, which enter into such a determination is surprisingly large, and many of them are of a character which makes it difficult to give them definite and complete expression. Of these the greater part are, indeed, commonly left out of account altogether, for the sake of simplicity, and we content ourselves with an antiquated and totally insufficient measure of our sources of illumination, which we call candle-power. It is to these other elements which enter into the question of the character of artificial light—such, for instance, as have to do with its *quality*—but which are, as a rule, quite lost sight of, to which I would ask your attention this evening, together with a discussion of those which are commonly made use of in photometry. If, in so doing, I am led to speak of methods which belong to the realm of pure science, I trust that you will agree with me in thinking that the results are not unimportant even to those whose interest in the problem of the production of artificial light is purely utilitarian.

In the important question of the efficiency of a light-producing machine or process, the practice of to-day is very far from having reached that degree of exactness of expression which we demand in other cases of the transformation of energy. In electric lighting the energy expended is readily determined in absolute measure, or in that excellent practical unit, the *Watt.* In lieu of any attempt to express the useful energy obtained, however, we still content ourselves in practice with that most unscientific unit, the candle-power, based upon a source of illumination which is particularly subject to fluctuations of intensity and color. Even as used in the Bunsen photometer the shortcomings of the standard candle are sufficiently apparent; but no one who has not attempted to study it by methods which make it possible to detect changes of color as well as of brightness, can fully appreciate its fickleness.

Such as it is, we do not, moreover, make the best of the standard candle as a basis of comparison of artificial lights. We speak of a 16-candle incandescent lamp, for instance, meaning oftentimes "mean spherical candle-power" and then we compare it with a petroleum or gas flame, measured in the horizontal plane only, much to the disadvantage of the electric lamp. The study of the distribution of intensities in accordance with the very complete system devised for that purpose by the Franklin Institute Committee in 1884, affords an invaluable means of comparing the performance of various types of incandescent lamps; but the adoption of "mean spherical candle-power" as obtained by that method, is, to say the least, misleading when it comes to the comparison of electric lamps with candles, oil or gas. It is to be hoped that when the present confusion of methods of measuring candle-power now existing in the electric lighting establishments in this country is supplanted by a recognized system, the fairer standard already established by law in England, viz., mean horizontal candle-power, may be adopted. It is, however, in the comparison of arc and glow lamps that the latter are put at the greatest disadvantage. Here we pit the incandescent lamp, using mean spherical candle-power as our basis, against candle-power measured at the angle at which the arc light sends out the greater part of its rays. Indeed, in too many cases, it is not with the actual intensity even in this most favorable position with which we have to do, but with an estimated or "nominal" candle-power which the arc, in matter of fact, never approaches.

The electric arc stands quite alone, for the purposes to which it is adapted, and need fear no rivals. It does not need to be estimated upon any fictitious basis, and the continuance of the pernicious practice of ascribing to it powers of illumination which it does not possess, must, in a great measure, be charged to the account of the standard candle; for with the best intentions in the world, the comparison of sources of light differing so widely in color as the candle and the electric arc, is nothing more than a rude approximation, leading by its very inaccuracy to the temptation of the grossest exaggeration.

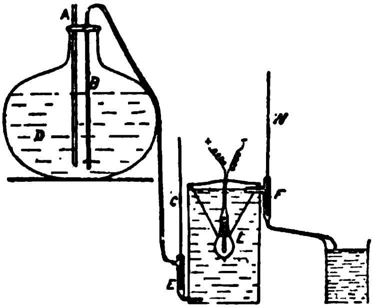


Fig. 1.

There are, fortunately, better methods of estimating the efficiency of a lamp than that which is based upon candle-power.

We may, for instance, determine the ratio of the energy of the light-giving radiation to that of the total radiation of the source of illumination. The efficiency of the incandescent lamp, according to this definition, has been recently determined by Mr. Ernest Merritt,[2] of the Physical Laboratory of Cornell University. Mr. Merritt followed two independent methods of investigation. In his first experiments the lamp was placed in a calorimeter constructed entirely of glass, Fig. 1, through which a constant flow of water was maintained. The temperature of the water upon entering and upon leaving the calorimeter was measured by means of the thermometers E and F. From this difference of temperature and the amount of water discharged in a given time, the heat absorbed by the calorimeter was calculated; proper correc-

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[2] Ernest Merritt, *American Journal of Science*, vol. 37, p. 167.

tions for loss of heat by radiations, etc., being applied. Now the water with which the calorimeter was filled is nearly transparent to the light-giving radiations from the lamp, and almost entirely opaque to the longer wave lengths which constitute the so-called heat spectrum. To have assumed it to be entirely so, would, however, have involved very considerable errors, and the diathermancy of the calorimeter on the one hand and its power of absorbing the luminous wave-lengths on the other, were accordingly carefully determined by methods long since elaborated and made use of by Melloni and by his distinguished English follower, Professor Tyndall. By applying these and other necessary corrections, we are able to calculate very accurately in watts, the non-luminous radiation from the lamp. The ratio of the total radiation, obtained either by submerging the lamp in a metallic calorimeter or by direct computation from the current and electromotive force gives the actual efficiency of the lamp considered as a light-making machine. The result is not a gratifying one, when we compare this process with most others in which energy is transformed. The efficiency of the best types of incandescent lamps, previous to the introduction of the recent three-watts-per-candle lamps now in successful operation, which would doubtless make a somewhat better showing, was found to be rather under than over five per cent. at normal candle-power; the values ranging from 0.5 per cent. to 6.5 per cent. The last named value for the efficiency was attained only at a temperature incompatible with any considerable length of life. The results obtained in this investigation are shown in table i.[3] and in the accompanying diagram, Fig. 3.

In this table W is the total energy, in watts; L the energy of the light, also measured in watts; C. P. the candle-power.

The second method pursued by Mr. Merritt was intended rather as a check upon the results already obtained than in the expectation of gaining further data. The rays of the lamp were allowed to fall upon the face of a delicate thermopile placed at a

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[3] Dr. Blattner, in an Inaugural Dissertation, published at the University of Zürich, gives similar values for the efficiency of the incandescent lamp. He finds, as the result of extended measurements with the glass calorimeter, that * * * "at the normal temperature of incandescence, where the intensity lies in the neighborhood of 16 candles, the efficiency does not rise above five per cent. to six per cent."—Emil Blattner; *Der Optische Nutzeffect der Glühlampen*, Frauenfeld, 1886.

distance of about 60 cm. The thermopile was in circuit with a low resistance galvanometer of the well-known "tripod pattern" of Sir William Thomson. The deflection of the galvanometer was taken as a measure of the total radiation, luminous and non-luminous, which fell upon the face of the pile. A cell containing a solution of alum was now interposed, and the reduction in the galvanometer noted. The arrangement of this apparatus is shown in Fig. 2.

Table I.[4]
Lamp A. Edison.

E. M. F.	W.	C.P.	L.	$\frac{L}{W}$	$\frac{L}{C.P.}$
74.2	34.6	0.9	0.18	0.005	0.59
91.6	56.2	4.8	0.98	.012	0.14
97.3	64.6	7.3	1.13	.017	0.15
100.3	69.8	8.9	1.62	.023	0.18
107.6	81.6	14.6	2.97	.036	0.20
109.8	84.4	16.3	4.57	.054	0.28
124.1	115.4	38.2	7.46	.065	0.19

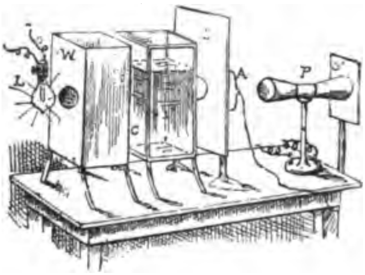

Fig. 2.

An aqueous solution of alum fulfills much more nearly the desired condition of complete opacity to "dark heat" and complete transparency to the rays of the visible spectrum than any other substance, but a certain small percentage of the longer wave lengths will pass through such a bath, and a considerably larger percentage of light-giving rays will be cut off by it. The proper corrections are, however, easily determinable, and when in this case they had been determined and applied, the ratio of total to luminous radiation was found to be in gratifying agreement with the efficiency obtained by the calorimetric method already described. See table ii.

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[4] Tables i and ii, and figures 1 and 2, are taken from Mr. Merritt's paper, already referred to.

TABLE II.

Lamp B.

An Edison 16 c. p. lamp. Resistance = 249 ohms.

E. M. F.	W.	C. P.	L.	$\frac{L}{W}$	$\frac{L}{C. P.}$
63.0	25.4	0.3	0.42	0.016	1.61
74.6	37.8	1.0	0.77	.021	0.79
85.4	52.5	2.5	1.96	.037	0.78
99.0	72.2	6.3	4.30	.059	0.68
116.0	102.0	15.2	7.38	.072	0.49

Lamp C.

Weston 16 c. p. Cold resistance = 402 ohms.

E. M. F.	W.	C. P.	L.	$\frac{L}{W}$	$\frac{L}{C. P.}$
72.0	21.6	0.4	0.46	0.021	1.27
87.4	33.5	1.5	1.10	.033	0.76
102.0	47.8	4.4	2.09	.044	0.48
117.0	66.1	10.7	3.19	.048	0.30

Lamp D.

Weston 16 c. p., 70 volt. Resistance = 152 ohms.

E. M. F.	W.	C. P.	L.	$\frac{L}{W}$	$\frac{L}{C. P.}$
43.0	25.8	0.5	0.53	0.021	1.06
50.7	36.0	1.6	0.97	.027	0.62
60.5	52.0	5.2	2.03	.039	0.39
67.5	65.5	11.0	3.95	.060	0.36

Lamp E.

Bernstein 8 c. p. Resistance = 11.3 ohms.

E. M. F.	W.	C. P.	L.	$\frac{L}{W}$	$\frac{L}{C. P.}$
12.2	25.2	0.2	0.20	0.008	1.00
13.4	30.8	0.5	0.41	.013	0.84
15.0	40.4	1.3	0.75	.018	0.57
16.4	53.2	4.1	2.03	.038	0.50

It is interesting to compare these results with some obtained by still another method. Captain Abney and Colonel Festing in the Proceedings of the Royal Society have described a series of elaborate studies of the radiation of the incandescent lamp. The portion of their very important investigation, which has a bearing upon the efficiency, deals with the exploration of the spectrum of the lamp, under various degrees of incandescence, by means of an exceedingly sensitive thermopile. The energy of the visible spectrum of most sources of light is so minute that it has generally been deemed impracticable to measure it in this way. Captain Abney and his co-worker have, nevertheless, secured measurements covering not only the extensive regions of the so-called heat spectrum lying inside the red, but also the shorter wave lengths which constitute the visible spectrum. Their results are shown graphically in Fig. 3.

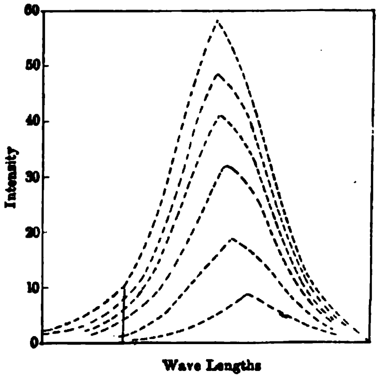

FIG. 8.

One of the British Electric Co's lamps was subjected to measurement in six different states of incandescence, corresponding to 33.6, 60.2, 93.8, 116.4, 130.8 and 150.5 watts respectively. The curves show the intensity of each wave length of the spectrum when the above amounts of energy were expended in the filament. and the total area inclosed by each curve affords a relative measure of the total radiation of the lamps in the state of incandescence in question. I have added to this diagram, of which, in other respects, Fig. 3 is a faithful reproduction, the vertical line lying between scale divisions 5 and 6 of their scale. This line marks the boundary between the visible and invisible spectrum. The entire area to the right of that line represents the energy wasted in the lamp; the much smaller area to the left comprises.

the light-giving rays. The ratio of the entire area of each curve to that portion lying within the boundaries of the visible spectrum gives us the efficiency of the lamp for the state of incandescence produced by the expenditure of the number of watts for which the curve is drawn. This ratio increases from a very small value with the brightness of the lamp.

I have made approximate integrations of the areas embraced by the curves for 150.5 and 130.8 watts, respectively, for the purpose of comparing the efficiency thus calculated with the results obtained by Mr. Merritt. The ratios are :—

Watts.	Efficiency.
150.5	5.55 per cent.
130.8	5.15 "

The corresponding values for lower states of incandescence are all smaller than the above.

It will be seen that these results are in complete agreement with those obtained with the glass calorimeter and by the method of Melloni. We have, indeed, reason to feel that our knowledge of the absolute efficiency of the incandescent lamp, in so far as it can be expressed by the ratio of luminous to total radiation is well grounded, and that we are in position to determine the light-giving efficiency of any type of glow lamp in any condition of incandescence with the same certainty and exactness which is attainable in the measurement of other machines for the transformation of energy.

The average net efficiency of the incandescent lamps of to-day, for we may leave untouched for the present the efficiency of the processes by which the latent energy of fuel from which the current is generated has been converted into electrical energy, is rather below, than above five per cent; and since the ratio of total radiation to light-giving radiation increases but slowly as the temperature of the radiating body rises, the efficiency is not likely to be increased in any very marked degree until we shall have learned how to suppress altogether these long wave lengths which yield us dark heat, and are able to limit the vibrations of our source of light to that brief octave which comprises the wave lengths to which alone the human retina responds.

Whether this great step is to be made by robbing the glow-worm and fire-fly of their secret, as has been suggested in a recent address by the director of the Sibley College of Mechanical

Engineering.[5] or by development along the lines sketched the other evening by Professor Brackett at the meeting of the Electric Club, or by the application of some principle as yet unconceived, we know not. Meantime, we have in the methods of artificial lighting of to-day abundant material for study and investigation.

The investigation of the efficiency of the arc light by the methods under consideration, is a much more difficult matter than the same determination in the case of the incandescent lamp. The radiating surface of the latter is very nearly of the same temperature throughout, and the ratio of total radiation to luminous radiation, obtained for the bundle of rays sent out in a single direction, gives us at once the measure of the efficiency. In the arc we have, however, a light-giving area of great brilliancy in the immediate neighborhood of the arc, and the temperature of the carbons falls with great rapidity from the maximum at the point and crater to regions which are at a red heat.

The entire light-giving area is included between the line surrounding the positive carbon, which is at red heat, and the corresponding line upon the negative carbon. Between these two lines we pass rapidly through regions, the light from which varies with the degree of incandescence by insensible gradations from that which a red hot surface is capable of emitting to that coming from carbon at the highest temperature which that substance can be made to assume. Now, the curve of distribution of candle-power taken in a vertical plane is, as we all know, a very peculiar one, and if we place our thermopile with alum cell interposed to measure the energy of the light-giving rays, the amount of radiation received upon the face of the pile will greatly depend upon the position in which the apparatus is set up. At an angle

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[5] "The second of these greatest of inventors is he who will teach us the source of the beautiful soft-beaming light of the fire-fly and the glow-worm, and will show us how to produce this singular illuminant, and to apply it with success practically and commercially. This wonderful light, free from heat and from consequent loss of energy, is nature's substitute for the crude and extravagantly wasteful lights of which we have, through so many years, been foolishly boasting. The dynamo-electrical engineer has nearly solved this problem. Let us hope that it may be soon fully solved, and by one of those among our own colleagues who are now so earnestly working in this field, and that we may all live to see him steal the glow-worm's light, and to see the approaching days of Vril predicted so long ago by Lord Lytton."—ROBERT H. THURSTON—*Transactions of the American Society of Mechanical Engineers*, 1881, *p.* 22.

of 45 degrees, or 50 degrees below the horizontal plane, for instance, the amount indicated will be five or six times greater than in the horizontal plane. Now, the surface of total radiation is much larger than that from which the light-giving rays emanate. It includes, in addition to the incandescent surfaces near the arc, all those portions of the carbons which are heated, either by conduction or by the current.

The amount of heat radiated from regions which are below the red heat is very considerable, and forms an important factor in the determination of the efficiency. Were the distribution of total radiation in the vertical plane identical with that of the light-giving rays, the measurement of their ratio with the axis of the thermopile in any plane which passes through the arc, would give us the efficiency of the lamp; but the curve of distribution of total radiation is not the same as that of candle-power, and the ratio in question is a function of the plane which the axis of the pile makes with the horizon. It becomes necessary, therefore, to make an exploration of the entire zone through which the lamp sends out rays, determining the ratio of total to luminous radiation for each angle, and then to integrate the results. Such an investigation is now in progress under my direction, and Mr. H. Nakano, who is carrying on the experiments, has reached some exceedingly interesting results. The research will soon be ready for publication in its complete form, and it will afford us much more definite data concerning the radiations of the electric arc than we now possess. It will not be out of place even now to say a word concerning the method which is being employed, and the general character of the results already obtained.

The first measurements were made by the second method employed by Mr. Merritt, the thermopile in the horizontal plane. The lamp subjected to measurement was of the long arc type, with 9 amperes of current and 45 volts. The efficiency in this plane was found to be surprisingly small. It was found, moreover, to vary in marked degree with the diameter of the carbons used, increasing as the diameter decreased to the point at which the carbons were rendered hot by the current, when, as might be expected, it fell off again.

The lamp was then mounted in a frame which also carried the thermopile and alum bath, and which was so constructed that measurements could be made at any desired angle. All necessary changes of angle could be made without varying the distance from the arc to the face of the pile, or the relation of the alum bath to the latter. By means of this apparatus, which is shown

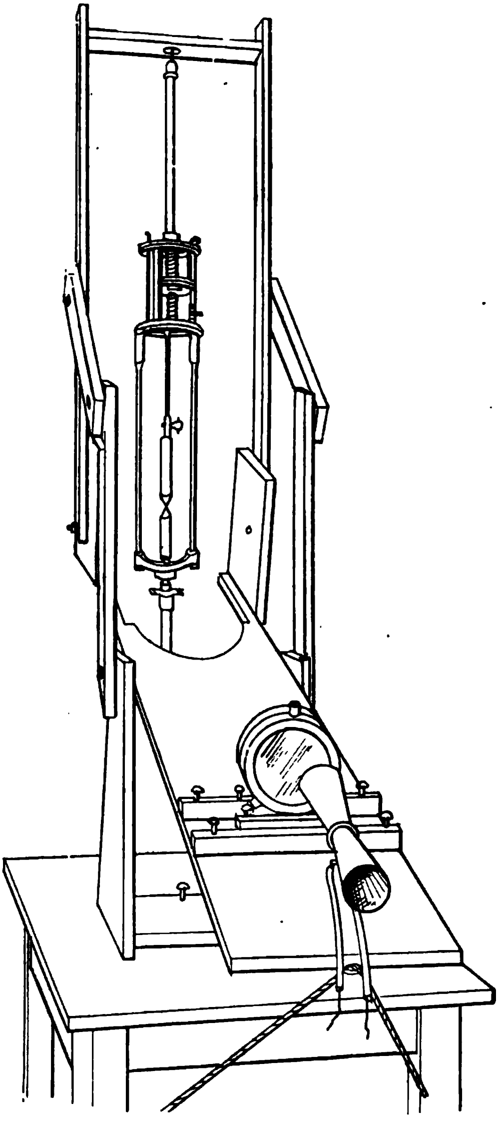

Fig. 4.

in figure 4, it has been found possible to determine the ratio of total to luminous radiation throughout the zone of radiation of the lamp, and to plot the curve of distribution of each.

Without entering into the details of an incompleted research, I may indicate the general character of the results by means of a diagram. The curves shown in figures 5 and 6 are those relating to a "long-arc" lamp with carbons .45 inch in diameter. They represent respectively the distribution of luminous and total radiation emanating from the lamp. The radii show the positions for which measurements were taken; distances measured along these lines from the origin to each curve give the relative intensities of light-giving and total heat energy in each position.

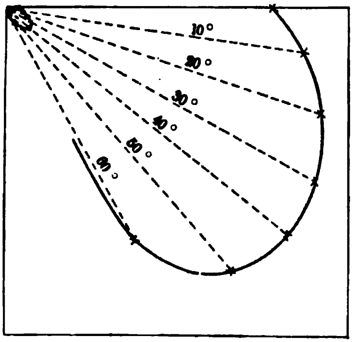

Fig. 5.

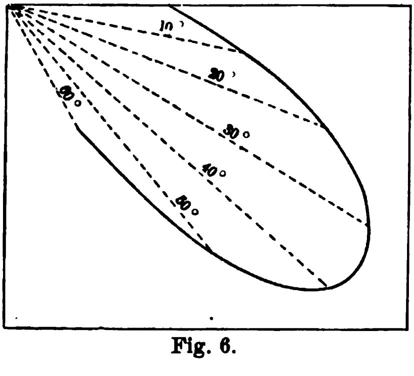

Fig. 6.

The curve of luminous radiation corresponds in form with that of candle-power. The distribution of total radiation, however, differs widely from these, and the ratio, $\frac{\text{luminous energy}}{\text{total energy}}$, is a function of the angle which the line of measurement makes with the horizontal plane. The values of the efficiency in the case in question for the various angles at which measurements were made, are given in table iii. They form the basis for the curve in figure

6, which is the diagram of efficiencies of the arc light under the conditions of operation already stated.

TABLE III.

Efficiency of an arc lamp; after measurements by Mr. H. Nankano.

Angle.	Efficiency. = $\frac{\text{Luminous radiation.}}{\text{Total radiation.}}$
0°	.0548
10°	.0901
20°	.1228
30°	.1506
40°	.1552
50°	.1059
60°	.0676
68°	.0492

The integration of these values gives as the mean efficiency of the arc lamp in question, .104, or 10.4 per cent.

The ratio of the radii of two semi-circles, the areas of which equal the areas inclosed by the curves of luminous and total radiation, gives us a measure of the "mean spherical" efficiency of the lamp. This ratio in the case of the lamp to which these curves refer is very much smaller than the generally accepted values of the efficiency of arc lamps, expressed in terms of the candle-power per watt, would lead us to expect. The intensity of the arc is, however, seldom expressed in mean spherical candles, and there are few data relating to the candle-power of this source of light upon which to found satisfactory estimates. On the one hand we find it stated, doubtless correctly, that the mean light-producing efficiency of the eleven dynamos, tested at the International Health Exhibition, held in London in 1884, was 1,440 candles per horse-power,[6] while the glow lamps only yielded 175 candles, and it is with estimates similar to the above with which we have generally to satisfy ourselves in comparing arc and incandescent systems of illumination.

For the purpose of deciding whether the results of Nakano's method of determining the ratio of luminous to total radiation are compatible with our other sources of knowledge concerning the arc light we must make use of mean spherical candle-power as our basis of comparison.

Very careful measurements of the mean spherical candle power of two arc lamps of representative and successful commercial types were made in the physical laboratory of Cornell University,[7]

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[6] Julius Maier, Arc and Glow Lamps, p. 52.

[7] *The Photometry of Arc Lights.* Thesis by Benjamin W. Snow, 1885. MS. in the Library of Cornell University.

following the method of the Franklin Institute Committee. Both of these lamps were rated at 2,000 c. p. They were maintained by dynamos of the types for which they had been especially designed, working under normal conditions. One of the lamps gave a mean spherical candle-power of 295, when working in series with nine other lamps of the same construction. When placed in circuit alone, the candle-power rose to 348. The energy expended in the lamp in each case was 550 watts, and the efficiency accordingly 1.8 watts and 1.6 watts per candle.

The other lamp was found to reach 609 mean spherical candles at the cost of 601 watts. An inspection of the result obtained by the Franklin Institute Committee would lead us to regard these as extreme values, between which the performance of almost all arc light would lie, as a rule, nearer the lower than the upper of these limits. The efficiency of arc lights, then, expressed in mean spherical candle-power per watt is frequently as low as 1.5 times that of the incandescent lamp, and, rarely, three times as high. Now the mean efficiency of the arc lamp under discussion, *i. e.*, the ratio, $\frac{\text{luminous radiation}}{\text{total radiation}}$, according to the measurements of Mr. Nakano, is almost exactly 10 per cent. The efficiency of incandescent lamps, defined in the same way, according to Mr. Merritt, may, as we have seen, be taken at about 5 per cent. The ratio is 2:1 in favor of the arc lamp, which lies within the limits determined by comparison of candle-power and watts.

Small as these values for the efficiency of the electric light are, they show marked improvement over the efficiency determined in a similar manner for sources of illumination which depend upon the incandescence of carbon by direct combustion. More than a quarter of a century ago, Julius Thomsen[8] presented before a gathering of Swedish men of science, the results of an investigation of the energy consumed in the sperm candle, and in various oil and gas flames. His memoir bore the suggestive title of the Mechanical Equivalent of Light, and is one of the earliest contributions to our knowledge of the energy of luminous radiation.

The instruments employed in Thomsen's experiments were the thermopile and galvanometer, the indications of which were reduced to absolute measure by determination of the deflection

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[8] Julius Thomsen: Das Mechanische Æquivalent des Lichtes; *Poggendorff's Annalen*, Bd. 125, p. 348.

produced by the radiation from a glass globe filled with hot water. The total loss of heat by radiation which this globe suffered was calculated from its rate of cooling, using Dulong and Petit's law. The water equivalent of the globe was 1,351 grains; its total loss of heat per minute at 50 degrees C. was 250 calories; its loss of heat by radiation at the above temperature, when the air of the room was at 17 degrees, was 102 calories per minute. Under these conditions the influence of the globe of water upon the thermopile placed at a given distance was observed. A luminous body having been substituted for the globe, its influence on the pile was likewise noted, and its total radiation in calories calculated by comparison of the deflections produced in the two cases.

For a sperm candle burning 8.2 grains per hour, the total radiation estimated by this method was 210 calories per minute. Other flames gave out quantities of heat nearly in proportion to their candle-power, as will be seen by inspection of table iv.

TABLE IV.

Calories per candle-power radiated by various flames, according to Julius Thomsen.

Source of Illumination.	Candle-power.	Calories per candle.
Sperm candle.........	1.	210
Gas flame............	1.2	201
Gas flame............	7.7	199
Moderator lamp.......	8.6	199

The luminous energy was obtained by a method similar to that pursued by Merritt in the experiments already described. A bath containing a layer of water 20 cm. in thickness was interposed between the lamp and the thermopile and the indications of the galvanometer noted. The results were based upon the assumption that the cell absorbed 13 per cent. of the light and all of the dark rays. This estimate is doubtless faulty, and may involve a reduction of several per cent. from the values given by Thomsen. His results, uncorrected, are as follows:—

TABLE V.

Energy of luminous radiation of various flames in calories, according to Julius Thomsen.

Source.	Candle-power.	Luminous Energy. per. c. p.
Candles...	1.	4.4 calories.
Moderator lamp....	6.25	3.9 calories.
Moderator lamp....	8.6	4.1 calories.
Gas...............	7.7	4.2 calories.
Gas...............	1.2	3.7 calories.

The energy of the light-giving rays per candle-power of light produced is then, according to Thomsen, very nearly the same for the flames of the candle, oil lamp and gas burner. This conclusion has been abundantly confirmed by spectro-photometric observations of the quality of the light which they emit. It appears, moreover, from these observations, of which I shall have occasion to speak at length presently, that the temperature of the incandescent particles in such flames agrees very closely indeed with that of an incandescent lamp, consuming 5 watts at 16 c. p, being, as a rule, lower, rather than higher, than the latter; and the calculation of the energy of such a lamp in calories per minute, making use of the data furnished us by the investigation of Abney and Festing, or of Merritt, affords most satisfactory evidence of the accuracy of Thomsen's results. Let us take for example the case of an incandescent lamp in which 5 watts per candle-power are expended, and in which the ratio

$$\frac{\text{Luminous radiation}}{\text{Total radiation}} = .05.$$

The energy converted into light in such a lamp is .25 watt. Now,

$$1 \text{ gramme-calorie} = 4.2 \times 10^7 \text{ ergs}.$$

$$1 \text{ watt} = 10^7 \text{ ergs} = \frac{1}{4.2} \text{ gramme-calories per second}.$$

$$.25 \text{ watt} = \frac{.25 \times 60}{4.2} \text{ gramme-calories per minute},$$

and the heat equivalent of the luminous energy is 3.6 gramme-calories per minute.

This value is somewhat smaller than those given by Thomsen in the preceding table, but proper corrections for the diathermancy of the water bath used in his experiments and for the difference in standards of candle-power—the light given by his sperm candle being slightly in excess of the legal standard of the present day—will bring the two into excellent agreement.

The entire energy developed by the chemical reactions occurring in these flames per candle-power of light produced were, however, about 1,400 calories in the case of candle and oil lamps, and 4,100 calories in the case of gas, so that the gross efficiency would be respectively 0.3 per cent. and 0.1 per cent. The average ratio of total radiation reaching the thermopile, to luminous radiation is about .2 per cent., but this ratio cannot be considered in the same light as that obtained in the case of the incandescent lamp, since a gas or oil flame is in reality a column of heated vapor containing particles of unconsumed carbon, which reaches far above the luminous region, sending out dark rays from its upper and cooler portions, which do not reach the face of the thermopile at all. The relation which the total radiation of this heated column bears to the heat dissipated by convection cannot be determined, and the determination of total radiation from the light-giving portions alone is of little value. It is only in the matter of gross efficiency, therefore, that we can institute a comparison between electric lamps and the older methods of illumination, and the gross efficiency of the former depends upon the efficiency of production of electric current. Under conditions, such that 10 per cent. of the total energy of the fuel consumed is converted into electricity—conditions, which are, I believe, rarely attained by our present methods—the gross efficiency of an incandescent lamp, of which the net efficiency, determined by the methods which I have already described, is 5 per cent., would be 0.5 per cent.; a value which is five times greater than that of a lamp burning the kind of gas used by Thomsen, and which stands in the ratio of 5:3 (or, probably, rather more than that) to the gross efficiency of an oil flame. These flames must, of necessity, vary very widely with the quality of the fuel consumed, and with the condition under which combustion is maintained.

If we apply a similar method of computation to the values given by Mr. Preece in his recent address at the Bath meeting of the British Association for the Advancement of Science,[9] assuming that the conditions under which each illuminant is consumed are such that the temperature of the incandescent carbon equals that which is maintained in the filament of an incandescent lamp at 5 watts per candle, with a net efficiency, as before, of 5 per cent., a set of conditions which can be attained in practice only in the most perfectly constructed oil and gas lamps, and which are fulfilled in many of the best of our incandescent lamps of to-day, we obtain the following results:—

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[9] Preece. Address before Section A, meeting of the B. A. at Bath, 1888. *Nature*, vol. 38, p. 496.

Table VI.

Gross efficiency of various illuminants deduced from the values given by Preece. The heat equivalent of the luminous radiation of a candle is assumed to be 3.6 gramme-calories per minute.

Illuminant.	Watts per candle. (Preece.)	Gramme-calories per minute per candle.	Luminous radiation. / Total energy = gross efficiency.
Tallow........	124	1770.	.00203
Wax.	94	1340.	.00268
Sperm	86	1230.	.00293
Mineral oil ...	80	1143.	.00315
Vegetable oil..	57	815.	.00442
Coal gas	68	971.	.00317
Cannel gas ...	48	685.	.00521

In considering these values it should be borne in mind that coal-gas is a fuel which has been obtained by the previous expenditure of considerable energy. Were we to take as our basis the consumption of coal necessary to the production of a candle-power of light, instead of the heat of the combustion within the gas flame itself, the values for the efficiency of coal-gas as an illuminant would be very considerably reduced.

We have already seen that there are excellent experimental grounds for the statement that the gross efficiency of the incandescent lamp is about .005, and that of the arc lamp about .01, under the assumption of the waste of 90 per cent. of the total energy of the fuel in the various processes leading to the production of the current, and that the latter figures, small as they are, are considerably in excess of the gross efficiency of candles, oil or gas. We are in possession, moreover, of the means of expressing exactly the degree of superiority which the electric light possesses over those illuminants in the important matter of the heating effect upon the surrounding atmosphere. Gas and oil flames deliver in the neighborhood of 1,000 gramme-calories of heat per minute to the room for each candle-power, of 3.6 gramme-calories of luminous radiation. An incandescent lamp of 5 per cent. net efficiency delivers only 72 gramme-calories per candle-power.

The net efficiency of the incandescent lamp may be raised somewhat by increasing the temperature of the carbon filament, and, indeed, it has already been somewhat raised by the introduction of the new "3 watt" lamps. These lamps have been excluded from this discussion because we have as yet no complete data with reference to them, and because it seemed desirable, aside from that, to institute comparisons between sources of illumination in which the degree of incandescence was the same, and for all of which, consequently, candle-power had the same meaning.

When we raise the temperature of our source of light, however, we introduce changes, of which the methods thus far considered take no cognizance.

In increasing the degree of incandescence, the ratio of luminous energy to total energy of radiation is slowly increased, and at the same time the relative brightness of the various rays which constitute the visible spectrum changes. In a word, the *quality* of the light changes, and we are confronted by important variations which cannot be expressed in candle-power.

The investigation of these changes in composition, upon which color depends, involves the study of the visible spectrum of the source of light and the comparison of its intensity, wave length for wave length, with the spectrum of some properly chosen standard. The instrument necessary to this work is the spectro-photometer, by means of which the spectra of the two lights to be compared are brought together in the field of view, one above another, like wave lengths everywhere in the same vertical line. The measurements consist in bringing the two spectra to the same intensity in each of the regions selected for observation successively, and in determining the amount by which it is necessary in each case to reduce the intensity of the brighter. A variety of devices have been made use of for varying the brightness of the spectra, and the standards of comparison adopted by different observers have included the candle, the petroleum flame, the gas flame, and the incandescent lamp, maintained at a constant voltage. Thus W. H. Pickering[10] obtained the necessary range of intensities by placing the lamp under inspection upon a slided carriage and varying its distance from the slit of the spectroscope. His standard of comparison was a gas flame. H. C. Vogel[11] used as his standard a petroleum flame. Crova,[12] who brought the intensities of the two spectra to equality by means of a polarizing device, adopted the moderator lamp as a standard. Otto Schumann, in

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[10] W. H. Pickering, *Proceedings* of the American Academy of Arts and Sciences, vol. 15, 1880.

[11] H. C. Vogel, *Berliner Monatsberichte*, 1880, p. 801.

[12] A. Crova, *Comptes Rendus*, 87, 1878, p. 322.

his study of the incandescent lamp, made use of a "benzine" candle. The intervention of a revolving disc with open and closed sectors and of wedges of smoked glass have also been applied to the regulation of the intensity, and the method of varying the width of the slit itself, a device due to Vierordt, has been found to give good results, where the range of intensities was inconsiderable.

The methods of presenting the results of this class of measurement have differed as widely as the methods of observation and the choice of standards; but it is possible wherever the data are properly given and the character of the standard is known, to reduce them all to a common basis of comparison. When thus reduced these measurements are found to agree in all essential particulars, and they afford us very definite knowledge of the character of the light emitted by the sources of illumination to which the spectro-photometer has been applied.

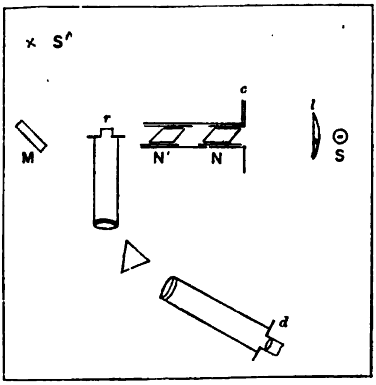

Fig. 7.

Figure 7 shows the form of the spectro-photometer recently used by Mr. W. S. Franklin and myself in an extended series of observations upon artificial sources of illumination and in the comparison of their spectra with that of daylight.

To the slit of a one prism spectroscope are attached a set of total reflection prisms, *r*, by means of which rays may be introduced into the upper and lower halves of the slit, these rays coming respectively from sources of illumination placed to the right and left-hand in a line at right angles to the axis of the collimator tube. The standard of illumination, which is an incandescent lamp s, maintained at 16 candles, is placed at the observer's right hand.

Its rays, having been rendered parallel by means of a condensing lense, *l*, pass through a pair of Nicol's prisms, N and N′, the first of which is free to revolve, and enters the upper half of the slit. The intensity of the spectrum of this lamp can be reduced at will by the rotation of the first Nicol, and its brightness is calculated from the angle between the flames of polarization of the two Nicols. To the left of the slit is mounted a block of magnesium carbonate, its face vertical and forming an angle of 45 degrees with the axis of collimation of the spectroscope. The rays of the source of light to be investigated, s′, which is placed at a convenient distance, fall upon the magnesium carbonate, and it is such of the diffusely reflected rays from the latter as reach the total reflection prisms, and thus enter the lower half of the slit, which form the second spectrum. The object gained by the interposition of this white surface is an important one. All artificial light, except that from an incandescent lamp, emanates from glowing materials varying greatly in temperature, and the spectrum obtained directly from the source is always that due to some particular portion of the luminous body under investigation, to the exclusion of the remainder, and differs more or less in character from that of the source taken as a whole.

The spectrum of rays reflected from the face of the magnesium carbonate, however, contains in proper proportions the light from all portions of the source, and one of the chief sources of error to which spectro-photometric work is subject, is thus eliminated.

The investigations made with this instrument, which have been described in a paper read before the American Association,[13] included the measurement of candle flames, and of various petroleum and gas lamps, also of the lime light, the incandescent lamp under a wide range of conditions, of the electric arc and of daylight by clear and by clouded sky. The results obtained illustrate the precise character of those differences in composition to the bearing of which, upon the study of the efficiency of artificial illumination I desire to call your attention.

In the first place it was found that the light of the candle, and of oil and gas flames, although subject to considerable fluctuations from variations of the condition under which combustion occurs,

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[13] Edward L. Nichols and W. S. Franklin. A Spectro-photometric Comparison of Artificial Sources of Illumination. Paper read before Section B; A. A. A. S. at the Cleveland meeting, August, 1888.

differ but slightly in quality from each other and from that of incandescent lamps maintained at normal candle-power. The incandescent material in all is carbon; and it is a significant fact that the average temperature of incandescence is nearly the same in all luminous flames, and that the highest temperature, until very recently at any rate, at which it has been found practicable to maintain the carbon filament of the glow-lamp, is very nearly that at which the same material exists under the most favorable conditions in oil and gas flames. Hence it is that the radiant energy per candle-power, as has been shown by Julius Thomsen, is nearly the same for all these flames.

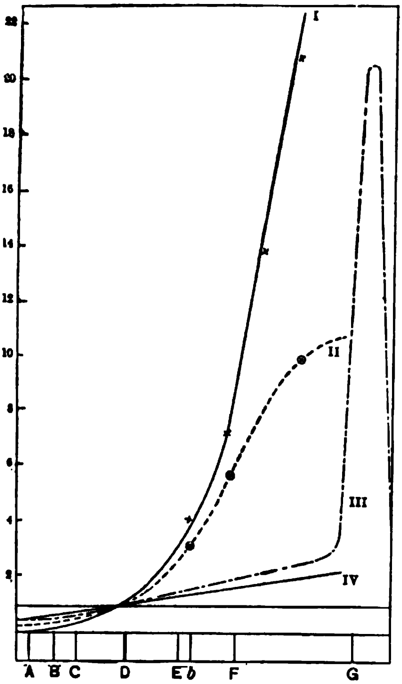

FIG. 8.

Passing to other sources of illumination, such as the lime light and the electric arc, we find the entire portion of the spectrum lying beyond the yellow increasing more rapidly, regions of longer wave length than the yellow increasing less rapidly than the candle power; the rate of increase growing steadily as the wave length diminishes from the red to the violet end of the spectrum. Fig. 8 shows the results of comparisons of the spectra of the lime light, electric arc, and of daylight, by clear and by unclouded sky, with that of an incandescent lamp maintained at its normal candle-power. This lamp, which was chosen as a standard of comparison, was an Edison 16-candle, 100 volt lamp. Its light was precisely similar to that of a "Board of Trade" argand gas burner, showing the same relative brilliancy throughout the whole of the visible spectrum. The measurements were made with the spectro-photometer just described (see Fig. 7), the light in every case falling upon the block of magnesium carbonate, and entering the slit of the spectroscope after reflection from the face of the latter. For purpose of comparison the brightness of the standard incandescent lamp has been taken as unity throughout the spectrum. If we make a diagram in which abscissæ are wave-lengths and ordinates, intensities, referred to that of the standard as unity, the curve for this lamp will be a straight horizontal line, with ordinate = 1.

All other spectra are reduced to the same brightness in the region of the D line of Fraunhofer, and their brightness in other regions, referred to that of the corresponding wave lengths in the spectrum of the standard, is shown by means of curves.

Observations were made in ten regions of the spectrum, equidistant as to wave length, and embracing the whole of the visible spectrum. It will be seen by reference to the figure that the lime light is relatively much stronger in the violet and much weaker in the red than the standard. In the arc light this increase of intensity toward the violet is even more marked, but its variation in quality from the lime light is by no means so great, as its exceedingly brilliant, bluish-white appearance to the eye would lead us to expect. The source of that appearance and of the very high actinic value of the arc is found, however, in the presence of an exceedingly bright band in the extreme violet. Here the relative intensity of the arc light spectrum rises abruptly about 3 to 20 times that of the incandescent lamp.

Measurements were made upon a long arc lamp with ½ inch carbons, with 10 amperes and 50 volts and a short arc lamp with a 20 ampere current. The character of the light was found to be almost identical excepting in this light band in the neighborhood of the G line, which is much more prominent in the long arc lamp. The character of the light was found to be much the same at all angles below the horizontal plane.

The only other spectrophotometric measurements of the electric arc with which I am acquainted, have been made upon the

Foucault regulator, a lamp with carbons of small diameter. Fairly concordant determinations of the distribution of intensities in the spectrum of this lamp by W. H. Pickering,[14] H. C. Vogel,[15] and Crova,[16] show that the light of this lamp is decidedly bluer than that of the lamps with large carbons in vogue at the present day; the spectrum of the former being about five times as rich in the violet as that of gas or oil flames. Mr. Nakano's results, of which mention has already been made, indicate marked increase in the efficiency of the arc lamp, as the diameter of the carbons is decreased; and since increase of efficiency arises from a higher average degree of incandescence, we should expect just such an increment in the shorter wave lengths as shows itself in the case of the Foucault regulator.

Finally, in the case of daylight we find the intensity of the shorter wave lengths exceedingly great as compared with that of any artificial source of illumination. If the spectrum of daylight be brought into comparison with that of an incandescent lamp, and the two be reduced to the same intensity in the yellow, the extreme red of the daylight spectrum will be so *dim* in comparison with that of the lamp as to render measurements difficult. In the violet the discrepancy will be just as marked, the sun's spectrum being relatively of enormous brilliancy. Curve i., Fig. 8, shows the result of such a measurement of the daylight spectrum, taken just after noon upon a cloudless summer day.

Vogel has given data of a determination in which the preponderance in the violet was even more marked, the relative intensity of daylight for the region corresponding to wave length, 4,260 (just beyond the G line), being 100 : 1 for spectra which were equally bright in the neighborhood of the D line.

Ordinarily, there is considerable absorption of the blue and violet by moisture in the atmosphere, and a measurement of daylight under a densely clouded sky gave curve ii. Even under these conditions, with the violet reduced to about one-tenth of the value which it possesses in clear weather, daylight is very much bluer than any artificial light. The light emitted by burning magnesium approaches more nearly to it than any other, being, according to Pickering, considerably stronger throughout the blue and violet than the light from the Foucault arc lamp.

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[14] W. H. Pickering, *l. c.*

[15] H. C. Vogel, *l. c.*

[16] A. Crova, *l. c.*

Its color, however, is not reinforced by a violet band like that in the spectrum of the electric arc, a region which, by its extraordinary brightness, has marked influence upon the color of the light.

These variations depend upon temperature primarily, and they may be studied to the best advantage in the spectrum of the incandescent lamp. The radiating surface of such a lamp is at a nearly constant temperature throughout, and it may be given a very considerable range in the matter of incandescence, involving variations in color and total intensity, which are under complete control.

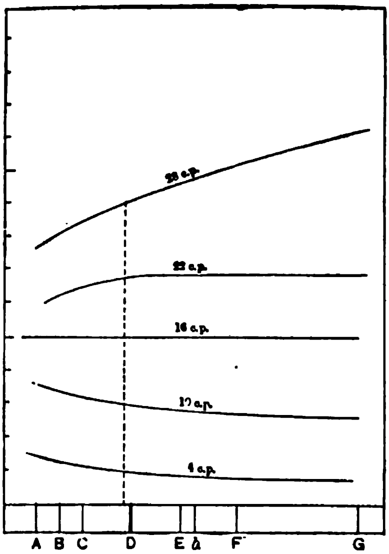

Fig. 9.

An Edison lamp, similar to that which had been used as a standard in our previous measurements, was subjected to spectrophotometric analysis by Mr. Franklin and myself, at 4 candles, 10 candles, 16 candles, 22 candles and 28 candles, successively. The results obtained are shown in figure 9.

The spectrum of the lamp at 16 candles is taken as of unit brightness throughout, and the other curves show the amount of light of each wave length emitted by the lamp in the above-mentioned states of incandescence. Intensities, in every case, are referred to the light of corresponding wave length, given by the same lamp at 16 candles, as unity.

At the red end of the spectrum the intensity increases more slowly than candle-power; at the violet end more rapidly. There exists an intermediate wave length for which the brightness is proportional to the candle-power. The vertical line in the diagram shows the portion of the spectrum for which this holds true, a point in the yellow at wave length 6,000.

If we suppose the same measurements executed upon a set of lamps in every respect similar to the one in question, excepting that their radiated surfaces are of such size that they give 16 candles, as indicated by the Bunsen photometer when the degree of incandescence corresponds respectively to that at which the lamp actually measured emitted at 4, 10, 16, 22, and 28 candles, there would result a set of curves of the same general character as those in Fig. 12. At wave length 6,000 these curves would have a common value, and they would serve to indicate both the quantity and quality of the variation of the carbon filament, as a function of the incandescence, candle-power remaining constant. The form of these curves is shown in Fig. 10. They afford us data for discussion of the candle as a standard of illumination which could not be obtained in any other way. They show the relative distribution of energy in the spectrum of five lamps, which, according to the indications of the Bunsen photometer, are of equal brightness, but which differ both in the quality and

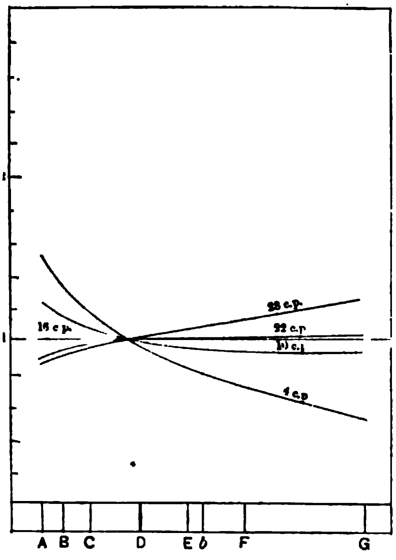

FIG. 10.

in their total energy of their luminous radiation. Their efficiency expressed in terms of the candle-power produced per watt expressed in the lamp, rises rapidly as the incandescence increases. For lamps of the type upon which these measurements were made, for instance, the relative efficiency referred in each case to that of the lamp, the incandescence of which corresponds to that of an Edison lamp at 16 candles, is given in the column headed $\frac{\text{Candle-power}}{\text{Watts}}$ in table vii.

TABLE VII.

State of incandescence equivalent to that of an Edison 16-candle lamp, giving—	Relative efficiency referred to that of a 16-candle lamp as unity.	
	$\frac{\text{Candle-power}}{\text{Watts.}}$	$\frac{\text{Luminous radiation.}}{\text{Total radiation.}}$
(1) 4 candles..............	.394	.211
(2) 10 "	.729	.632
(3) 16 "	1.000	1.000
(4) 22 "	1.218	1.368
(5) 28 "	1.391	1.589

If, however, we retain as the measure of efficiency, the ratio, $\frac{\text{luminous radiation}}{\text{total radiation}}$, we can deduce from these curves values coinciding with those reached by the method of the thermopile and alum cell. To this end we must know the above-mentioned ratio for some one candle-power and the number of watts necessary to the production of each state of incandescence for which the computation is to be made.

The relative efficiency can then be calculated from the areas inclosed by the respective curves. Were the candle-power proportional to the energy of luminous radiation, the values thus obtained would be identical with the relative efficiency determined from the relation of candle-power to watts. The relative efficiency thus computed (see table vii.) does not coincide with that obtained from the ratio of watts to candle-power, however, for the reason that the various rays which make up the visible spectrum do not enter into the production of candle-power in proportion to their energy. The most important wave lengths, so far as light-giving power is concerned, are those which form the yellow of the spectrum, and the relative luminosity falls off rapidly both toward the red and the violet. The longer wave lengths have, however, much more influence upon candle-power than the more refrangible rays, as will be seen by an inspection of the curves, the relative falling off of the red end of the spectrum being compensated by larger and larger accessions in the blue and violet as the state of incandescence increases.

Luminosity is the factor, which we must take into account in seeking a complete expression for the efficiency of any source of illumination, and the method to be pursued in the determination of the luminosity must depend upon the use to which the light is to be applied. If we estimate light by its power of bringing out the colors of natural objects, the value which we place upon the blue and violet rays must be very different from that which would be ascribed to them if we consider merely its power of illumination as applied to black and white. In a picture gallery, for instance, or upon the stage, the value of an illuminant increases with the temperature of the incandescent material, out of all proportion to the candle-power, whereas candle-power affords an excellent measure of the light to be used in a reading room.

A number of determinations of the luminosity of artificial light have been made. In the earliest of these by Vierordt,[17] the amount of white light which may be mixed with each color of the spectrum, without producing an appreciable change of tint, was taken as the measure of the luminosity of that color. The result is shown graphically in Fig. 11. The curve indicates the light-giving power of each wave length, estimated in the above manner. The source of light experimented upon was a petro-

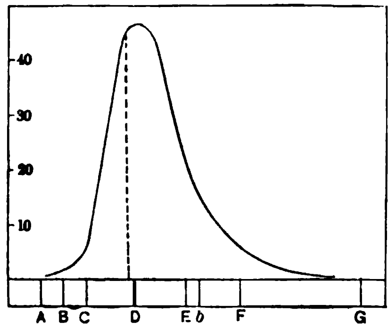

FIG. 11.

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[17] C. Vierordt, *Annalen der Physik und Chemie*, Bd. 187, p. 200.

leum flame. The vertical line indicates the region of the spectrum within which—according to the measurements of the quality of light of the incandescent lamp already described—the increase in the brightness of the spectrum is proportional to the candle-power. This region corresponds very nearly with that of maximum luminosity of the spectrum.

The luminosity of the blue and violet rays is so very small that in the production of *candle-power* the influence of the very rapid growth of this end of the spectrum with increasing temperature of the lamp is scarcely appreciable. If we estimate the light-giving value of the different portions of the spectrum by means of the facility with which we can distinguish black characters upon a white ground, the importance of the more refrangible rays is still farther diminished. Thus Macé de Lepinay and Nicati[18] have shown that if yellow and blue light, estimated to be of equal brightness by photometric means, are of such intensity that one can clearly distinguish a printed page when illuminated by the yellow, the same page will be entirely illegible, when the blue light alone falls upon it. These observers conclude, indeed, that "the mere distinguishing of objects is due almost exclusively to the illumination produced by the less refrangible half of the normal spectrum;" so that at equal brilliancy "the superiority of yellow sources of light (luminous gas flames, incandescent lamps) over sources richer in blue rays (light of the electric arc) is incontestible." "The only real advantage," they add, "upon the side of the light from the electric arc is when one desires to rehabilitate objects more nearly in the hues which they present in the light of day." This single advantage is one which it is impossible to take due account of, numerically, in an estimate of the efficiency of artificial illumination. It is, nevertheless, a most important factor in determining the adaptability of a light to nearly all the purposes of everyday life.

Otto Schumann,[19] to whom we owe a most exhaustive study of the light of the incandescent lamp, has determined the luminosity of several types of such lamps at various stages of incandescence. His results, in so far as they apply to a 16-candle Edison lamp, are given in table viii.

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[18] Macé de Lepinay and W. Nicati, *Journal de Physique*, second line, T. 2, p. 75, 1888.

[19] Otto Schumann, *Elektrotechnische Zeitschrift*, Bd. 5, p. 224, 1884.

TABLE VIII.

Luminosity of an Edison lamp of various stages at incandescence, according to Otto Schumann.

Amperes.	Volts.	Watts.	Luminosity of various regions of the spectrum.						
			Red $=\lambda$ 676	Orange λ 656	Yellow λ 615	Green λ 557	Blue λ 487	Indigo λ 464	Violet λ 429
0.625	87.3	54.6	1.18	4.64	......	20.14	0.64	0.11	.....
.678	90.4	61.3	1.68	6.66	.....	30.35	1.05	0.20	.....
.737	94.7	68.8	2.24	9.34	53.11	50.05	1.71	0.34	....
.743	97.4	72.9	2.59	10.92	63.28	62.23	2.28	0.47	....
.823	105.0	86.4	4.06	17.14	99.22	104.3	4.09	0.88	. ..
.944	112.9	106.7	6.97	28.85	163.0	189.1	7.88	1.75	.....
1.009	117.6	118.6	8.74	37.14	222.6	255.7	11.62	2.71	0.42
1.035	120.4	124.7	9.36	41.67	263.8	317.7	16.04	3.79	0.60
1.099	124.5	136.4	11.43	52.36	345.2	472.2	26.24	6.58	1.04

I have made these values of Schumann's the basis of a set of curves, by means of which the variation in luminosity, with total energy in watts, is exhibited (see Fig. 12). It will be seen that the position of the maximum moves slightly towards the violet as the brightness of the lamp increases. This tendency is very slight within the range of temperatures reached by the incandescent lamp. In the arc light we should find the displacement more marked, but in the case of sunlight, in which, as we have seen, the shorter wave lengths are in such preponderance, the movement of the maximum is inconsiderable. The curves shown in Fig. 13 are taken from the investigation by Crova, who has determined the distribution of luminosities in the spectrum of sunlight and of the flame of a petroleum lamp. The maximum in sunlight is at $\lambda = 582$, that of the petroleum $\lambda = 592$. Nearly all sources of illumination will be found to have a maximum lying between these two wave lengths.

A comparison of the total luminosity of the incandescent lamp with its candle-power shows that the former increases with the energy of the lamp more rapidly than the latter, although the discrepancy is not so marked as in the comparison of efficiency of candle-power with that of luminous radiation.

The following table, obtained by the summation of the luminosity curves, shows the relation between candle-power and total luminosity, in the case of the lamp studied by Schumann:—

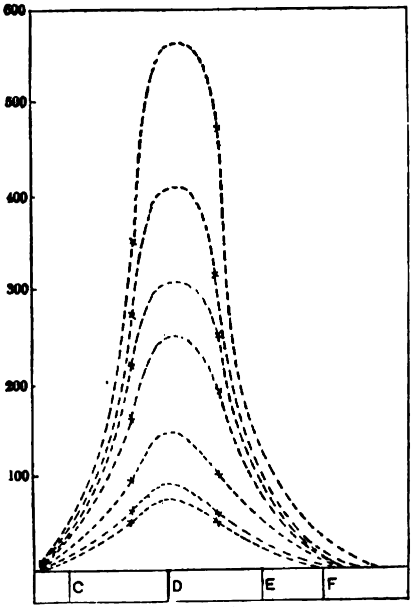

Fig. 12.

TABLE IX.

Candle-power and total luminosity of the incandescent lamp.

Watts.	Candle-power.	Total luminosity.
54.6	.....	4.11
61.3	.....	6.31
68.8	10.12	10.20
72.9	12.05	12.58
86.4	18.80	20.64
106.7	31.60	36.80
118.6	41.44	49.77
124.7	50.16	60.85
136.4	64.15	.....

Plainly then, candle-power does not afford a measure either of the light-giving energy or of the luminosity of a source of illumination. When used in the comparison of sources which differ in temperature, the indications of the photometer always yield us relatively too low a value for the intensity of the lamp of higher incandescence. In the case of candles *vs.* gas, or of either of these in comparison with the incandescent lamp at normal temperature, the error is not important, but between the candle and the "16-candle" lamp maintained at 50 candle-power, the discrepancy amounts to 10 per cent. For the electric arc the ratio of candle power to total luminosity is very nearly 4 : 5, and in the case of daylight it is about 4 : 6.

In arc light photometry, candle-power is already a well-nigh meaningless term. Would it not be well to abandon it altogether in favor of some standard affording us an expression for the luminosity of radiation? So far as the incandescent lamp is concerned, light-giving power is a perfectly definite function of the temperature and of the area of the radiating surface. We are unfortunately not in position at the present day to measure the temperature of an incandescent filament, but the time will undoubtedly come when the relation between the temperature and the quality of the light emitted by glowing carbon will be definitely known. We may indeed look forward to the development of some optical method for the measurement of the temperature of incandescence which shall be as easily performed as our present method of determining candle-power, and which shall admit of a much higher degree of precision. The comparison of lamps, the temperatures of which are the same, will then reduce itself to a question of radiating surface. Total luminosity and total energy of luminous radiation, per unit of surface, together with net and gross efficiency, may then be expressed, in so far as incandescent carbon is our source of illumination, as functions of the temperature alone, and the performance of any lamp may be defined in terms which admit of no uncertainty.

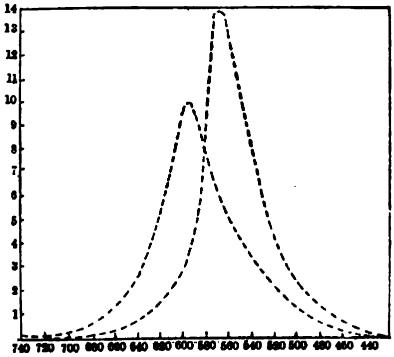

FIG. 13.

DISCUSSION.

THE CHAIRMAN:—Gentlemen, I am sure that you must have heard with the greatest pleasure the very able paper that has been read extemporaneously—if I may so express it. I have the paper before me, and I think that such a subject should incite us to a very full and free discussion. We have with us this evening a number of gentlemen who are interested in this question, either from a scientific or commercial standpoint, and a great many of them have already had some practical experience with regard to the fact that candle-power does not express the light-giving capacity of a lamp, and they have had some little difficulty in convincing city councils and other bodies of that fact. I think that such members should take an active part in the discussion, and I do not propose to stand in their way by occupying your attention longer.

MR. UPTON:—I listened with great pleasure to the paper of Prof. Nichols. I think that we have heard a paper which puts more clearly than it has ever been put before, the subject of candle-power. For the last few years there has been considerable discussion regarding candle-power. It is a matter which enters very largely into my business; and speaking of the difficulty of making good measurements, and of the meaning of candle-power, I might mention a story concerning a young gentleman whom many of you know, who was engaged to be married. He went out into the country and spent Sunday there, and, on coming back, innocently remarked that he had never noticed how much light a lantern gave until that Sunday. What Prof. Nichols has said as to the amount of light that a low candle-power would give for illuminating purposes is very true. For example, you take a fire, and you will notice how it will illuminate the buildings around, and yet the candle-power is low. Where you have a low glow from coal, the candle-power is very low, yet the general illuminating power is very large. As to the artificial or fixed method of rating candle-power in incandescent lamps, I consider that the present method of calling an incandescent lamp 16 or 20 candle-power is entirely wrong. Incandescent lamps are simply sorted. You put them in at 100 volts or 105 volts, so that they will be even if burned. That is all it means practically. Each consumer of light should fix the candle-power to suit the conditions. The marking that is now done on incandescent lamps is merely nominal; that is, it is in the hands of the parties who use the light to make the candle-power as much as they like. I wish to express my pleasure at the production of such a paper as this, because, it is of the class that we want. We want more of them in these meetings. It brings to us the results of laboratory work, such as is now being carried on at Cornell, and when we can get such papers, we can count ourselves very fortunate.

Dr. Otto A. Moses:—I, too, have listened with a great deal of pleasure to the paper and to Mr. Upton's comments upon it, and it carries me back to the day when the determination of candle-power was the all absorbing problem—the day when it was the question whether the incandescent light had any reason for existing or not. However, the concluding remarks of Prof. Nichols open up, I think, the proper field for us to cultivate to-night, and that is the method by which we can arrive at some photometric standard. The method that he proposes has one very great difficulty. He suggests the direction, but it is like pointing to the stars—you may go on to infinity. Carbon, which is the material now used for furnishing the incandescent filament, depends for its atomic condition upon the degree of temperature to which it has been subjected, that is to say, it varies in density and composition. This method struck me as we were listening intently to Prof. Nichols' remarks—if it were possible to surround the light-giving center with some spherical film of definite thickness and containing a certain quantity of a substance to be acted upon by the light, it would be possible by superposing those films, to get, I imagine, what would be the summation of the chemical actions on all the different colors in the spectrum from the dark rays up to those of highest incandescence. In that way, by dissolving out from this film the soluble portions and determining the insoluble matter, it would be possible, by a chemical method, to arrive at the total chemical action on all those films, and in that way make some definite approach to what is the real energy expended by the light. But it is a subject on which I think we can all say with Goethe in his dying moments, "We need light—more light."

Prof. Nichols:—If you are not all quite tired of me, I think I could perhaps make the last point a little clearer. The slide did not quite do its duty, I think, in bringing out the relationship between the various wave-lengths of radiation of four lamps all of which give the same candle-power, and from that I think I can indicate to you the line along which I would suggest a change of standard. I will draw a diagram which, perhaps, will be a little clearer. Suppose this (see Fig. 14) is the spectrum which is assumed to be of unit intensity throughout. This is the yellow (indicating); here is the red; out here is green, blue and then violet, which is the limit of the spectrum. Now, if you have a lamp of the same type of carbon, which corresponds to a 16 candle-power lamp of to-day; take this same lamp and run it at a temperature which corresponds to 4 candle-power to-day, and make that lamp large enough so that the photometer says it gives 16 candles, we find a curve like that which I had in my diagram, of this variety. Here are two lamps, one 16 and the other 4 c. p., and the photometer says that they are the same, but they are not the same either in total amount of energy, as one of my tables

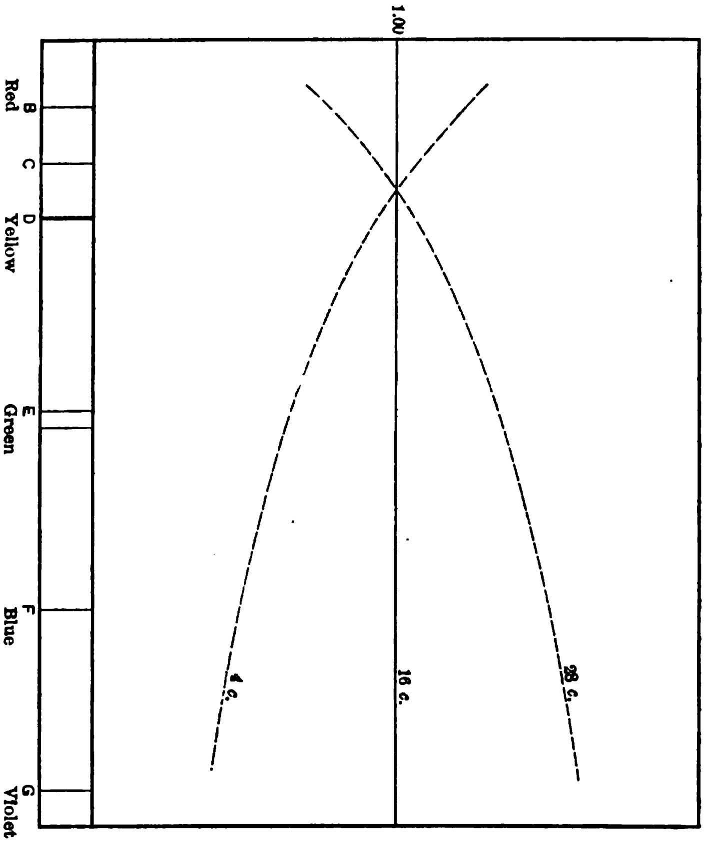

Fig. 14.

showed you, or in luminous effect; and if we run that on to 28 candles, we find the red very much weaker and the blue correspondingly much stronger. And there are three lamps, all of which the photometer says, give 16 candles, and yet they vary in total luminous energy and in total light-giving value as measured by their luminosity. At any rate, for a single type of carbon, prepared according to a certain process, and I believe it will hold through very closely of all filaments, if you know the state of incandescence, the temperature and the radiating surface, you have got a complete definition of the performance of that lamp, and you can then express exactly what it will do for any purpose for which you design it. If you want colors, you use it at high incandescence. If you want the greatest possible reading power, you will get a maximum for that purpose at a lower temperature. You may have lamps which will give very different values when measured in these more exact ways, and yet give the same value by the Bunsen photometer.

In the case of the 16-candle lamp raised to 50 candles, this error amounts to 10 per cent. As between the incandescent lamp of 16 candle and the arc light, the ratio between candle-power and luminosity is 4 to 5 nearly. When we come to compare candle-power with daylight, one is half as good again as the other, and the ratio is 4 to 6, That is, a man who would give us 16 candles of a light like daylight would be giving us practically half as much again, or 24 candles. Light of the quality of commercial arc light, is as 5 to 4, that is, 16 candles of that light are worth 20 candles of the incandescent lamp. He who brings his incandescent lamp to a state which corresponds to 50 candles gives us about 10 per cent. more, measured according to the luminosity, and yet we still are satisfied with the candle as our measurement of the performance of lamps.

MR. JOSEPH WETZLER:—Prof. Nichols has outlined in a very good suggestion, the method which he would pursue to get at the correct method of measuring the candle-power of the lamp, and his method would consist in obtaining the radiating surface of the filament and its temperature. I would like to ask if he has attempted any practical way of carrying that out. Naturally it would seem that the determination of the radiating area of a filament would be easy to accomplish, knowing its size and shape, and then would come the question of the temperature of the filament. It strikes me that if we knew for any particular carbon, the rate of variation of resistance for increase in temperature, that the latter might be comparatively easily determined, I would like to know if Prof. Nichols has worked in that direction. We would have merely to measure the resistance of the carbon by a method similar to that of the Siemens pyrometer to obtain its temperature, and then, having the temperature and the area we could get at the value which he suggests.

PROF. NICHOLS:—I would say, in reply to Mr. Wetzler's suggestion, that I have been very much discouraged from attempting that, on account of some very singular results which Prof. Anthony obtained some years ago, and which I think he reported to the Institute at that time,[1] in the matter of the behavior of incandescent lamp carbons as to resistance at high temperatures. Supposing that we have a curve along which the resistance is falling off; we should expect it to go on falling off according to a well defined law; but he came across a number of lamps in which this curve turned upward, and at a certain point you would have a very high temperature and a low temperature at which the resistance would be the same. Phenomena of that kind rather deterred me from that method of getting at the temperature of the carbon. But I think there is a hope of getting an optical method, an instrument by which you can look directly at the light and ascertain its temperature in degrees centigrade. I think it would be premature to speak of that now, possibly at some future meeting I may be able to report progress on it.

DR. MOSES:—This is an interesting subject and there are those here who can give us some information which will lead us towards the proper solution of it. Mr. Upton, I am sure, will be able to answer some of these questions for us. In the manufacture of carbons, for instance, from bamboo, in which he perhaps has had more experience than any other living man, the carbons are manufactured of certain dimensions. They are then submitted to approximately a definite temperature, but it is found that they vary considerably in their resistances. Nevertheless, being of the same dimensions, and the carbons, when rendered incandescent, being brought up to a certain definite temperature, irrespective of the resistances in the beginning, it would seem when definite amounts of current were passing, that in spite of this variation in resistance you would finally get the same amount of light, having these dimensions and the same material to act

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[1] Transactions American Institute of Electrical Engineers, Vol. IV. p. 198.

upon and the same temperature ultimately; the temperature in this case being brought about by the current. Now, if Mr. Upton would tell us about what would be the variations in carbons when of the same dimensions, when any given lot of them are put into lamps, if there would be a variation of as much as 20 ohms.

Mr. Upton:—The ohms vary, of course, from accident. The one constant is the candles per horse-power.

Dr. Moses:—But a definite fact is the amount of current that you allow to pass through them in testing them for their illuminating power. If you were to allow a definite number of watts to pass through those lamps, would you not approximately get the same amount of illumination irrespective of the initial variation in resistance? It is a problem which it seems to me would interest you. You may have tried it and solved it already.

Mr. Upton:—You are stating in a few words a pretty large problem; that is the trouble. Of course, the question of the quality of the carbon comes in, in a very large degree. That has been shown again and again. The one constant, as I said, is the candles per horse-power. The carbons will vary, of course. Referring to what Prof. Nichols says about the falsity of photometric measurements, many of us know that where lamps run a little bright a greater effect would be obtained for certain purposes far in excess of the increase of candle power. That has been noticed in store lighting where colors come in, and Prof. Nichols has explained it. There is another point about lighting that was not mentioned and that is the physical effect, we used to notice that. The moment you get lighting up to a certain brightness, it has a cheering effect. If you have it dull, it has the opposite effect, though both may be giving the same bulk of candle power. I hope I have answered your question.

Dr. Moses:—You have answered it, I think, quite correctly, when you say that we can ultimately get the same quality of carbons, if we carry the temperature of the carbon and keep it there for a time very much higher than the temperature at which the filament was originally carbonized. That is to say, when you take definite dimensions and bring them up, say to a power of 48 candles, and keep them there for some time, you then, so to say, cook that carbon and get just the very condition at which you can make these tests. Now, then, by passing a definite amount of current and bringing the temperature as closely as we can calculate up to 48 candles, would you not then get the same amount of candle power from all of these? That question answered would decide whether it was possible to use this filament as a photometric standard. That was the point we were originally discussing.

Mr. Upton:—As I said in reference to using a filament for photometric standard, carbon varies in the character of its surface, and you must specify your carbon and specify its carbonization before you can take it as a standard. You take a black carbon or gray carbon, or polished carbon and all the grades running between those, and you will have a variation in candle power. The dimensions do not fix the candle power. It is the quality of the surface that fixes the candle power to a large degree.

Dr. Moses:—But may we not get that quality of surface by bringing it to the same high intensity, by using a definite amount of current, and then determining whether those carbons do not furnish at the same amount of horse power consumed, the same amount of light.

Mr. Upton:—No. Take a thin flash carbon; you bring that up high and it has changed its quality; and the power it will take to give a certain light will change. Incandescent lamps depend on the surface, temperature does not fix it, as far as I know. Of course the temperature changes with the cooling effects; and to measure the temperature of the filament itself is a very difficult matter. I do not know how to do it at present.

Dr. Moses:—The meeting must pardon my keeping up the conversation, but I am reminded of Dr. Draper's experiments—the classical ones—where it was necessary to bring the object up to about 1,000 degrees of temperature before it became luminous. Now, we have got our conversation up to that point, and I hope we shall continue it in order to get results from it. Nature indicates a very definite chemical composition in her vegetable products. It is true any fibre will vary in different parts of the plant, and there will be a difficulty again; but since Mr. Upton, I believe, is careful to have them all from the same radial distance from the axis of the plant, on that account we may consider that the material is homogeneous and uniform. But when we raise a body to a given temperature we drive off those substances that may be distilled off, and if we finally get a definite amount of temperature, entirely irrespective of its composition in the beginning, and then put a definite amount of current through a definite parallelopipedon of carbon, we ought to get a definite amount of illumination. Now, then, would not the same cross-section and hence the same areas of similar carbons give exactly the same amount of light? My reason for pressing the question is to determine whether the suggestion of Prof. Nichols may not ultimately be taken as the definite photometric standard. Now the platinum method has obvious disadvantages, from experiments which have become classical, made at the laboratory at which Mr. Upton assisted, where platinum was discovered to be constantly changing its condition in a vacuum and constantly giving off its surface under the bombardment of the particles combined with some action of the current, and it is not by any means constant. So that carbon may be a better material than platinum. Nevertheless they, and all other substances are varying constantly. So that it is scarcely to be hoped that it will be ever taken as a standard. It is, however, so easy to obtain these standards, and they can be so easily renewed, that perhaps they may be ultimately adopted as such.

Mr. Mailloux:—The constitution of carbon, as it occurs in the incandescent lamp, is to a large extent an artificial one, and one which is dependent greatly upon the method of treatment. You may say, for instance, that the value of a carbon filament would depend, other things being equal, on the density of the carbon. No doubt that density has some relation to the color of it and that again the color has some relation to the condition of the surface, whether it is granular, and whether it is crystalline, and whether it is smooth and polished. Now, it occurs to me that it may be that this variation of resistance in the incandescent lamp filament is one. which instead of being abnormal, is perfectly normal and characteristic of carbon in a highly incandescent state. May it not be that the resistance of carbon varies with its density and that this attains a minimum at a certain point. I am brought to this idea by a certain analogy with substances which are perfectly familiar to us—electrolytes. I have had considerable experience in electrolysis, and have had to determine the specific resistance of a great many substances, and I find that a great many—I will not say all—but a large number of chemical substances used in electrolysis have the very peculiar property, that there is a particular density at which they give the minimum resistance. For instance, with sulphuric acid, which is so extensively used now-a-days in storage batteries, the curve is a very peculiar one; a very small increase at first in specific gravity, where you start with pure water, will make a very large decrease of resistance, but at a specific gravity of 1.2 you have reached very nearly the minimum, and beyond that the resistance increases, and finally, when the sulphuric acid is at its maximum density, the resistance will become quite high and the curve presents a very striking analogy indeed to that which Prof. Anthony presented before the Institute in his paper referred to by Prof. Nichols. Moreover, it is found that each class of salt—take the sulphates for instance—gives a series of lines which have the same general character of curvature, and no doubt they are amenable to one general form of equation, and may it not be that we have here to deal with a phenomenon which is entirely normal, and that when we once determine the particular law which would appear to be involved here, we would have right there a means of measuring the efficiency of the lamp. Having found an expression for the law which connects the density of the carbon with the temperature or with the resistance in some way, we shall have made some step toward the finding of a practiced method of gauging the efficiency of incandescent lamps. As I understand it, Prof. Nichols states that the ideal method would be one which would be based on the radiation of the energy from the filament of the incandescent lamp itself, and if that were the case, it looks to me as though you must look right there for the path leading to the proper solution.

DR. WHEELER:—It seems to me that the most important measurements to be made are of actually existing lights—not ideal ones; and for that reason I would like to know if Prof. Nichols thinks that the sums of the photometer readings of a light in different parts of the spectrum represent its real value.

PROF. NICHOLS:—Mr. President, as I understand it, we have to multiply the photometer reading in each part of the spectrum by a factor which itself is a function of the wave-length and which expresses the light-producing power of that particular wave-length. I can, perhaps, illustrate this by means of a curve. Suppose you have a light of this kind. (Illustrating.) That is the distribution of energy in the spectrum from the violet to the red, and you take the sum of those wave-lengths right down. The integrated value of this energy, that is the area enclosed by this curve, is the quantity which I speak of as the total luminous energy or radiation; but that is not the light-giving power.

The light-giving power varies enormously in different parts. Now, the product of the ordinates of the two will give you the total light-giving power of the spectrum. Out of the red we get great energy and little light-giving power and at the end of the spectrum our luminosity ceases. We have to take, in the case of each wave-length, the product of the amount of energy and its luminosity as determined by one of the methods described; that is the power which that particular wave-length has in enabling us to read fine print. If you take the blue light and yellow light in the spectrum, and bring them to the same brightness so that to the best of our average judgment we would say that they were equally bright; then take fine print and put it in the yellow, you can read it there, but put it over in the blue and you cannot read it. In other words, the power of the blue to enable us to distinguish print is less. It is of no value, practically, for that purpose. The rays which enable us to distinguish black characters on a white ground are located in that half of the spectrum which lies toward the red, and the rest might as well be thrown away. In almost all uses of the electric light the feature which Mr. Upton spoke about comes in, namely, that indescribable value, that quality which brings out the colors in the art galleries and in our rooms. That lies largely in the violet, so that I think the *value* of light increases with its incandescence faster than its luminosity does. It is a factor which must be determined for each case according to the purposes for which you intend to use the light.

Dr. Moses:—It is a sad thing for one to admit that we have to determine all these questions ultimately by an imperfect instrument. The eye has to be the final umpire of these measurements, and we can scarcely consider the action of the eye as an accurate means of determining this question. If the eye is used under certain circumstances in direct light we find that it undergoes a kind of paralysis; there is a neutralization of light upon the retina, so that our own powers of observation are affected by these varying conditions, and I do not see how it is ever to be possible for us, except by using special determinations for special purposes, to arrive at a result. I have many a time seen the cobblers in Germany sewing, their stitches being exactly in the focus of a globe of water, with the smallest kind of a farthing dip at the other side, giving a concentration of light at the very point at which it was needed. They got a better light for their purpose than had they been working in the full glare of an arc light. We have to consider the conditions, and it is there we have ultimately to determine according to the necessity of the case.

MR. DOBBIE:—I would like to ask Prof. Nichols if he has thought of a photographic method of determining this. There are three points in the spectrum which could each be determined by a plate specially prepared for it. For instance, a dry plate can be dyed by aniline and made sensitive to one particular ray. As the question of lighting is a commercial question, might it not be a practical thing to determine the power of the lamp in a special part of the spectrum by a plate and give it that value and state in what part of the spectrum its value was given for. I believe that the yellow is most commonly used, but for photography there would of course be a call for the shorter wave lengths of the blue rays.

MR. BIRDSALL:—I have given some thought at different times to photographic methods of measuring light. The fact that I have also dabbled in photography has been rather discouraging. As it is quite well known that ordinary silver salts are more sensitive to the blue and violet rays than to others, and almost unaffected by the dark red rays, that would seem to give us a means of measuring the most desirable rays for colors, namely, the violet rays; but it is also a fact that right good photographs have been taken in almost absolute darkness to ordinary eyes. In other words, the violet rays past the visible end of the spectum have the power to affect the sensitive plate, and in that way in Germany photographs have been taken at night; that is, at night when there was no starlight of any kind, it being fairly cloudy, in which the conditions were those of almost absolute darkness. Speaking about the varying density of carbon and the great variety of effects you can get from apparently uniform treatment and conditions, all photographers know that gelatine has the same elegant property; with the same treatment you can get quite varying results within 100 per cent.

MR. MAILLOUX:—I would differ with Dr. Moses as to the eye being made the ultimate arbiter of the efficiency of the incandescent lamp, and I believe that it will be found possible to measure the energy of the incandescent light by other means. I have great faith in the calorimeter, and I believe strongly in the method which was ontlined this evening by Prof. Nichols. I do not see that there are any physical impossibilities lying in the way of our arriving at such a method by which we can differentiate the various kinds of light, and get the sum of the different kinds and find out the law connecting them together. Having done this, then it occurs to me that we could express the efficiency with reference to the properties for which the light is to be used and that we could have a sort of *modulus*, as it were, by means of which the varying conditions of the use of the lamps would be taken into consideration in estimating their particular efficiency.

A MEMBER:—It strikes me that what we want to get at is a practical method of determining the value of a lamp for every day use. We can't use fine methods. It amounts mathematically to trying to fix a curve with one point while we want to know three points of that curve at least. If we have some practical method of determining the three points of that curve instead of one, the matter would be solved for all practical purposes. Suppose we select the line in the spectrum. That is, practically, what is selected now. When we use the candle the other two ends of the spectrum are allowed to go up or down just as they please. Perhaps by some method the two ends of the illuminating power might be fixed. For instance, in determining the illuminating power, instead of selecting a white glass, select two others and give the candle-power through three instead of one.

DR. WHEELER:—I do not think that I made my question quite clear. I would like to try it again. In measuring the power of the light I understand, of course, there are three sets of rays, one of heat and one of light and one actinic, and to know how much illumination the source gives, we do not care anything about the first or the third, what we want to measure is the second set. Now you get the value of say the yellow part. Then, if you run the incandescence of that lamp up, may you not get a less proportion of yellow and a larger proportion in some other parts of the spectrum, so that while the yellow would read lower, yet the total amount of illumination would perhaps be the same?

PROF. NICHOLS:—I think one of the slides which I showed answers the question—that one deduced from the experiments of Schumann, in which it is found that as the incandescence increases this curve simply rises—one curve over another in this way (illustrating), but not strictly so that the line passing through the tops is a vertical line. There is a continual tendency for the line to work off this way, and if you were to carry it on to a degree of incandescence equal to that of the sun, you would then find the effect just described, that is to say, these curves would lie side by side; but, of course, the range of intensity with which we deal with artificial lights is very much less than between the sun and any artificial lights. Now, the difference of wave length between these maxima has been determined; in the sun it is 582, and in this case (indicating) it is only shifted to 592, so that for the rays of incandescence which we have to deal with in artificial light, you may say that the whole thing rises together.

THE CHAIRMAN:—It is long past our usual hour for adjournment, but I think we have very far from exhausted the subject. It is in fact impossible to begin to exhaust such a subject in an evening's discussion, even so full and able a discussion as we have had. I think we appreciate our own ignorance in dealing with such a question. It is remarkable how far in advance our own speculations and ideas of the subject are, of the ideas which are ordinarily entertained by the public, and while listening to the discussion this evening as to candle-power, I was reminded of an incident that occurred last week in Bristol, Pennsylvania, where one of the large drug stores of the town was illuminated by candles at the time of the starting of the incandescent lamps. The druggist had heard that the incandescent lamps were of 16 candles, and in order to show how poor and insignificant the new illuminant was, he decorated the whole of his window with 16 candles. The result of the illumination was not as great a success as he anticipated, and he was overwhelmed with offers of the loan of snuffers on all sides. Although the incandescent lamp compared so favorably with the candles that were shown by the Bristol druggist, I think we have before us a great deal to do with the improving and perfection of our arc and incandescent lamps in order to secure a greater efficiency and in order to be less wasteful. The lines of one of England's great poets occur to me in that connection. You may remember them; they are those in which Tennyson says:

> "Our little systems have their day,
> They have their day and cease to be,
> They are but broken lights of Thee,
> And Thou, O Lord, art more than they."

And when we can get our own broken lights somewhat nearer the perfection of daylight we shall have done some of the work which awaits us. Of course, Tennyson, in speaking of "Our little systems" having their day, did not have in mind the systems which we see in such rapid process of consolidation with other larger systems. (Laughter.) At the same time I think he hinted at a very great and singular truth.

Adjourned.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

SOME RESULTS WITH SECONDARY BATTERIES IN TRAIN LIGHTING.

BY ALEXANDER S. BROWN.

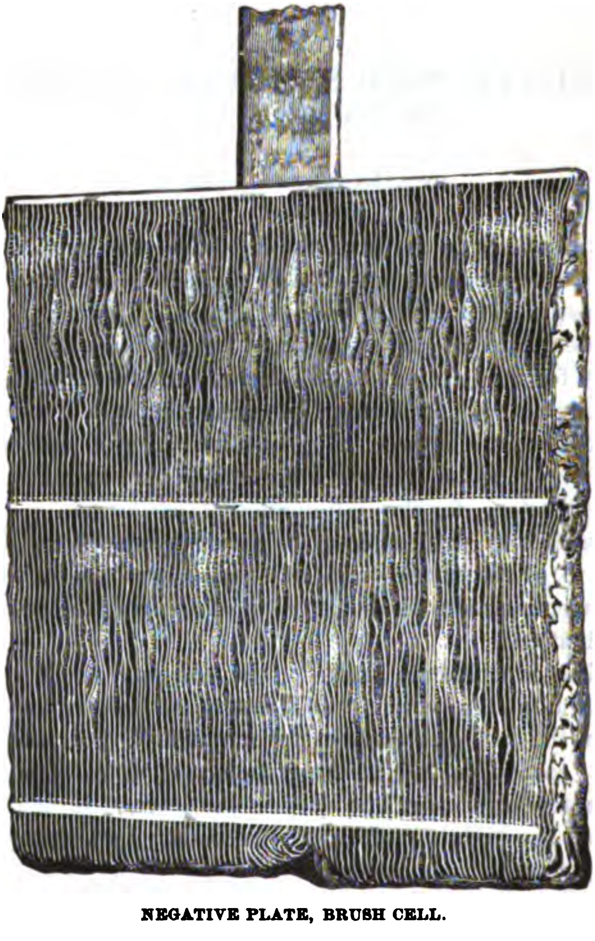
The arguments in favor of the incandescent electric light for railway passenger trains are so well understood by the members of the Institute, from their own general knowledge of the subject, as well as from papers previously presented, that it is unnecessary for me to allude to its admitted superiority over either gas, oil or candles. It is now four years since the Pennsylvania Railroad Company, appreciating the advantages of electric lighting for this purpose, began a series of experiments with the secondary battery, for the illumination of parlor cars.

I am unable to give the cost of lighting by any of the batteries referred to, as the expense appears to have been a secondary consideration compared with the necessity of securing the best system of illumination.

The first experiments made by the company were with the Faure accumulator, which was brought to this country from France in the spring or early in the summer of 1882, and put on passenger coach No. 397 in August of that year. The car was run for a few days only, but enough was learned in that time to show that the lighting of cars by secondary batteries was perfectly practicable, when a modified form of the battery could be obtained.

During the summer of 1884, experiments with the Brush storage batteries were commenced at the Altoona shops of the Pennsylvania Railroad, and continued through the fall and winter of that year. These experiments were made by Dr. Dudley, and proved so successful that eight parlor cars which were being constructed at that time were equipped for incandescent lighting, and put into regular service on the 1st of April, 1885.

The Brush cells used consisted of three plates each, two negative and one positive, the amount of surface of the positive plate being equal to that of both the negatives. These plates differed from those now used, there being no paste in their composition. The negative plate shows the form of construction, and was designed to give the greatest possible amount of surface from the least weight of material. The positive plate measured 8 × 9 × 1 inches, and the negative, 8 × 9 × ½ inches.



NEGATIVE PLATE, BRUSH CELL.

These batteries were rated at seventy ampere-hours, but the hard usage they received soon reduced their capacity, and at the end of a year's time the positive plates were almost entirely destroyed. In fact, I have taken plates out of the cells which had holes in them fully four inches in diameter, while the deposit in the bottom of the cell would be from an inch to an inch and a half in depth. The plates also had a decided tendency both to buckle and grow, and in a number of cases it was a very difficult matter to remove the plates without injury to the cell; you will understand, of course, that I allude more particularly to the positive plate. The weak points in the Brush battery were the small number of ampere-hours per pound of material—about two, I believe. There was a tendency to buckle and sulphate, and the arrangement for connecting the plates was such that there was oxidation at the junction, causing the plates to break when subject to the motion of the car. This last feature was a source of endless trouble to us, and as is usually the case, was sure to happen at a time or place where it was impossible to repair the break. While using the Brush battery the cars were wired with the battery boxes, one on each side of the car, coupled together in series, each box holding three trays, and each tray containing four cells, this giving a total of twenty-four cells to a car, or a current of forty-eight volts. The lamps used originally were of the Brush-Swan type, with small platinum loops projecting from the base, these hooking into a socket and held in place by a spiral spring, this socket fitting into another one, to which the connections to the wires were made.

After using this style for some time an improved pattern of Brush-Swan lamp was substituted; but the great drawback to this lamp was its frailty, the breakage per day often averaging four per cent. of the total number of lamps in use on the cars. These lamps required about forty-five volts and one ampere of current, and each car contained ten lamps, seven in the body, one in the smoking-room and one in each vestibule, thus using about ten amperes of current; and as the batteries were rated at seventy ampere hours, we should have obtained from six and one-half to seven hours' good light, but it was very seldom, after the batteries had been used for a short time, that we could get over five and one-half hours'. As will readily be seen, this could hardly be called economical, for to obtain five or six hours' light we were obliged to charge from fifteen to twenty hours with a Brush arc light dynamo giving a current of about ten amperes. Then taking the weight into consideration, each time the car needed charging, the trays, six in number and weighing 1,200 pounds, had to be loaded on the truck, pulled over to one side of the car, and the discharged trays taken out and the freshly charged ones put in their place. The remaining three were then taken around to the other side of the car and changed in the same manner. As the cars invariably stood on a double track, it was necessary to carry half the trays for some distance, over tracks, etc. Then, again, the tracks were usually full of cars, and if there was not sufficient time for them to be parted the batteries had either to be carried under the car or over the platform. Under the present system this has been done away with to a great extent, as will be explained subsequently. During the month of December, 1886, we commenced using one battery of the 7 B type, manufactured by the Electrical Accumulator Company. This battery was a decided improvement over those previously used, giving, as it did, nearly double the number of ampere-hours for the same weight of material. After using this battery for about a year, the positive plates began to give way, and new ones were substituted, the negatives, however, being retained, as they had not deteriorated. In fact, we are still using the negative plates of our original 7 B battery. This battery proved to be so satisfactory that in June, 1887, we added two more of the same type and two manufactured by the Julien Electric Company. The latter batteries were similar in size and shape to the 7 B type of the Accumulator Company, but did not seem to stand the hard usage so well, as they would crack instead of buckling and allow a large quantity of the paste to become loosened and fall out. It is proper to state, however, that these plates were made especially for us, to fit the cells we were using, and were not of the regular Julien type. This, no doubt, will account in a great measure for their failure to keep up to the standard of the present type of Julien batteries. After the positive plates of this battery became worthless, and not having any others to put in their place, we decided to make a kind of consolidated affair, and see how Julien negative and Accumulator positive plates would work together. The result was extremely satisfactory, and we have had no trouble with them whatever, excepting the occasional buckling of a positive plate.

During the fall of 1887 we began substituting the Accumulator batteries for the Brush batteries, and by the end of the year were using them altogether, with the exception of the two Julien I have mentioned. We, however, kept ninety-six of our best Brush cells, to be used in case of an accident to the incandescent dynamos, which are used to supply the current for the railroad telegraph as well as for lighting the passenger station.

After the change in batteries had been made, it was thought best to change the system of wiring the cars, so that instead of its being necessary to put batteries on each side of the car, we need only put them on one side and get the same amount of light. This was done by connecting the two boxes under the car in parallel, and substituting 23-volt Edison lamps for our 45-volt Brush-Swan lamps. After doing this we found that we were able to get as many hours' light from twelve cells as we formerly did from twenty-four. Of course this change reduced the expense of lighting the cars considerably, for where we formerly had a change of battery for each car, or forty-eight cells, we now needed only twenty-four cells per car, with a few extra ones to be used in case the twelve cells would be unable to supply sufficient light for a long trip. This system of lighting proved so successful that it was decided to substitute it for gas in the remaining seven parlor cars. The wiring of these cars was commenced at the Meadow shops, near Jersey City, last September, and has just been completed. These cars are arranged for the 23 C type of the Accumulator Company's battery and 19 B type of the Julien battery. The battery boxes under the cars are larger than the old style, and contain four trays or sixteen cells, the lamps we used taking twenty-three volts of current. Changes were also made the first of the year in the system of charging, and the batteries are now charged from an incandescent dynamo instead of an arc, thus enabling the batteries to be charged with any amount of current desired. In connection with this description of the four years' work of the Pennsylvania Railroad, I would like to add something in regard to the electric lighting of the Chicago limited express.

In 1887 the Pullman Palace Car Company commenced using the electric light on one of three trains of the New York and Chicago limited express, and the charging of the batteries at Jersey City was given into the hands of the Pennsylvania Railroad Company. The batteries used were the 7 B type of the Electrical Accumulator Company, thirty cells to a car, and usually there are six cars in the train. These batteries would furnish enough light for one trip, and each time the train arrived at either terminus it was necessary to replace them with freshly charged cells. This was not objectionable at Chicago, for the train arrived there in the morning, so that they could be charged during the day; but in Jersey City, where the train did not arrive until evening, the work was very difficult on account of the dynamo being in use for other purposes. After this one train had been running a short time, another one was fitted up, 19 B type of Julien battery being used. It was thought that this would give us a chance to see which was the better of the two batteries, but they both worked very well, considering the treatment they received. After Mr. Bauer became connected with the Pullman Company he did away with charging the batteries at the ends of the line, and introduced a Brotherhood engine and Eickemeyer dynamo in the baggage car of each train, in order that the batteries might be charged in transit. When the train leaves Jersey City the baggage car is on the rear, and consequently no charging is done until it leaves Philadelphia, where the train is reversed, bringing the baggage car next to the locomotive. The batteries are charged continuously until the train reaches Chicago, and the same practice is continued on the return trip. The steam for driving the Brotherhood engine is taken from the locomotive boiler, and the exhaust is used for heating the train.

So far as the Pennsylvania Railroad is concerned, the electric lighting of passenger trains may be considered an assured success, and leading, as it does, to the abolition of kerosene lamps, and incidentally encouraging the adoption of steam heating, the safety and comfort of the passengers are certainly increased.

DISCUSSION.

MR. GEO. B. PRESCOTT, JR.:—I think Mr. Brown's paper is very interesting, and I am sure the Pennsylvania Railroad Company deserves a great deal of credit for making the experiments. I think that these tests have demonstrated one thing very positively, and that is, that an accumulator made entirely on the Planté principle, without active material applied according to the Faure method, is an impracticable battery for commercial purposes. The improvement in storage batteries of late years, and particularly in the past year, has been very great, and the practical use of batteries shows that unless they are handled as if they were a delicate piece of machinery they will never give very satisfactory results. Wherever accumulators have been properly installed and have been treated as if they were worthy of being properly cared for, they have been very reliable, and they certainly fulfil certain conditions that no other electrical machinery does to-day.

The improvements in the use of electrical accumulators are not confined to the cells themselves. No matter how good an accumulator may be, if it is not properly installed with suitable apparatus for regulating both the charge and discharge, it will never give satisfactory results. If the accumulator is overcharged, the paste is peroxidized to such a high degree that it becomes a very fine powder, without sufficient consistency to hold together, and it gradually deposits in the bottom of the jar. That comes from overcharging, and yet it was only a year ago that it was thought that overcharging improved accumulators. We know now also that if the cells are allowed to overdischarge they become very heavily sulphated, and the longer they stand in a discharged state the more dense this sulphate becomes, and then, if the charging is commenced at what would otherwise be the normal rate, the gas seems to be developed within the interstices of the pellets, and as the sulphate is not in a condition to take up the oxygen it is liberated, and the result is that these pellets are forced right off the perforations in chunks, so to speak. If, however, the cells are charged and discharged within certain limits, which can be determined for every size of cell, and if any short circuits that may occur through accident or other cause are removed at once, the accumulator, to-day, is a perfectly reliable piece of apparatus.

MR. C. O. MAILLOUX:—I would like to ask Mr. Brown whether the charging of accumulators at either terminus of the road has been definitely abandoned in favor of the system of charging on the train. I ask this question because I know that on some roads there is a very great and decided objection to deriving the energy for charging the batteries from the locomotive; that is particularly the case with the Boston and Albany Railroad. I once had occasion to discuss the matter with the President of the Company, and he would not entertain at all any project for charging the batteries on the road, for the reason, he said, that they needed all the energy, whether they used it in the form of steam or derived it from the axle—they needed it all for propelling the train itself, their object being to make the shortest time.

I would like to ask another question about the latest modifications. Mr. Brown said that a smaller battery would do the work. I do not exactly understand this, unless he used lamps of higher efficiency, giving the same candle power with a smaller expenditure of energy. I would like to have him elucidate that matter for us.

Mr. Alex. S. Brown:—Mr. President, in regard to the charging of batteries on a car or at the terminus, you will understand that the Pennsylvania Railroad still charge at Jersey City. It was the Pullman Company I alluded to; they have done away altogether with the charging at either terminus, with the exception of keeping a few spare cells both at Jersey City and Chicago, so that in case the train arrives in Jersey City in the evening, and anything is the matter with the cells, or the trays, we simply have a good tray ready to put in its place.

In regard to the lamps. I spoke of using a battery with a less number of cells. When we were using 24 cells of the accumulator battery we would get about 14 or 15 hours' light, using the Brush-Swan lamps of 45 volts. When we changed and commenced using 23 or 24 volt lamps and 12 cells we got about 7 hours' light. You see it was really about the same thing, but we found it was easier to change the cars a little bit oftener and use a less number of trays, and at the same time we had to keep a less number of cells on hand in the battery-room. We found it more economical in both ways to work in that manner.

Mr. Wm. H. Peirce: I would like to ask the weight of those cells per lamp, and if there have ever been any experiments tending to show the quantity of steam used.

Mr. Brown: I really cannot say very much about the Pullman work, because I am not connected with that in any way now. I do not know just how much steam they require. But in regard to the other matter—the lamps—I supposed it to be a 16-candle power lamp; that is what they are held up to. Of course they may fall off a little, but when the battery is properly charged, they seem to hold right up. We have no method of testing in Jersey City, but that is what they are rated at. The weight of a cell is about 50 pounds.

Mr. Peirce:—That feeds how many lamps?

Mr. Brown:—We use 10 lamps on a car. The weight of battery on a car is 600 pounds; we use 12 cells.

Mr. Prescott:—Mr. Mailloux referred to the objection that he had heard made by some railroad company to making use of steam for driving the dynamo. I think that objection is not very well founded. We all know that the use of steam for heating cars is being universally adopted, and some steam could be used for driving a small engine to run the dynamo and the exhaust used for heating the car. On the other hand, the amount of steam required to drive the small engine is such an almost infinitesimal part of the total amount of steam generated in a locomotive boiler that it hardly need be taken into account.

PROF. EDWARD L. NICHOLS:—I should like to inquire of the author of this paper whether he can tell us anything about the average life of accumulator cells used in railway service as compared with the life of the same type of cells doing the same amount of work when stationary. I have had this question asked me many times and have looked in vain for definite information concerning it.

MR. BROWN:—I am sorry that I cannot answer that question, because I have never had any experience with stationary cells. My experience has been confined entirely to railroad cells. We calculate our positive plates will last about a year. They are in use almost all the time. Of course they have a great deal more usage than the stationary cells, although I do not think that the motion of the train does them any harm at all. We do not find that the batteries splash any. Of course there is some little motion, but being in the center of cars about 65 feet in length, the motion there is very little, so I do not think that does any harm.

THE PRESIDENT (Prof. Elihu Thomson):—Mr. Vansize can probably give us some information on the point just raised.

MR. W. B. VANSIZE:—As to the President's suggestion, I would say that the details under consideration come more within the line of practical every day contact with the work, with which I have had very little experience. I have heard it suggested, however, that there might be some advantage in the motion on a railroad car, because the agitation of the liquid would make a more even dispersion; that is, the specific gravity would be more equal at all points of the cell, and while there might be some detrimental results from the shaking to which the material was subjected, the difficulty in that direction would be perhaps more than made up by the uniformity of the electrolyte. I do not know that any tests have ever been made to determine this question.

Prof. Nichols asked the relative duration of a cell whose life was spent upon a car as compared with that used in stationary work. I do not know that it has been determined; but the general opinion is, so far as my experience goes, that the railway cell has a somewhat abbreviated life. The varied attention which the two classes of cells receive would also determine that result to a certain extent. A cell in use on a railroad train would naturally receive a little more careful attention than a stationary cell, unless a system of inspection was in use in connection with the latter.

MR. MAILLOUX:—I may be able to give some information in that direction by reference to the experience on street car lines. The storage battery is never put to a more crucial test than it undergoes upon a street car. My own experience and observation lead me to believe that a battery submitted to the same rates of charge and discharge would last longer on a car than in stationary places. I have seen the same battery when used in stationary work and when in use on street cars and also in railroad cars. In street cars the work was always necessarily much more severe. There were times when the battery might have to withstand a current rate of from 50 to 100 amperes; while in stationary work the same battery would never have a current rate exceeding perhaps 25 to 35 amperes; and yet, even under those very trying conditions, I have known of batteries, the positive plates of which were still quite good after fifteen months' use—almost daily use. For a long time the plates were used daily at least once, and sometimes charged twice a day, and yet after fifteen months' use the active matter was not sufficiently gone; in other words, the plates (of course I refer to the positive plates, because the negatives were nearly as good as ever) were still perfect enough to do good work, and this would show that there must be something in the vibration or the shaking of the cells which counterbalances somewhat the hard usage which it receives in the way of excessive rates of either charge or discharge. The particular battery which I refer to was once discharged on short circuit while on the road by the bursting of an armature. One of the cars was going down the steep grade in the upper part of Madison Avenue and the man thought it would be splendid fun to coast down hill. I suppose he must have developed a speed of 20 miles or more, and one of the armature bands ruptured, and being only a street car man he did not seem to think that that made much difference—perhaps he did not notice it. When he got to the bottom of the grade he very innocently turned the switch to put on the current. The car did not seem to move very fast, and he was made conscious that something had happened by the conductor telling him that something was smoking. The trap door was removed, and he found that one of the motors seemed to be burned up. So the car was brought back. It was an experimental car, and upon investigation I found that the current rate of discharge had been sufficient to burn up one of the connecting strips by which the circuit was closed from cell to cell. In other words, one of these strips had acted as a fusible plug, and from its area of section I concluded that the current rate must have exceeded 300 or 400 amperes. I also found that the field wire, which was 4½ B. & S. gauge wire, had become heated sufficiently to char the insulation almost the whole length, so I concluded that it must have had a fair rate of discharge. I naturally thought that the batteries would be ruined, but I had them taken out and cleaned, and to my great surprise I found that while there was some active matter in the bottom of the cells, it was not at all, in quantity, what I expected. In fact, it was so little that I did not know how much of it was due to that immediate circumstance and how much was due to previous wear and tear. At any rate, the batteries were put into the same car, except two of them, which were ruined by the melted lead flowing out where the circuit was broken by the melting out of the safety plug. Shortly afterwards the same battery was again short-circuited by some accident which occurred in the station itself while making connection with the machinery. It was short-circuited in such a way that only the field wire of one of the motors was put across the terminals, and the connection seemed to be better this time, and we did not know of it until we heard a violent hissing noise. I jumped on the car—it was one o'clock at night—and struck a light, and noticed that the solution was boiling in several of the cells; so I very quickly opened the circuit and we found that the battery had been discharging at an excessive rate. Again I thought the cells might be ruined, but I had them washed and cleaned, and they did a good deal of work after that. This goes to show that in the first place storage batteries will stand a much higher rate than generally supposed, with comparative impunity, and it corroborates perhaps the belief which I have that when once the active matter of a storage battery has become thoroughly recep tive by a proper course of treatment either originally in the making or construction of the battery or subsequently in the nursing or treatment of it, that you can do with it a great deal that would ordinarily be considered as abuse.

THE PRESIDENT:—As we have another paper on the storage battery, perhaps it would be well to have the paper read before continuing this discussion. We have with us Dr. Duncan and he will give us a paper on the inherent defects of lead storage batteries—a subject which cannot fail to be of great interest.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

THE INHERENT DEFECTS OF LEAD SECONDARY BATTERIES.

BY DR. LOUIS DUNCAN AND H. WIEGAND.

Since the year 1881, when the inventions of Faure gave such an impetus to the industrial development of lead secondary batteries, their commercial history has been marked by numerous and disastrous failures, while at the same time there has been a steady improvement in their construction and performance until at present they have reached a stage which makes them for some purposes a commercial success. At the same time there remain in the best batteries a number of defects which prevent their taking the place in the practical development of electricity which rightfully belongs to them. The action of the secondary battery is quite well understood. In the Faure type a support plate, usually made of lead or an alloy of lead, has mechanically applied to it some salt of lead (minium or litharge). A number of such plates are placed in dilute sulphuric acid, the alternate plates being connected respectively to the + and — poles of some source of electricity, and a current is sent between them. The result is a reduction on the positive plates (the plates through which the current enters) to Pb. O_2, on the negative plate to spongy lead. After this "formation" the action, roughly speaking, consists in a reduction of both peroxide and spongy lead to sulphate of lead on discharge, while on charging they are reduced again to their original composition. We find that in the process of charge and discharge there is a loss of energy varying from 15 to 40 per cent. within the practical limits of discharge rate. If we calculate the theoretical storage capacity of a given weight of lead and peroxide, we will find that the plates of even the best modern batteries weigh for the same capacity ten times as much as would those of a theoretically perfect cell. We will find, too, that there is a constant depreciation, especially of the peroxide plates, the rate of depreciation increasing with the rate of discharge, and in general depending partly on the way in which the cell is treated, partly on its construction. The principal defects of the modern lead secondary battery are (1) the comparatively small storage capacity, (2) the loss of energy, (3) the depreciation, (4) the low discharge rate necessitated by considerations of efficiency and depreciation.

It was especially the question of the loss of energy in the battery that we wished first to investigate. There are two factors which determine the extent of this loss: in the first place, the number of ampere-hours obtained on the discharge of the cell is less than the number put in; and in the second place, the P. D. at the terminals is greater during charge than during discharge. This loss of energy exhibits itself in two ways—in a generation of heat, and in chemical actions which are not reversed on discharge. It is well known that after a cell has been in use for some time, especially if it be submitted to rapid charge and discharge, there will be found in the bottom of the containing vessel a white, powdery deposit, a sulphate of lead which has been formed from the active material of the plates and which has not been afterwards reversed. Again, when the cell is charged we find bubbles of gas escaping from the plates during almost the whole of the charge, the escape becoming quite violent towards the last. The escape is at first principally from the positive plate, but afterward it is from both plates. This escape represents, of course, a loss of energy due to the electrolysis of the dilute acid in the cell, the products being free hydrogen and oxygen.

Let us consider what takes place during the charge and discharge of a cell. Supposing the cell to have been discharged until its P. D. has dropped to 1.8 volts, on beginning to charge the P. D. increases until it reaches a value of about 2.1 volts—at a normal charge rate,—then increasing very slowly during a considerable portion of the charge, then increasing faster until it reaches a value of from 2.4 to 2.5 volts, when the cell is "boiling." The chemical action results principally in the reduction of the sulphate of lead on the two plates to peroxide and spongy lead respectively. The greater the charge rate the higher will be the P. D., and the sooner will the cell begin to boil, and the greater will be the loss.

On discharge the P. D. drops to from 2 to 1.95 volts for normal discharge rate, where it remains during the greater part of the discharge, there being a gradual fall during the latter part to 1.8 volts, when the discharge should cease If a high discharge rate be employed, there is a decrease in the capacity and efficiency, and a more rapid depreciation. If the discharge be continued after the P. D. has dropped below 1.8 volts, there will be a formation of white sulphate on the plates, there will be a loss of energy, as will be shown, and there will be a rapid depreciation of the cell. The result of the discharge is a formation of sulphate of lead on both positive and negative plates If we test the specific gravity of the solution at different times we will find that the solution has a maximum strength—say 1.200—when fully charged, with a minimum on discharge—say 1.150,—the sulphating of the lead decreasing the strength of the solution. The number of ampere-hours obtained on discharge is less than the number put in by an amount depending on the construction of the cell and the conditions of charge and discharge. There is a further apparent loss of energy in the fact that the electromotive force on discharge is less than that during charge.

Our first experiment was made to determine, if possible, whether part of this difference of E. M. F. was not due to the fact that the strength of the solution *in the plugs* varied, it being stronger during charge than during discharge. During discharge the sulphuric acid in the plugs has its strength decreased by the sulphating of the lead or peroxide. This weakening continues until the diffusion of the stronger acid in the cell produces a condition of equilibrium. It is known that the electromotive force of a cell varies with the strength of the solution, being higher as the strength increases. Gladstone and Tribe have found that when the acid is very weak the chemical action is changed, the result on a positive plate of sheet lead being the formation of streaks of a mixture of yellow and puce-colored oxides, while on other parts a white substance is formed, which is easily detached, falling in clouds into the liquid. This white substance is probably a basic sulphate of lead. When this action takes place the corrosion of the plate is more than doubled. So if the diffusion in the plug is slow, it may very well happen that there will be a great difference of density during charge and discharge, causing a difference in electromotive force and a formation on discharge of chemical compounds which are not afterwards reduced. A rapid discharge rate would tend to greatly weaken the acid, and therefore to decrease the efficiency and hasten corrosion of the positive plate.

To find the rate of diffusion we soaked the plates or single plugs to be experimented on in acid of a specific gravity of 1.175, and then placed them in vessels of distilled water, letting them remain for different intervals of time, and determining the amount of acid diffused out into the water. To give some idea of the magnitude of the result, I select the following figures from a number of experiments. The plates used weighed about a pound and a half (.7 kilos.), and were of the grid type:—

POSITIVE PLATE.

CHARGED.		DISCHARGED.
Time in water.	Acid diffused.	Acid diffused.
1 min.	.695 grms.	.29 grms.
5 "	1.41 "	.60 "
30 "	2.50 "	1.43 "

NEGATIVE PLATE.

CHARGED.		DISCHARGED.
Time.	Acid diffused.	Acid diffused.
1 min.	.86 grms.	.317 grms.
5 "	1.42 "	.700 "
30 "	3.05 "	1.35 "

The curve shown in Fig. 1, gives the diffusion from a charged positive plate.

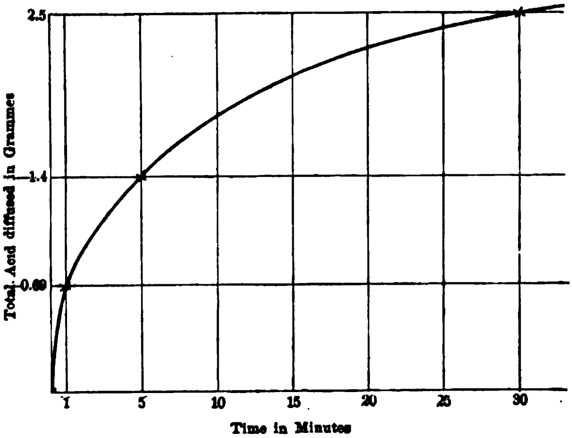


Fig. 1.

The total amount of acid in the charged positive plates was

(25 grms. of 1.175) about 5 grms.; so it will be seen that the rate of diffusion of the acid in the interior of the plugs is slow, for after 30 minutes, when half of the acid remains in the plug, the rate of diffusion has decreased from about .7 grammes for the first minute, to about .025 grammes per minute. While this is hardly the condition of affairs in actual practice where the rate at which the acid is being added or abstracted varies in different parts of the plug, yet it gives us some idea of the magnitude of the quantity. It should be noted that the rate of diffusion is materially the same for positive and negative plates, but that the rate for a discharged is considerably less than that for a charged plate. Keeping these facts in mind, let us pass to the phenomena of charge and discharge.

To investigate the loss of energy from heating, we placed the cell to be experimented on in a wooden box lined with a layer of felt about an inch thick. There was a top for the box, also lined with felt, and through it passed the rod of a stirring paddle and the stem of a thermometer. Experiments showed that the loss of temperature in this arrangement was, for a

Difference between air and box.	Degrees loss per Degree diff. per hour.
7.2°	.12°
5.1	.1
4.2	.95

In the experiments we tried as nearly as possible to keep the air and cell at the same temperature, and the correction for radiation could usually be neglected. The cell was charged and discharged under a number of conditions, and the rise of temperature and other data were observed. The losses of energy that occur must, as has been stated, exhibit themselves in heat or in chemical changes which are not reversed. The cell was of the grid type, with 4 positive and 5 negative plates. The weights were:—

Total weight	25 lbs., 8 oz
Weight of plates	17 " 8 "
" " vessel	1 lb., 3 "
" " solution	6 lbs., 8 "

This gives a neat capacity for the cell such that the energy lost is approximately 3.2 watt-hours per degree.

The cell was first charged at 5 amperes until over 150 ampere-hours had been put in; both positive and negative plates were boiling freely. The discharge was at a rate of about 15 amperes. Some of the particulars of the discharge are:—

Ampere-hours discharge.	Rate of rise of temp. per amp.-hour.	Total rise of temp.	C^2 R.
10	.012	....	....
20	.01	....	....
30	.008	....	....
40	.015	....	...
50	.03	....	....
60 P. D. falling.	.05	2.03	11 watts.
70	.12	....	....
77	.30	3 75	18 watts.

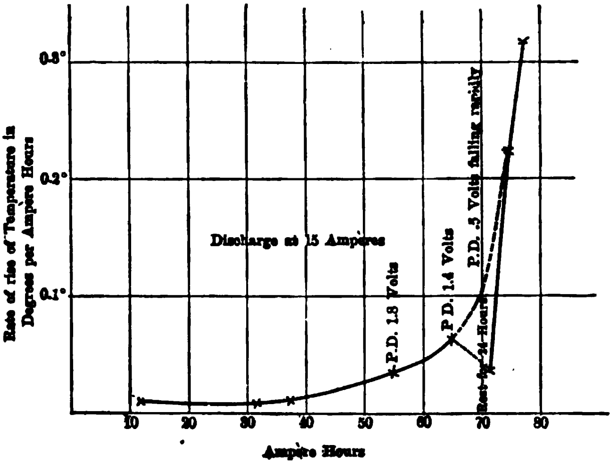


Fig. 2.

In this case the discharge was carried far beyond the limit of economical discharge. It will be seen that the loss is greater as the discharge continues, increasing slowly until the E. M. F. begins to drop, when it rises very rapidly.

After this discharge the cell was charged at a rate of 10 amperes.

	RATE OF RISE.		
Ampere-hours.	Degrees.	Total rise.	C^2 R.
20	.04	....	....
40	.04	....	....
60	.05	....	....
80	.08	4.7	8
100	.166	8.9	....
120	.20	11.5	12

The cell thus charged was discharged at a rate of about 20 amperes. The rate of rise of temperature was very much as in the case of the discharge at 15 amperes. The total rise, with the P. D. down to 1.7 volts, 60 amperes having been taken out, was 1°.1; on further discharge down to .7 volts, taking about 20 ampere-hours more from the cell, the rise was 4.1°. This again shows that the loss increases as the P. D. falls.

The next charge was at 20 amperes. The rate of rise of temperature is given by the curve, Fig. 4. The total rise was 30.7°. The rise for 100 ampere-hours was 11.5°. The corresponding values of C^2 R. were 34 watts and 20 watts.

The effect of a rest is shown on the portion of the curve between 105 and 120 amperes, where a 16-hour rest gives a considerable reduction in the rate of temperature increase. The maximum rate is about .3 degrees per ampere-hour.

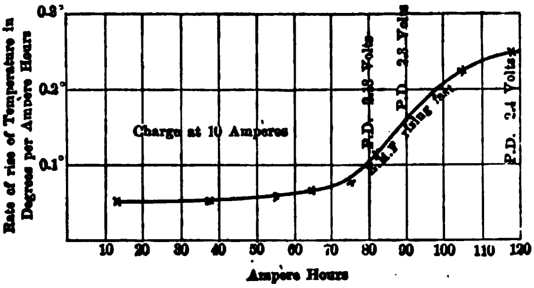


Fig. 3.

The cell thus charged was discharged at a rate of 30 amperes. The discharge was divided into periods of 20 minutes, with periods of equal length between, the object being to determine, if possible, whether there is a local action in the mass of the plug due to the different chemical conditions of the different parts of the plug, which would especially be the case if the charge or discharge were rapid. As in the previous case of discharge, the rise of temperature was slow, there being a gain of but 1.3° for a discharge of 40 ampere-hours, the heating effect of the current in that time being 12 watts, equivalent to almost 4°. For the first three periods there was little, if any, rise during the time of repose; for the fourth period there was a rise of about .1°, or .3 watt hours. During the fifth period the E. M. F. began to fall, and during repose after it the temperature rose .15°. The sixth discharge period was with the same current, but at a greatly reduced P. D. and resistance. It lasted 12 minutes; after it the temperature rose .5°, corresponding to 1.5

watt hours. The output of this discharge was 44 ampere-hours at a normal P. D.; 11 at a low P. D. The total rise of temperature was 3.5°; the value of C^2 R was 16 watts.

The cell was then charged with 70 ampere-hours at 30 amperes, the same period of repose being allowed. There was a definite rise amounting on the average to about .2°; after the first period the rise was but .1°, the others slightly over .2. The details of the charge were:—

Ampere-hours.	Rise in temp.	Rate.	C^2 R
15	1.	.067	4.5
40	4.4	.18	11.5
70	13 1	.29	20.5

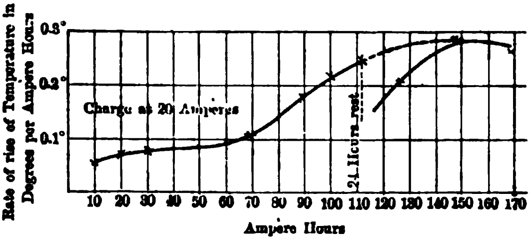


Fig. 4.

The details of the discharge at 30 were:—

Ampere-hours.	Rise.	Rate.	C^2 R
10	.14	.014	3.2
25	.62	.032	7.7
35	1.02	.04	10.7
45	1.62	.06	13.7
54 P. D. fell.	2.62	1.1	16.5

The rise during the periods of rest for this discharge was at first small, but afterwards something over .2°.

The next charge was at a rate of 40 amperes. The following are the figures:—

Ampere-hours.	Rise of temp.	Rate.	C^2 R.
20	1.5	.075	8.
43	3.9	.11	15.
63	8.0	.20	22.
80	13.2	.3	28.
97	18.65	.32	34.
	Rest.		
106	21.35	.30	36.75

The rise during periods of repose was .1° for the first period, .7° for the second, and decreased as the charge increased until it reached a value of about .2°.

The discharge corresponding to this charge was also at 40 amperes, and was divided into periods as before.

Ampere-hours.	Temp.	Rate.	C^2 R.
14	.46	.083	5.
28	1.16	.05	11.
42	2.24	.077	17.

Whether the rise in temperature during the periods of rest is due to the heat of the plates being diffused into the liquid, or is due to a local action in the plugs is difficult to determine. If it were due to local action we would expect a greater rise during the first periods of the charging experiments, and this is what we find. If it were due simply to diffusion of heat, we should have approximately the same effect at corresponding periods of charge and discharge, the rates being the same, but we see that it is greater during some of the charge than during corresponding discharge periods, and it is not uniform even during charge and discharge, it being sometimes greater when the resistance of the cell is less. Some of the rise is undoubtedly due to the diffusion of heat from the plates, but it seems certain that a part of the effect is due to the local action.

Our next experiments were with negative and positive plates which were in different conditions of charge and discharge. At first a cell was made up of fully charged negative plates, with positives from which 45 ampere-hours had been taken, and it was discharged in the calorimeter at 10 amperes.

Ampere-hours.	Rise.	Rate.	C^2 R.
4.2	.05°	.012°	.5
17.5	.20	.011	1.8
Rest.			
35.8	.45	.009	2.6
49.1	.85	.03	4.0
66.1 P. D. fell.	3.05	.13	6.0

Next a cell with fully charged positives and negatives from which 47.5 ampere-hours had been taken, was discharged at a rate of 5 amperes.

Ampere-hours.	Rise.	Rate.	C^2 R.
10.	—.1°	—.01°	.5
15.5	—.15	—.01	.75
20.5	—.20	—.01	1.00
23.	—.20	—.00	1.1
29.	—.20	—.00	1.4
34.	—.20		1.65
39.	—.17	+.008	1.9

Ampere-hours.	Rise.	Rate.	C^2 R.
41.5	+.05	+.08	2.05
Rest.			
47 5	+.28	+.04	2.45
49.5 P. D. fell.	+.50	+.11	2.55
Cell short-circuited, for very small current.			
1 hour.	+ .80		
6 hours.	+ 3.00		
11 hours.	+ 5.2		
22 hours.	+11.2		
23 hours.	+11.8		

After this exhaustive discharge the cell was charged at five amperes, attempts at discharge being made at intervals. At first the P. D. fell at once on closing the discharge circuit, but after about 16 ampere-hours had been put in, a discharge of five amperes for eight minutes was obtained. The temperature, which had been rising quite fast, began to rise slowly at this point, the rates being given by the curve.

The cell boiled with a charge of 117 ampere-hours, when the total rise of temperature was 96°, C^2 R = 7 watts. For a charge of 160 ampere-hours the rise was 17.3° with C^2 R = 9 watts.

Another discharge of a negative cell which had 58 ampere-hours taken out gave:—

Ampere hours.	Rise.	Rate.	C^2 R.
9	0°	0°	.5
22.2	.39	.019	2.0
31.0 P. D. fell.	.61	.026	2.5
34.2	.90	.09	2.7

This gave the total capacity of the negative plates at a discharge rate of five amperes as about 90 ampere-hours; the previous experiment gave 93. The capacity of the positive plates was 94 ampere-hours. It was noticed in these experiments that when a partly run-down negative was used, the fall of potential after a value of 1.5 volts was reached, was very rapid. From 1.3 to .5 volts took less than a minute, while in another minute the value had reached .25 volts, and it was soon only a few hundredths of a volt. With the discharged positive, on the contrary, the fall was slow. After a value of 1.35 was reached, the fall in a half-hour was only to 1.04 volts, and in four hours it was .66 volts. After several hours short circuit, when the value had reached a few hundredths, the circuit was broken. In five hours the value was 1.65, and the next day it was 2.02 volts.

With some plates made by depositing thin layers of spongy lead and peroxide on lead support plates, the effect was still more marked. Here the P. D. of a negative plate fell from a value of 1.8 to .05 in ten seconds; while with the positive the fall was gradual.

Now let us see what our experiments, as far as we have carried them out, show. In the first place the loss that exhibits itself in heating increases as the charge or discharge goes on, not being very great in the latter operation until the P. D. has begun to fall. The loss is greater during charge than during discharge. For instance, take a discharge at 15 amperes, the P. D. begins to fall when 60 ampere-hours have been taken out. The total rise in temperature is 2.03 degrees, while the rise due to the heating of the current alone, ($C^2 R$) should be 3.4°. In other words, there has been an absolute lowering of the temperature by all the other actions outside of the Joule effect. Taking a charge at 10 amperes, the rise for a charge of 100 ampere-hours was 8.9°, with a loss due to the Joule effect corresponding to 3°, a difference of 5.9°. The difference in the losses for charge and discharge—neglecting $C^2 R$—is about 5.3°. For a charge of 120 ampere-hours, the rise was 11.15°.

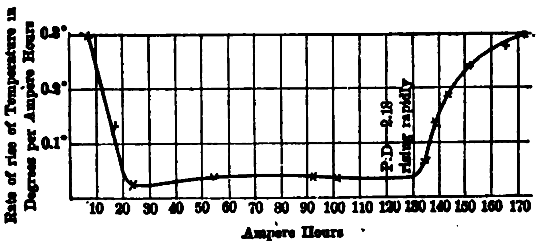


Fig. 5.

We will find the same results in all of the other records, there being sometimes an absolute fall of temperature on discharge. This is without doubt due to the fact that the acid is strengthened on charge, weakened on discharge. In changing from a specific gravity of 1.14 to 1.18, the evolution of heat is such as to raise the temperature of the solution about 3.2; there will be a corresponding cooling effect on discharge.

To the above charge, at 10 amperes there was a discharge at 20 amperes, which gave a total of 84 ampere-hours, of which 60 were at normal P. D.; the rise for normal P. D. was 2°; the total rise was 4.7°. $C^2 R = 12.1$ watt-hours for the 60 ampere-hours at normal P. D., corresponding to 3 8°; the total value of $C^2 R$ for the 84 ampere-hours was 15.1, corresponding to 4.7°. The total heat loss in the process of charge and discharge was 11.5 + 4.7 = 16.2°, or about 51 watts. The total loss obtained from taking the difference between charge and discharge energies, calculated from the ampere-hours and potential difference, was 98 watts, more than half of which appeared as heat. Of the 51 watts which appeared as heat, 27 were due to the Joule effect, 24 to other causes. We believe that part of this loss—a very small part—is due to local action between the positive material and the support plate. Another part is due to local currents in the plugs themselves. When the plate is charging or discharging, the distribution of current in the plug is not uniform, but it is denser at the surface than in the interior. After a while the plug is not uniform in its chemical composition, and there are doubtless eddy currents in such a way as to tend to bring the plug to uniformity of chemical condition. These will be more important as the current rate increases. The last part of the charge of the cell consists largely in the electrolysis of the dilute acid with a liberation of oxygen and hydrogen. It is known that heat is generated in this process, the amount depending on the density of the acid and the nature of the electrodes. M. Gramme in some experiments on the electrolysis of water, only utilized 50 per cent. of the energy expended in producing electrolysis, the remainder being lost. In our own case, when the charge has reached such a point that the reduction of the plates is complete, and the principal action is the electrolysis of the solution into free hydrogen and oxygen; if the total P. D. is 2.5 volts, and if the energy of combination of oxygen and hydrogen corresponds to about 1.5 volts, we may expect of the energy of 2.5 watt hours, corresponding to a charge of 1 ampere-hour, 1 watt-hour to appear as heat. If we look at the tables, we will see that the rate of rise of temperature for overcharge is in the neighborhood of .3° per hour per ampere, and this corresponds to about 1 watt, as we would expect. As this action (shown by the evolution of gas from the plates) continues through the whole of charge and part of the discharge (doubtless due to local action in the latter case); we will always have *some* corresponding rise of temperature, although it will not reach its full value until the cell is boiling.

The rise of temperature then, is due to:

(1.) The Joule effect, 27 out of 51 in this case.

(2.) Currents caused by local action between active material and support.

(3.) Currents caused by local action in plugs.

(4.) Heat losses corresponding to electrolysis of solution into free oxygen and hydrogen.

The last three probably account for the remaining 24 watts.

But there are 47 watts left unaccounted for, which must be due to chemical changes not reversed. The most important of these, *as far as loss of energy is concerned*, is doubtless the formation of free hydrogen and oxygen. Another component of the 47 watts is due to the local action between the plugs and support. But we believe the most important, as far as *deterioration* is concerned, is in the formation of irreversible compounds caused by the weakening of the acid in the plugs. We have found that the rate of diffusion in the plug is comparatively slow, so that during a rapid discharge there must be weak acid in the plug. Where the outer layer of active material is reduced, the inner layer is surrounded by weak acid, causing a lowering of the E. M. F. and corrosion, as described by Gladstone and Tribe, with a considerable local action between the outer and inner layers. As the rate of diffusion is less with a partly discharged than with a charged plate, a heavy discharge rate has a more marked effect with a partly discharged than with a fresh cell, as experience has shown.

These are, we believe, the principal sources of loss in a lead secondary battery, and the depreciation can be mainly charged to them. The expansion and contraction of the plugs in a grid plate is another source of trouble, and cannot be avoided. Our own experience has been that the losses are less with plane plates, coated with thin layers of active material, than with plates made of the grid form. Under any circumstances, an increase of surface for a given current rate is to be desired, as it lengthens the time of charge before the violent boiling occurs. A plane plate gives a uniform distribution of current, and, therefore, very little local action.

DISCUSSION.

THE PRESIDENT:—I am sure we have all listened with great attention to Dr. Duncan's paper, as it brings forth a number of points which I do not think have been given the prominence which they deserve, in relation to the storage battery, particularly the point of the existence of eddy currents due to the difference of chemical conditions of different parts of the plug; it also fixes the losses just where we would naturally expect to find them. There is one point I might simply ask information on. I understood the Doctor to say that there was a continual and rather large loss after the charging was completed.

Dr. Louis Duncan:—Not after the charge is completed and the current shut off, but when the action is simply the electrolysis of the solution.

The President:—I wish to offer one more remark—that possibly part of the loss may come in the recombination of the ozonized oxygen with the free hydrogen, producing heat in the solution itself.

The paper is open to discussion and we should be pleased to hear from any members on any of the points touched upon.

Prof. E P. Roberts:—I would ask if it was a constant rate of charge, or if the charge gradually tapered off as the battery filled. I was not quite sure when Dr. Duncan spoke of plates that merely had a face action instead of plugs, whether they heated comparatively more or less than when there was plug action.

The President:—I understand the inquiry to be whether there was more or less energy in the flat plate with the coating of active material laid over it than when the plugs were used? Is that it?

Prof. Roberts:—Yes, sir.

Dr. Duncan:—Well, the discharge and charge rates were always the same. As for the flat plates, I experimented on some plates which were made by depositing low and pure oxide on plane sheets of lead from a caustic solution of litharge. In that case with a very thin layer, about a quarter of an ounce to the square foot of spongy lead and a corresponding amount of peroxide, the efficiency of the cell at a discharge rate which corresponded to that of the Julien cell, 25 amperes for a surface of about 7 feet, I think, was about 90 per cent. But, of course, in that case the proportion of active material to total weight was very great. There was only a very thin layer and the distribution of chemical action was uniform and there was comparatively very little loss. A part of that could be accounted for by the electrolysis of water.

As for the heating effect due to the electrolysis, I think Prof. Thomson's suggestion is a very good one, but we did not attempt to find out what that heating effect was due to. It ought to be in about the same proportion, if the electromotive force is 2½ volts, then for one ampere hour the loss is 2½ watt hours. If the electrolysis itself takes up 1½ of the 2½ volts, then the other actions, whatever they might be, should give a loss of 1 watt hour. I simply put that 1½ down to loss from heating effect. But it is probably due to the cause that Prof. Thomson gave.

Prof. Roberts:—I asked that because of an impression I got from some experiments, though I did not make enough of them to be sure it was the fact. I also happened to think of the practical bearing of the fact of the electromotive force rising after a few moments of rest. I happened at one time to have some storage batteries in my hands and the lights would get very bad at night, and the next morning it was a very nice thing, when the customer complained, to turn the light on and say, "See, the lights are all right and up to full candle power."

Mr. Townsend Wolcott:—I understand Dr. Duncan to say that the chemical action of the plug begins at the surface of the plate.

Dr. Duncan:—I think it does, yes.

Mr. Wolcott:—As far as my experience goes, if you take the ordinary form of plug, (Fig. 6.) the grid is so much better a conductor than the active material that the latter starts to form right around the edge, and the last place that it is formed is right there in the middle. The thing is to get that middle formed up as good as the other part. I think that is where a good deal of loss comes in.

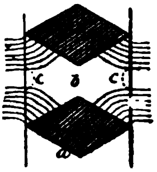


Fig. 6.

Dr. Duncan:—That probably is where that action does take place.

The President:—On the point just mentioned it would, of course, appear that that plug is not as good a conductor as it ought to be. If it were a very good conductor, then we would have it even up at once, even though it might not take place from actual conduction of the current to the plug.

MR. WOLCOTT:—That emphasizes the point that Dr. Duncan mentioned about a very thin film uniformly spread on a flat plate—that the action is equal all over. Of course, the current would come right straight out. There would be no spots.

MR. MAILLOUX:—There is one point regarding the rate at which batteries charge and discharge, which I think has a great deal of influence on the efficiency, and it also has some interest industrially. In order to get the most of a storage battery we want to charge it at such a rate that we should get the maximum efficiency, so as to lose as little energy as possible in electrolysis. On the other hand, the circumstances of its use dictate that the time expended in charging the battery should be as small as possible. Now, I think that it is possible to satisfy both requirements, because I have found by experience that a battery will stand during the first period of charge a very high rate without evolution of gas, and, if watched, it is possible to charge the battery in the minimum time, and also, probably, with as good efficiency as in any other way, by always keeping the current at such a rate that it will be just below the point where it causes evolution of gas. I have in this way made it a practice to begin charging batteries whose normal rate of charge would be say 15 amperes with as high current as 50 amperes, which could be kept up for an hour or two. Then, perhaps, 30 for another hour; then 20, then 10, then 5, and sometimes I would finish with as low as 3 amperes. Now, in that way the battery was never allowed to evolve gas, and I think that that is the best way in which to treat the battery, for the simple reason that it gives the energy as fast as the receptive matter can receive it. In charging, the surface probably becomes oxidized first, and the inside may become oxidized by a kind of electrolytic conduction in the plug itself, and you must give it time for the actions to take place, and by pursuing such a method you allow the necessary time, and do not evolve the gases. The condition which Mr. Prescott has called to our attention is a well known one—namely, the continued evolution of oxygen on a peroxide plate, after the battery is charged, disintegrates the active matter and makes the peroxide into a fine granulated form, which is not coherent, so that it is very undesirable to prolong the evolution of gas. On the other hand it is desirable to charge the battery fully. There is no way of doing it satisfactorily but by reducing the rate of charge towards the close of the charge.

Mr. Prescott:—I would like to ask Dr. Duncan if he took into account the difference in the internal resistance of the cells owing to the different state of charge or discharge in order to calculate the dual effect, and whether he took a mean of a few extreme readings or whether it was determined at uniform intervals.

Dr. Duncan:—It was determined every five or six minutes and the effect was calculated at corresponding times. The resistance for full discharge was sometimes five times as great as it was for full charge.

Prof. Nichols:—I wish to emphasize what seems to me the importance of the point brought out by Mr. Mailloux a moment ago, and the importance of the experiment Dr. Duncan has described. It seems to me that this is the only way in which this great number of perplexing questions that have arisen can be satisfactorily answered, and I think the sort of experiments described in this paper, made patiently and persistently, will finally give us complete control of this matter. This question of time is, of course, a very important one in the matter of charging. It would seem perfectly practicable to determine a rate to give the proper charge in the minimum time, never at any time giving it more than it can safely take, so as to complete the charge in the smallest possible interval. However, I think that this has more to do with the economic question of the time of charging than it has to do with the efficiency, and I think so, because in some recent tests which we have been making, they were really practice tests, but they brought out this point very nicely; the battery was charged first with uniform current throughout, at a rate not such as to produce marked evolution, then discharged and its efficiency determined. The same battery was then charged with a tapering current, starting off with a large amount and making the average current for the whole time of charging the same as before. The efficiency was found to be within one per cent. of what it was before. Finally the same battery was taken again and charged intermittently with uniform current and then breaking and allowing it to stand for half an hour, the impression being that there might be a reversible reaction which would take place if the battery had an opportunity to readjust itself between these charging periods. The battery was brought to the same condition and again discharged continuously, and all three of these efficiencies came out within one or two per cent. of each other.

Mr. Francis B. Crocker:—I would like to ask Dr. Duncan if the loss of energy by the evolution of gas could not easily be determined by collecting it. He refers to it as an unknown factor in the loss of energy. It seems to me the number of unknown factors could be reduced by determining the amount of energy the gases represent. The point which the President brought out in regard to the recombination of the oxygen and hydrogen would be eliminated also, because the gases collected would represent the actual loss of energy from the evolution, whereas the recombination would be heat, added to that produced in the battery. It seems as if one of the rather numerous factors of loss could be fixed in that way.

Dr. Duncan:—I think I was misunderstood if it was supposed that I said that the loss due to electrolysis of the solution was not a definite quantity. When you electrolyze a solution of sulphuric acid you get a definite amount of oxygen and hydrogen. That has been done by Gladstone and Tribe, and they find that it is a definite amount. But accompanying that is another loss. Gramme found that in electrolyzing dilute acid, the efficiency of the process was not much more than 60 per cent. under the most favorable conditions. It is not the loss due to electrolysis that is at all questionable. There is a heating which goes on entirely independent of the $C^2 R$ effect, and what that heating is, I don't know. Probably what Prof. Thomson suggests will account for some of it. I do not think it would account for all, because most of the calculated amount of gas was collected by Gladstone and Tribe in their experiments.

Mr. Mailloux:—I think that in considering the work lost in electrolysis we must allow for the amount of heat which must be abstracted from the surrounding medium by the expansion of the gas. We must remember that as it is evolved into a gaseous form it expands, and in doing so it would necessarily absorb energy from surrounding substances, and as this energy must be provided by the circuit itself, it would probably react on the circuit in such a way as to cause either an increase of resistance or of counter-electromotive force. There are only two ways in which energy can be abstracted from the circuit, just as in the case of a transformer or dynamo; either by inductive resistance, in the form of counter-electromotive force or by ohmic resistance. I might point out that the very able and interesting researches of

Dr. Duncan will greatly facilitate the mathematical treatment of the storage battery, which thus far has been waiting for just such researches. If we consider the storage battery as a transformer, it is very easy to point out its analogy to other forms of transformers, and while generalizations are not correct, absolutely, to their full limits, yet there are a great many important points of resemblance. For instance, the induced currents and the local actions which Dr. Duncan has described, are very closely analogous to the currents of self-induction, while the losses due to heat, for instance, are analogous to the increased resistance due to heat and the consequent loss, and I think that before long somebody will give us analyses of the principles governing the action of a secondary battery, with as much clearness and exhaustiveness as we have had in the case of the dynamo or of the transformer

Mr. Prescott:—There is one point that I consider of very great importance which Dr. Duncan has passed an opinion on, to the effect that the active material applied to the surface of the plate produces better effects than when applied to a perforated grid. I should like to ask him what he considers the surface of the plate. Is it the surface exposed to the acid or is it the surface of the oxide in contact with the lead? Mr. Wolcott has made a diagram on the board, which shows that the most effective surface is that in contact with the metal itself, and if that is true, why, I should think the objection to the grid form would fail.

Dr. Duncan:—While action takes place all along the grid, yet if the diffusion is not very rapid the action at one point is taking place in comparatively strong acid, and the action at another point is taking place in comparatively weak acid. At the same time the condition of those points is different, therefore the loss will be different. With two plane sheets, one opposite the other, then the density would be exactly uniform, supposing the conductivity of the support were great enough. As far as the efficiency goes, the only experiments I have made, as I have stated, were with the deposit of peroxide upon plane lead plates from a solution of litharge caustic. If you take a very concentrated solution of caustic soda and potash and put in litharge, and boil it, you get on the lead plate a deposit of spongy lead, which will be a half inch thick for one quarter of an ounce. Then you can press that until it is about one-hundredth of an inch thick. You get about six-tenths of the theoretical capacity of the lead when it is compressed with a pretty fair pressure.

And it was with such plates that I got a higher efficiency than I did with grid plates, because their action is perfectly uniform and the thickness is so small that there is hardly any chance of eddy currents.

Mr. Prescott:—Therefore it is possible that the same method of preparing the plate if applied to the grid might have given the same result.

Dr. Duncan:—The great point is that every part of the plate is in exactly the same condition. The distance from the other plate is exactly the same. Whereas in the grid plate there is a very great difference, and consequently the distribution of current is not the same.

Mr. Prescott:—The modern practice is to increase the separation of the plate very much more than has ever been done in the past, and also to make much thicker plates and to make smaller perforations. The result is that the difference in the distance between one plate and any portion of the interior of the perforation of the opposing plate is almost inappreciable. That is the modern tendency.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, with discussion thereon.

ELECTRIC MOTOR REGULATION.

BY FRANCIS B. CROCKER.

Although this subject is one of great scientific and practical interest, it has not been very frequently or very fully treated up to the present time. Of course, motor regulation is very similar to dynamo regulation, but it is by no means identical with it, and it deserves and requires separate treatment. I do not think it is much exaggeration to say that at the present time there is no subject more generally important to electrical engineers than motor regulation. Considering this fact, it is certainly remarkable that the literature of the subject is so limited. We find stray articles in electrical periodicals, rarely more than a description of one particular invention, and in electrical books, regulation is referred to incidentally in descriptions of the various motors. There is, also, to be sure, usually one short chapter devoted to this subject, but it is apt to be either very general, merely mentioning a few regulating devices, or else, it is confined to the mathemathical analysis of some one particular method.

Motor regulation is a subject which is somewhat confusing, and requires, therefore, accurate definition of the quantities and conditions which enter into it. One case is, at first sight, very similar to another, and yet more careful consideration will show, very likely, that it is exactly opposite. Take, for example, the two commonest and most important cases, viz: the constant potential and the constant current motors. The conditions of the two cases are precisely opposite and the solution of the problem is, naturally, entirely different. To maintain a motor at a constant speed with a variable load on a constant potential circuit, which is the case of the ordinary shunt motor on the regular 110 volt incandescent circuit, is a very easy problem; in fact, it solves itself. A simple shunt-wound motor will run at a practically constant speed on a constant potential circuit, even if the load varies from zero up to the full capacity of the motor. Now this form of motor is exactly what we would use, even if we did not care to have it regulate.

It is customary, to be sure, where very close regulation is required, to wind constant potential motors differentially, *i. e.*, with series as well as shunt coils, but this is not essential for ordinary work. The case of the constant current motor, on the other hand, is as difficult as the first is easy. It seems as if it had to bear the burden of both. Neither a shunt-wound nor a series-wound nor a compound-wound motor can be used practically on a constant current circuit to maintain a constant speed with variable load. The ordinary series-wound motor, which is the natural and common form for the constant current motor, will race away when the load is taken off and increase in speed until it tears apart or strains the armature and ruins the machine. We require some special and very effective device to accomplish the regulation in this constant current case, and it seems to happen that almost every device that had been thought of has had serious difficulties and complications. The ordinary, and what would seem to be the best solution of this problem is to vary the magnetic effect of the field upon the armature directly in proportion to variations in the load. This is effected by cutting out the field coils, and by other methods which will be described later. But there are serious difficulties with almost all these plans.

These two cases, which, as already stated, are the commonest and most important which occur, and yet which are so very different in almost every respect, give a general idea of what we have to deal with in motor regulation.

In order to fix and hold the rather numerous quantities and conditions that present themselves in connection with motor regulation a tabular statement of them is given below.

TABLE I.

ELECTRIC MOTOR REGULATION.

1. Hand regulation.
    Usually employed for varying the speed.
2. Automatic regulation.
    Usually employed to maintain constant speed, and is effected by
        Centrifugal governors,

Dynamometric governors,
Electro-magnetic devices.

PRINCIPAL METHODS OF MOTOR REGULATION.

1. Shunt winding. (Fig. 1.)
2. Differential winding. (Fig. 2.)
   Varying external resistance in series with
3. Armature.
4. Field. (Fig. 1.)
5. Both armature and field.
   Varying internal resistance of
6. Armature.
7. Field by cutting out coils in series. (Fig. 3.)
8. Field by cutting out coils in multiple arc. (Fig. 4.)
9. Field by grouping coils in series or multiple arc.
10. Both armature and field. (Fig. 5.)
    Varying shunt in multiple arc with
11. Armature.
12. Field.
13. Both armature and field. (Fig. 6.)

14. Varying current in separately excited field.
15. Shunting field magnetism. (Fig. 7.)
16. Short circuiting field magnetism. (Fig. 8.)
    Varying commutation by
17. Shifting brushes. (Fig. 9.)
18. Shifting commutator.
19. Shifting magnetic resultant.

20. Moving pole-pieces away from armature. (Fig. 10.)
21. Moving armature away from pole-pieces. (Fig. 11.)

TABLE II.

LIST OF POSSIBLE CASES WITH DIRECT CURRENTS.

P = Difference of potential in volts at terminals of motor.
C = Current in amperes supplied to motor.
S = Speed of armature in revolutions per minute.
L = Load or torque in lbs. at one foot radius.

Any of these four quantities may be either constant or variable, making the following combinations:—

Case.	Constant.	Variable.	Example.
1.	*P*	*C S L*	Series wound motor with variable load on constant potential circuit.
2.	*P S*	*C L*	Shunt or comp. wound motor with variable load on con. pot. cir.
3.	*P L*	*C S*	Shunt motor with con. load and var. field resist. on con. pot. cir.
4.	*P S L*	*C*	Not practical.
5.	*P C*	*S L*	Not likely to occur.
6.	*P C S*	*L*	Not practical.
7.	*P C L*	*S*	Not practical.
8.	*P C S L*		Shunt motor with constant load on constant potential circuit.
9.	*C*	*P S L*	Shunt motor with variable load on constant current circuit.
10.	*C S*	*P L*	Series motor with speed gov. and var. load on const. current circuit.
11.	*C L*	*P S*	Not likely to occur.
12.	*C S L*	*P*	Not practical.
13.	*S*	*P C L*	Motor with speed governor and var. load on cir. of var. pot. and cur.
14.	*S L*	*P C*	Motor with const. load and speed gov. on cir. of var. pot. and cur.
15.	*L*	*P C S*	Motor with con. load without speed gov. on cir. of var. pot. and cur.
16.		*P C S L*	Motor with var. load without speed gov. on cir. of var. pot. and cur.

In Table i, motor regulation is divided into hand regulation, which it happens, is almost always used for varying the speed of a motor, by a hand switch, Fig. 1, for example; and automatic regulation, which is almost always used to maintain a constant speed, and which is usually accomplished in one of the three following ways: first, by a centrifugal governor similar to that of the steam engine, operating a switch or other device (Fig. 3); secondly, by a dynamometric governor, *i. e.*, a mechanism through which the load or torque of the motor is transmitted, which may also operate a switch and vary the power of the field by the direct effect of changes in the load; thirdly, by an electro-magnetic device, such as a solenoid, through which the current or a portion of it, is passed, which causes the core of the solenoid to move and operate a switch, in accordance with variations in the current. (Fig. 9.)

The table next gives the principal methods used to regulate motors. These consist of different arrangements and combinations of parts, to produce different effects or changes in the working of the motor, and they constitute the means which we have to employ to accomplish a certain object in motor regulation. Method 1, is simple shunt winding, which consists in exciting the field magnet with a circuit of comparatively high resistance in multiple arc with the armature. (Fig. 1.) This maintains a motor at constant speed with varying load on a constant potential circuit, and does this all by itself, as it were, without any trouble, as already pointed out. The reason for this is, that the field, being fed with a constant potential, is, therefore, of constant strength, and a slight variation in speed of the armature, about five per cent., will vary the current in it from zero up to its full capacity, and, therefore, vary its power to an equal extent.

Method 2 is differential winding (Fig. 2), which is a refinement of the first, by which an almost absolutely constant speed, *i. e.*, within one or two per cent., may be practically maintained. This consists in winding a series coil of comparatively few turns upon the field magnets, in addition to the shunt-winding in the previous case, the series coil being wound in the opposite direction and opposed magnetically to the shunt coil. The effect of this is to slightly weaken the field magnetism as the load on the armature,

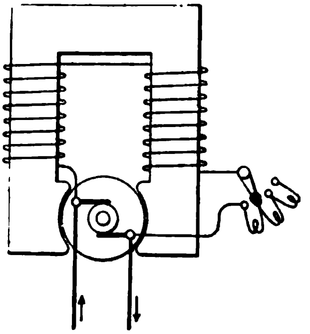


FIG. 1.—Shunt Motor with Variable Resistance in Field Circuit.

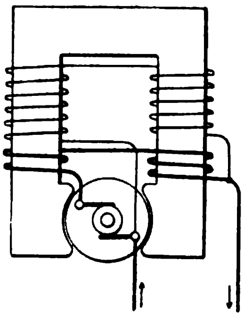


FIG. 2.—Differential Motor. Self-Regulating.

and consequently its current increases. This tends to increase the speed of the armature and counteract its tendency to slow down with increased load, thereby almost entirely eliminating even the small variation of five per cent. in speed which occurs in the ordinary shunt motor. The paradoxical fact that a shunt motor increases its speed with the weakening of the field magnet is well known, being due to the lowering of the counter-eletromotive force, and consequent increase of current in the armature as the field magnetism is reduced in intensity.

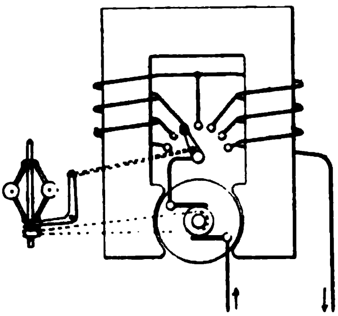


FIG. 3.—Series Motor with Switch for Cutting Out Coils in Series.

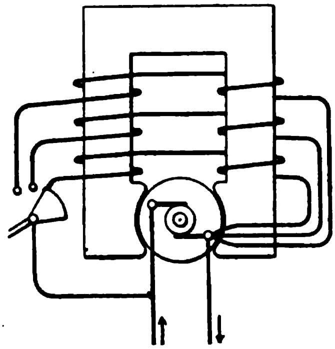


FIG. 4.—Shunt Motor with Switch for Cutting out Field Coils in Multiple Arc.

In the next three methods the external resistance in series with the armature or field, or both, is varied. Method 3 would be very wasteful and is hardly allowable in good practice, because it is simply destroying the main current to put resistance in the armature circuit. Method 4, varying the resistance in the field circuit, is very much less objectionable and is very convenient and common. (Fig. 3.) This mode can be adopted with a very small loss of current, because the field current is only a small part of the total current used in the motor, and the loss of a portion of this small fraction is practically insignificant. Method 5, putting external resistance in series with both the armature and field, is open to same objection as case 3, since it throttles and destroys part of the main current.

The next five modes of regulation, 6, 7, 8, 9 and 10, are theoretically more economical than the preceding, because we vary the internal or useful resistance, instead of adding external or dead resistance. Method 6, in which the internal resistance of the armature is varied, is very difficult to carry out in practice, simply because it is hard to get at and change the connections of the armature while it is revolving. One of the few ways devised to accomplish this, is to wind the armature with two or more circuits connected to a corresponding number of commutators, whereby one or more of these circuits may be used, thus varying the internal resistance and torque. This is clumsy and is practically little better than using two or more separate motors.

Method 7, cutting out the field coils in series, is a very simple and common plan. It is the ordinary way of regulating constant current or other series-wound motors. This method, or some modification of it, is one according to which the constant current or "arc" motors of the Baxter, "C. & C." and Excelsior companies are constructed.

Method 8, of cutting out field coils in multiple arc, is shown in Fig. 4, and is especially applicable to shunt-wound motors. The different layers, for example, may be respectively connected to the contact points of a switch, and thrown into circuit successively, the innermost layer first, etc, thus obtaining the most economical result.

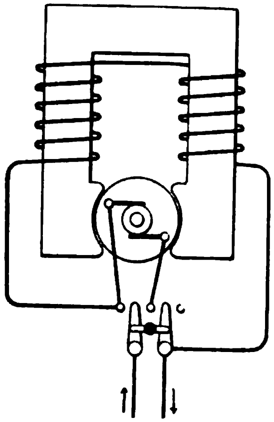


FIG. 5.—Motor with Switch for Throwing Armature and Field in Series or in Multiple Arc.

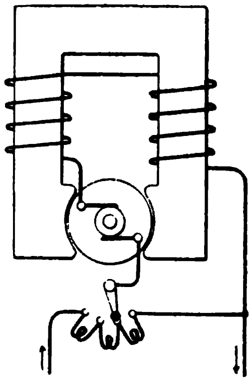


FIG. 6.—Series Motor on Variable Shunt.

Method 9, of grouping the field coils in series or in multiple arc, produces very great variations in field resistance, and is a combination of the two preceding cases. Like them, it consists in winding the field with separate circuits which are connected to a switch. This has been employed in street car motors to obtain great variations in resistance and power.

Method 10 is also merely a combination of two or more of the four preceding cases. A simple way of carrying it out is illustrated in Fig. 5, and consists in connecting the armature and field either in series or in multiple arc, by which a great variation in the total internal resistance of the machine is obtained.

The next method, No. 11, is open to the same objection as No. 6, because when the armature is shunted the main current of the machine is affected. This is, however, less objectionable than dead resistance, because in most cases, it is better to divert the current than to destroy it.

Method 12, like methods 4 and 7, is a useful and common one, because in shunting the field we are merely handling a very small fraction of the total current.

Method 13, varying a shunt in multiple arc with both armature and field, is wasteful, like No. 11. It was, however, the plan by which the first arc motors were regulated, a variable shunt being placed in the main circuit and the motor connected to the terminals of the shunt.

The variations in the shunt, of course, throw more or less current through the motor, thereby varying its power as required.

Method 14, varying the current in a separately excited field, is merely historical, as this class of motors has gone out of use. The plan, however, is a very effective one, because, of course, it is very easy to control the field current when it is produced by a separate source.

Method 15 is shown in Fig. 7, and consists in shunting the field magnetism, *i. e.*, diverting the lines of force from the armature through some other path. In the arrangement shown in Fig. 7, there is a simple series-wound motor with consequent pole field magnet, and if the coils exciting one magnetic circuit are short circuited, it is evident that the magnetism produced by the other magnetic circuit will be short-circuited or shunted by the former. The short-circuiting of the coils is effected by the variable shunt, as indicated.

Method 16 is very similar to the preceding, and consists in short-circuiting the field magnetism and diverting it from the armature by a keeper, which may be moved towards or away from the pole-pieces by means of a screw or other contrivance.

We now come to methods 17, 18 and 19, in which the line of commutation is varied. The first is the well-known plan of shifting the brushes. This is very effective and easily carried out, and has, therefore, been used quite extensively; but it has the very serious and almost prohibitory objection that when the brushes are moved from the neutral point they spark very badly. This may be remedied to a slight extent by some heroic plan like blowing out the spark produced. But, as sparking is one of the worst drawbacks of a motor, it hardly seems as if this method of regulation would be permanent.

Method 18, shifting the commutator, is simply another way of doing the same thing.

Method 19, shifting the magnetic resultant or direction of lines of force in the armature, may be accomplished by two sets of pole-pieces, the relative magnetic strength of which is varied. This is also open to the same objection as shifting the brushes.

Method 20, shown in Fig. 10, consisting in moving or separating the pole-pieces from the armature, has been used in the Diehl and other motors, but it is rather difficult to carry out mechanically, and introduces a hinge or joint in the magnetic circuit, which is, of course, very undesirable.

Method 21, moving the armature away from the pole-pieces, is very much easier to accomplish mechanically, and has a great many advantages as a means of regulation. Dr. S. S. Wheeler and myself have been working for some time on motors in which this method of regulation is employed and the results we have obtained have been very satisfactory, in fact, we found it to be extremely simple, effective and reliable.

The second table gives a list of the possible cases of motor regulation which may occur with direct or continuous currents. There are four supremely important quantities or conditions in connection with electric motor regulation—potential, current, speed and load—and it is with reference to one or more of these that practically all motors are designed, built, sold and used. Motors are always distinguished, both in science and in trade as constant potential or constant current motors; constant or variable speed motors, and constant or variable load motors; hence a classification of motors based upon the constancy or variability of these four factors is very convenient, and at the same time scientifically correct. The second table is made upon this plan, and in it the four quantities are represented by their initial letters, *P*, *C*, *S* and *L*, expressed respectively in volts, amperes, revolutions per minute, and torque in pounds, at one foot radius, these being the units ordinarily used in practical as well as scientific work. We find that there are in all 16 different cases, in the first four of which the potential is constant; in the second four the potential and current are both constant, in the third four the current only is constant, and in the fourth four neither the current nor the potential is constant.

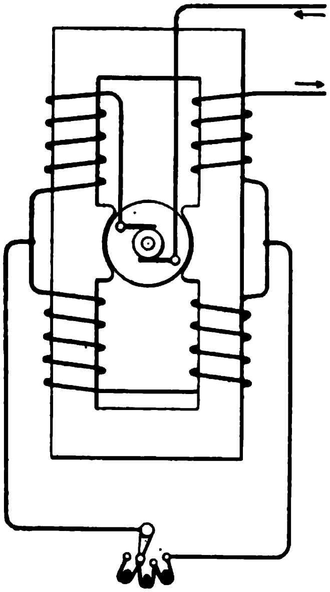


FIG. 7.—Series Motor with Variable Magnetic Shunt.

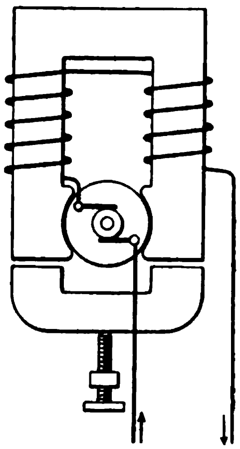


FIG. 8.—Series Motor with Variable Magnetic Short Circuit.

Case 1, in which the potential alone is constant is the case of a series-wound motor, for example, with variable load on a constant potential circuit. Such a motor would rise in speed when the load was reduced and *vice versa*. It would not be at all self-regulating, in fact, it would tend to run at a very high speed with light load, and a very slow speed with a heavy load. It is possible to give various other examples, but one is sufficient in most cases. In this case, however, I may say that the ordinary street car motor, fed from a constant potential circuit, is also an example of case 1, the current being controlled by varying the external or internal resistance, according to one of the methods given in the first table.

Case 2 is that of the ordinary shunt or differential motor, with a variable load on a constant potential circuit. This is self-regulating and maintains a constant speed, as already stated, and is the commonest case of motor regulation met with. The current varies directly with the load.

Case 3 is that of the ordinary shunt motor with constant load, having a variable resistance in the field magnet. This case is shown in Fig. 1. Increasing resistance of field circuit would weaken field magnetism and motor would run faster and take more current, *i. e.*, current would vary directly with speed.

Case 4 is not a practical one, because, if potential, speed and load are constant, the current cannot be varied without wasting it, that is, any excess of current would be simply destroyed, which, of course, is not permissible.

Case 5 is not likely to occur, because potential and current would not be constant with a variable load and speed, but if it did occur, it would require that the speed and load should vary inversely.

Cases 6 and 7 are not practical for the same reason as given in case 4, because if potential. current and speed were constant, the load could not be reduced without wasting electrical energy, and in case 7, the speed could not be varied without wasting energy.

Case 8 is that in which all four factors are constant, and is best illustrated by a shunt motor with constant load on a constant potential circuit, in which case the current and speed would also be constant.

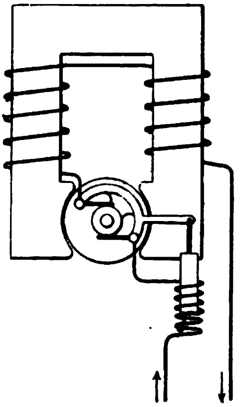


FIG. 9.—Series Motor with Automatic Device for Shifting Brushes.

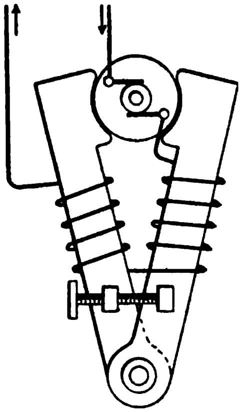


FIG. 10.—Series Motor with Device for Moving Pole Pieces Apart.

Case 9 may be illustrated by a shunt motor with variable load on a constant current circuit The speed would run up as the load reduced. A more common example is a series motor on constant current circuit, with switch for cutting out field coils, or, in short, a constant current motor with variable speed and load.

Case 10 is that of the regular constant current or "arc" motor with governor, such, for example, as the Baxter or "C. & C." motor. The ordinary form of this motor is a series machine with a centrifugal governor which operates a switch to cut in and out the field coils, as shown in Fig. 3. This is, next to the shunt and differentially wound motors, the most common and important case of motor regulation.

Case 11 is that of a constant current motor with constant load, with switch for varying field strength. The potential would rise directly with speed, but this case is not likely to occur.

Case 12 is not practical for the same reason as case 4.

The last four cases are those in which both potential and current vary. They are hardly practical cases, because electrical distribution is almost always either constant current or constant potential. It is, however, of course, possible to have cases where both may vary, and examples of each case are given.

Case 13 is one which does occur to a certain extent in practice, for example, if a so-called constant current circuit varies slightly, as they always do in practice, say from 9 to 10 amperes, then the current as well as the potential are variable, and this must be taken account of in the regulation.

In the experiments which I have already referred to, Dr. Wheeler and I have built a motor of the constant current type which will govern for wide variations in current as well as in load. This motor, for example, will not fall in speed, even though the current be reduced from 10 to 5 amperes, which is a great variation for this type of motor, as the field and armature being in series, the torque varies as the square of the current. This variation in current in the so-called constant current circuits is a much greater percentage than the variation in potential of so-called constant potential circuits, and amounts to from ½ to 1 ampere.

Having generally described the different cases of motor regulation, let us take up the mathematical relation of potential current, speed and load, which may be expressed quite simply.

Using the symbols P, C, S and L, as defined above, we have $P \times C$ as the total electrical energy supplied to motor in watts or $\frac{P\,C}{746}$ in horse power. Calling commercial efficiency a, the actual mechanical horse-power developed by a motor is $\frac{a\,P\,C}{746}$ expressed in electrical terms. The equivalent expression in mechanical terms is $\frac{2\,\pi\,S\,L}{33{,}000}$. Since the point at which load L is measured is one foot from centre of shaft and travels $2\,\pi$ feet per revolution, or $2\,\pi\,S$ feet per minute, this multiplied by the load and divided by 33,000, gives horse-power developed by motor.

Thus we have:—

$$\frac{a\,P\,C}{746} = \frac{2\,\pi\,S\,L}{33{,}000} = \text{horse-power of motor.}$$

or

$$P\,C = \frac{746 \times 2\,\pi\,S\,L}{33{,}000 \times a} = \frac{S\,L}{7.04 \times a}$$

For convenience we may write $P\,C = \frac{S\,L}{7a}$ with an error of about one-half of one per cent.

If the efficiency a, of motor is 85.2 per cent., the denominator $7.04a$ becomes 6 exactly, and we have $P\,C = \frac{S\,L}{6}$, and if the efficiency is 75.8 per cent., we have $P\,C = \frac{S\,L}{5}$. The former is as high and the latter is as low as is ordinarily met with in practical work, therefore these are the limiting values.

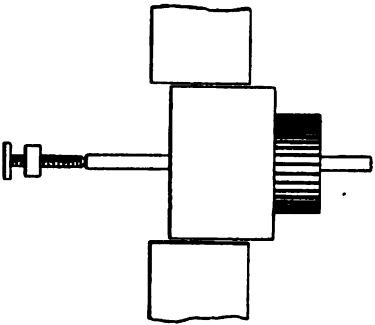


FIG. 11.—Series or Shunt Motor with Regulator for Moving Armature Out of Field.

The equation $P\,C = \frac{S\,L}{7.04a}$ or one of the three simplified forms just given may be advantageously used for solving problems in motor regulation. If we know any four of the quantities we may, of course, find the fifth by simply solving the equation with respect to it. It is also evident that if any three of the quantities are constant then the other two will vary directly or inversely with each other, depending upon their position in the equation. This is the ordinary case we have to deal with in regulation. Usually speed is required to be constant, either potential or current is maintained constant, and efficiency should be practically constant down to very small loads; thus we have either current or potential varying directly with load. The efficiency is rather a confusing element in motor regulation. For this reason it was not introduced as a fifth quantity in the second table, as it would have complicated it and made a great many more possible cases. The efficiency should be high (close to 100 per cent.), and as nearly as possible constant; therefore it would not affect the figures much.

If the load or speed is zero, or very low, the potential or current should be correspondingly reduced and the efficiency would remain constant. This is the condition for perfect regulation. Practically it always takes some current to overcome friction, etc., even when load is zero, in which case efficiency is zero, but this current should be very small.

The equation $P\,C = \frac{S\,L}{7.04a}$ applied to the cases marked "not practical" in second table, shows in case 4 for example, that if $P\,C$ and L are constant, efficiency a decreases directly with reduction in speed. This is not allowable. The same is true of the other cases that it is not proper to vary $P\,C$ or L simply at the expense of efficiency, or in other words waste current to obtain regulation.

Having now given the general points of the subject of motor regulation, we will proceed to consider the forms of motors which are now being used.

First, and most important, we have the regular shunt motor for constant potential circuit, which has already been referred to several times. Thousands of this type of motor are in use and give very satisfactory results. A properly designed motor of this kind will regulate within five per cent., which is near enough for most purposes.

It has no regulating mechanism or complicated circuits to get out of order, and it may be called the ideal self-regulating machine. The next important form is the differentially wound motor, which is even more perfect in closeness of regulation.

Theoretically, it might be made almost absolutely constant in speed, practically the speed varies about two per cent.

These are the figures given me by the company which manufactures these machines in the largest quantity and of the most excellent workmanship. Silvanus Thompson gives an example of one of these machines, which varies only 1½ per cent. in speed, with a variation from 1.1 to 11.14 h. p.

The rule for winding differential motors is that the number of turns in the shunt divided by the number of turns in the series coil, are equal to the resistance of the shunt divided by the sum of the resistance of the armature and the series coil. On this side of the Atlantic this is called "Sprague's law of winding;" on the other side it is called "Ayrton and Perry's rule for winding." The slight variation in speed which does occur is due to the heating of the coils and other slight disturbances. The shunt coil varies slightly in resistance by heat, therefore the series coil should have a few less turns than the rule requires. The heating of the shunt coil weakens the current in it, and consequently the field magnetism, thus increasing the speed of the machine; therefore the motor runs faster after it has been running for some time. The proper way is, of course, to consider the true speed to be that which the motor has in steady running. At first some trouble was experienced in this type of machine, on account of short-circuiting between the series and shunt coils, but this has been overcome by more perfect insulation.

The shunt or differentially-wound machines may be varied in speed by putting resistance in the field circuit, as shown in Fig. 1. This may, of course, be done automatically, or by hand, usually the latter.

The next important type of machine is the constant current motor. As already stated, this is merely a series-wound machine, with switch for cutting in and out the field coils, the latter being operated by an automatic centrifugal governor, as indicated in Fig. 3, or by hand. Various modifications of this type have been made; for example, a reversed coil having a variable resistance in circuit with it, may be used in multiple arc with regular series field winding. If this resistance is very high, the reversed coil has little or no effect; if this resistance is made very low, it will rob the main coil of its current and also neutralize its effect, thereby greatly reducing the strength of the field magnet.

Constant current machines with regulator are now in great demand for arc circuits; in fact, it is not practical or safe to use a motor on an arc circuit without a regulator. The differential winding of constant current motors to maintain constant speed is not practicable. The solution of it requires, either that the efficiency should be very low, or that storage batteries be used in combination with the winding to maintain a constant difference of potential. Any such plan as this is, of course, very objectionable.

The three types just given are really about the only continuous current motors used to any extent in practice, and it seems that there is at present no demand for any other type.

Nothing has been said of the regulation of alternating current motors, because it is a radically different subject, and, moreover, alternating current motors tend to run synchronously, maintaining a constant speed and regulating themselves.

In conclusion, I would say, that I hope to see this subject taken up by leaders in electrical science, and given the attention which I think it deserves but which it has not yet received.

DISCUSSION.

THE CHAIRMAN—(Major O. E. MICHAELIS):—Gentlemen, you have heard Mr. Crocker's paper, which I think I may say, in interest, is in thorough harmonic series with its predecessors. The subject is now open for discussion.

MR. TOWNSEND WOLCOTT:—I would like to ask Mr. Crocker if he has ever observed any difference in sparking between moving one brush and moving both brushes. So far as I have seen, in those machines which use but one brush, the sparking is immaterial. I do not know why it should be so. I never worked out any satisfactory theory.

MR. CROCKER:—It would be half as much, certainly, because there would be only one brush instead of two. But very likely there is some further advantage. I have never observed it and therefore never considered it. It is possible there may be some action, but I do not see what it could be. When one or more brushes are off the neutral point they seem to spark in proportion to the amount they are removed from that point.

MR. WOLCOTT:—I can show you a dynamo with one brush on what is supposed to be the proper contact point, and the other one about 100 degrees from it, sparking very little more than it does when it is set opposite to the first. But this is a dynamo that I am speaking of. I do not know whether the same thing would apply with a motor. This machine is built for 25 lights, and is running two or three lights.

Mr. Edward Weston:—Of course, the sparking between the two sections of the commutator of any machine would be dependent on the difference in potential of the sections in passing out of contact with the brushes. That would depend on the number of sections in the commutator and the total electromotive force. That would still further be complicated by changes in field by moving the brushes. It would be a very complicated question to discuss. I can well understand how you could have a machine in which one brush or both brushes could be moved from the so-called neutral point to the point of no current, and it would practically give no spark, and then again you could have a very heavy spark with some types of machine. With some types of machine, such as the Brush, it could not be done very well.

Mr. Crocker:—I understood Mr. Wolcott to say it was a 25 light dynamo running two or three lights.

Mr. Wolcott:—Yes.

Mr. Crocker:—Of course, in that case the machine was really working with only one-tenth of its normal difference of potential. Therefore the sparking would be reduced just in proportion. It is hardly a fair case if a dynamo is running at only one-tenth of its power. Of course, the sparking and all other effects are changed correspondingly. If the dynamo had been running with its full load of 25 lamps it would probably have sparked as much as any other machine.

Mr. Wolcott:—The current is the same. I understood Mr. Crocker to say that the sparking would be worse as you went away from the correct commutating point; so when you get down to two or three lights it ought to be worse than all.

Mr. Weston:—The sparking is entirely due to the difference of potential between the commutator strips.

Mr. Wolcott:—I should hardly say entirely due to that. When you get a good machine with the brushes on the right point there should be no sparking. Where there is a little it could be accounted for by self-induction of the coils.

Mr. Weston:—If there is any difference of potential between the strips there should certainly be a tendency to spark.

Dr. Schuyler S. Wheeler:—The principal cause of sparking in motors which are regulated, is the fact that the regulator is constantly changing something—either the magnetism of the armature, or of the field, in order to affect the regulation; and, as soon as that is done, what is called the line of resultant magnetism is changed, and while the brushes have not been moved, the non-sparking point has been moved away from the brushes, and we no longer have the brushes at the neutral point. Of course, there are two causes of sparking—one is the breaking of the circuit between the brush tip and the commutator, and the other is the short circuiting between the commutator bars, produced by the brush touching two bars at the same time. To avoid this short circuiting, which is by far the most serious cause of sparking, the brushes are always placed upon the commutator at the point where there is no difference of potential between the bars. These points, known as the points of commutation, mark the position of the magnetic poles induced in the armature by the joint actions of the field magnetism and of the winding upon the armature. Its position indicates the combined effects of these two efforts, at magnetization, and if the strength of either of them is altered, the position of the resultant polarity in the armature changes. Therefore, whenever we change the intensity of the field or of the armature magnetism we shift the line of resultant magnetism and the brushes are no longer on the right spot. For that reason the method of regulation shown by Mr. Crocker in Figure 6 in which the regulation is produced by shunting the current from the whole machine, thereby keeping the relative strength of the armature and the field the same at all times, has the advantage that it will never cause the motor to spark; because, although it alters the power of the machine, it alters the strength of both the armature and the field equally. The method is very objectionable on account of its destroying a large amount of current, namely, all of the current that is taken away from the armature by the shunt, but it has the advantage described, of not causing sparking.

With other forms of regulation, in which the magnetic strength of the field only is varied, it has been necessary to move the brushes, from time to time as the regulator acts, in order to keep them at the proper point; and in some of the forms of motors the brush holders are mechanically connected to the regulator so as to be moved by it automatically when it changes the field strength.

There is another method of regulation which I have tried experimentally that may be interesting, though it is not at all practical. Figure 12 illustrates the device, which consists of a pair of brushes connected to the terminals of the field winding of an ordinary series motor, and bearing upon a cylindrical piece of copper having long tapering teeth at one end. The spaces between the teeth are insulated and the brushes are so placed that when the cylinder is drawn down by an ordinary centrifugal governor the solid copper is in connection with the brushes and the field magnet coil is short circuited. But when the governor moves the copper cylinder upward on its axis at times when the speed is reduced, the toothed portion of the cylinder comes under the brushes, and the tips of the brushes are in contact with the cylinder and thereby in connection with each other only for a part of the time, and are out of connection the rest of the time. The time in contact and the time out of contact depends upon the width of the teeth at the point at which the brushes bear upon them, the position of the cylinder being controlled by the action of the governor. Now, it takes some seconds for the magnetism of the field of an ordinary dynamo or motor to change. Consequently, if we make and break this circuit, which short circuits the field very rapidly, we will not lose our magnetism entirely and get it all again ; we will simply let down the degree of magnetization, but keep it steady at some point, depending on what proportion of the whole time the circuit is closed and what proportion it is open. That is, if we take a field magnet which requires five seconds to saturate and throw the current on it for one, two, three or four seconds, we will attain different degrees of magnetization below the full strength of the machine, and if we arrange the commutator to put the

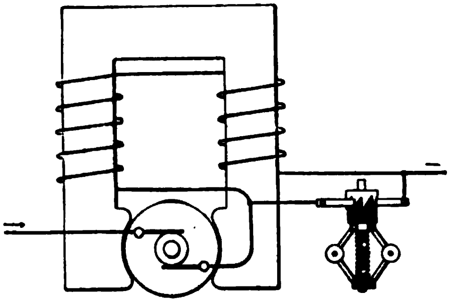


Fig. 12.

current on and off rapidly and make the aggregate time that the current is on one-fifth of the whole time, we will have a uniform magnetization considerably below the maximum, and without the use of any resistance coils whatever. Of course, the device is not at all practicable, the sparking caused by the large field coils being very serious.

Another method (Figure 13) which I think was not touched upon, although it comes under some of those already described as a sub-class, is that employed by the "C. & C." Motor Co. for a hand regulator. It consists of an armature connected in multiple arc with a field which is divided into a large number of coils. The switch is arranged to throw more or less coils out of circuit by simply short circuiting them. This has the double effect of reducing the strength of the field and of shunting the current away from the armature, and thereby reducing the armature strength, so that in this we have a machine in which the armature is regulated when the machine is operating on a constant current circuit, without the use of any special devices on the armature. By making the first coil of the field of heavy wire and each succeeding coil of a size finer, a machine is made in which the armature and field magnetisms are reduced in something like equal proportions by the action of the switch, and this is done without the use of any outside electrical resistance; but this method has the very serious objection which is sometimes fatal, that the minute the armature speeds up, its counter E. M. F. alters the proportion in which the current divides between the armature and the field circuits. Motors on this plan have been made, however, in considerable numbers, and the reason why this disturbance produced by the counter E. M. F. is not more serious is because the resistance of the field coils may be propor-

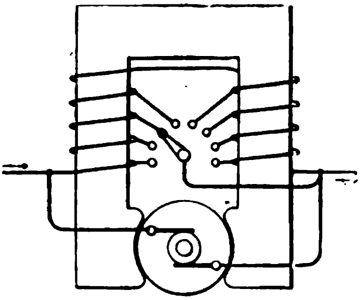


Fig. 13.

tioned to correspond properly with the increases of armature speed, which are apportioned to the several contacts.

The differential winding mentioned in the last part of Mr. Crocker's paper, as one of the methods of regulating constant current motors, consists of two *opposing* field windings with a set of resistances connected in series with one of them for determining which of the opposing windings shall predominate. (See Fig. 14.)

There is one other form of regulator, a method invented by Sprague, in which the field coil is permanently connected with the line and the armature terminals are connected by switch to various points on the field, the speed of the machine being regulated by sliding the armature terminals from the centre contacts towards the extremities of the field, which weakens the field and strengthens the armature. (See Fig. 15.)

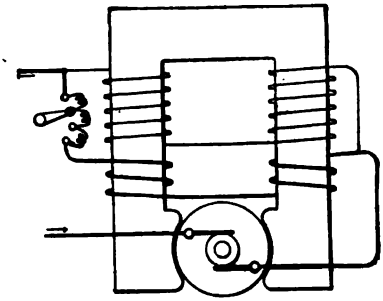


Fig. 14.

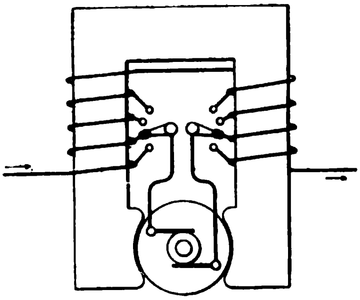


Fig. 15.

Mr. Joseph Wetzler :—I would like to ask Mr. Crocker whether he has ever considered the Waterhouse 3-brush regulation as applied to motors. If so, I would like to hear what his opinion is in reference to it.

Mr. Crocker :—I have considered the matter somewhat, but I never went into it at all deeply. It never was a very attractive method of regulation to me, but I cannot say positively whether it would work or not. I think very likely it would. The condition in a constant current motor is very similar to that in an arc light dynamo. If a certain regulation is good for an arc light dynamo it is probably equally good for a constant current motor. That is quite universally true. But of course this cannot be laid down as an absolute rule. You would have to apply it in each particular case before you are sure. But on general principles it is true.

Mr. C. O. Mailloux:—There is a very peculiar fact with reference to 3-brush regulation, in connection with the sparking, which was brought to my attention a year or two ago. I am sorry that Mr. Hamilton, the former chief electrician of the Western Union, is not here, because he is the man who made the discovery, if it may be called a discovery. (See Fig. 16.) I had occasion to design a machine for telegraphic work where it was expected to use a third brush for the purpose of getting varying electro-motive forces: assuming the total electro-motive force to be say 20 volts, then he proposed to use a third brush between the other two and move that brush so that we could get different currents. He could fix it so that there would be either 10 on each side or else 5 on one side and 15 on the other side. As sometimes one-half would not be working and the other half would be working, it was a question whether there would not be great sparking, but he told me that he proposed to avoid it entirely. I was greatly interested to know how he proposed to do it, and to my great surprise I found that he had done so entirely by this simple expedient of putting another brush on the opposite side and connecting the two together through a simple galvanometer. Now, we found by practice that as long as these two brushes touched correspondingly so that they were of equal potential, with no current passing through that galvanometer, there was absolutely no sparking. If we moved one connection, and if there was no appliance between the two there would be very excessive sparking, but in any case it did not matter where the brushes were located, provided that they were located at corresponding points. For instance, if he wanted to get only a small potential on one side, he might have one brush touch here and the other brush would have to be moved opposite. I cannot account for the phenomenon; perhaps somebody here may. It was interesting as showing that there is yet a great deal of mystery to be elucidated about the sparking of dynamo brushes.

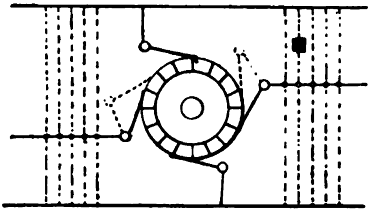


Fig. 16.

PROF. E. P. ROBERTS:—I would suggest that Prof. Nichols be called upon in reference to that mode of regulating.

PROF. E. L. NICHOLS:—I am much obliged for the compliment, but I really have little to add to what has already been said. Mr. Crocker's opinion that any device for the regulation of dynamos may be applied to motors with chances of success at least, I concur in. The behavior of third brush regulator machines is a pretty complex question, and depends upon a great variety of facts which we are scarcely yet masters of. We have been studying that at Cornell to some extent and have gained some light on the subject, but it is hardly in a condition to be presented here.

MR. WESTON:—The last case given by Mr. Mailloux would certainly be very singular. I am entirely at a loss to understand it. Under the conditions named it would indicate that the difference of potential between the strips had nothing to do with the sparking of the commutator. I shall take the first opportunity to try that experiment. It seems to me very remarkable that moving two brushes on opposite sides of the commutator and keeping them at points of equal potential, the two other brushes being in the normal position, could possibly bring about such a result

Mr. MAILLOUX:—I will only say that I myself said the same as Mr. Weston in speaking to Mr. Hamilton about it. He assured me, however, that he had reasoned it out and that it ought to work, and I only state the fact that he found it to work. There were several persons present and the experiments were very extended. As I stated before I regret that Mr. Hamilton is not here himself to give the details and his explanation of the fact. I have given the experiment as nearly as possible as I remember it. It is nearly two years since it occurred. The dynamo was a large machine, probably having a capacity of ten or fifteen horse power, and it was tried under large variations of load. The machine was intended to supply local circuits for telegraph work, and as we know, there might be a large number on or there might be none, and the great aim was to avoid sparking. If I mistake not the machine was put into practical use at Pittsburgh and may be there yet for all I know. I think it is giving satisfaction. Of course (referring to the figure) I should state that the galvanometer is only used for adjustment and that the two brushes are connected by a metallic conductor of low resistance so that the current supplied to the circuit is derived in equal proportions from both of the two additional brushes. The same thing could be accomplished by connecting a portion of the circuit to each brush.

THE PRESIDENT :—(PROF. ELIHU THOMPSON.) It is my experience that in designing and working the dynamo there is no machine so capable of giving unexpected results. If none of the other members have anything further to say I would like to make a few remarks on the general subject.

Mr. Crocker mentioned that in one of his cases the movement of the brushes in the motor would produce violent sparking. Now, I take exception to that. I say that sparking of that kind is within control, as is the case in dynamos which are built for constant current. Take for instance the modified Gramme form known as the American dynamo, in which by a proper adjustment of the over-reach or over-lap of the brushes, you can displace them enormously and still get no sparking. Apply the same process to a motor and move the brushes and you will get the same condition. The sparking at the brushes as I take it depends on conditions of balance, giving the exact time that is needed for current in the coil which is going under the brush to lose the current it has been carrying and take up the opposite current. If you give that time exactly, in any case, you will get no sparking, as in this case your field is constant and your armature current is also constant there would be nothing to prevent such a balance being preserved at all times. In other words, if we commute our currents in the armature bobbins in a certain density of field and in shifting the brushes do not disturb the conditions, or if we do disturb those conditions, but bring into action other conditions which will adapt the machine to the state of field or the state of commutation, then you can control the sparking. In other words, the self-induction is the thing you have to take into account. It takes time in any armature section to reverse the current, and we know that on one side of the brush the current is in one direction and on the other side in another direction. Keep then the coil under the brush for just that time which allows it to pass from 5 amperes which it may be carrying on one side to 5 amperes which it may be carrying on the other side, and you have no sparking in any case. That is one reason why flexibility exists in many forms of dynamo, especially where the armature has some influence on the disposition of the magnetic forces. In cases in which the field is abundantly strong and the armature is a weak magnet, then of course, the commutation is made in a rather weak field just before the bobbin reaches the strong field. But if the armature bobbin has passed forward under the field, then the conditions are all changed.

I would like to add also another case to Mr. Crocker's cases—one which I worked at for some time and which I found was a fairly satisfactory solution of the problem of constant current motors. I have not very much faith in constant current motors anyhow. They always require some extraneous mechanism like a governor or something of the kind which varies the conditions and that governor is apt to be behind time, especially on light loads. It may set the motor oscillating. The case is very similar in constant current motors to what we find with arc lamps on constant potential. We have instability with arc lamps on constant potential, and we have instability with motors on constant current, for the simple reason that the lowering of speed in the motor drops the counter-electro-motive force and we only have a definite certain current to work on; that means a drop in the actual amount of energy available. The time the motor needs more energy to bring up its speed is the time it loses the energy, and that is analogous to the rush of current that will go through an arc lamp put in multiple on a constant potential circuit, because the resistance of the arc falls with the increase of the current. Consequently the more current passes the more current will pass as hot vapor is produced. The case which I referred to as an additional case is one for constant current with the field wound in shunt with the armature. It is shown in Figure 17. We have the field in shunt with the armature but we vary the field coils in the size of wire. We start with thick wire, then thinner until we end with a rather thin wire. We now make our connections from this at various points and put a governor on the motor shaft which will give us a contact to traverse these

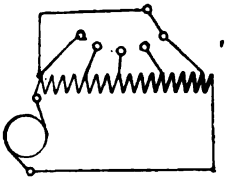


Fig. 17.

connections. The armature and field being in shunt the current will always divide between them, and it will be seen that when we have this contact over one side we have a comparatively high resistance field, or in other words, we have something like a shunt machine; that will be the condition under heavy load. As the load comes off and the speed rises the connection passes to the other end. We get then a low resistance field. The current passes through the field branch in greater proportion than through the armature branch. The field being low resistance is economical, and we get in that way a very fair and economical regulation for series motors. I have built motors on that plan and found they worked very satisfactorily indeed, with a proper arrangement for moving the contacts. There is another advantage that I find in such a motor, that the brushes are practically sparkless. That is, they may be set in one position, require no movement at all, even on so few segments as a three-coil armature, and of course on a Gramme armature and a multiple segment machine the constancy is even greater, but the brushes are simply set at a certain angle with the line joining the fields, and the governor takes care of this process. The objection to it is that it requires a rather nice construction of the machine. You begin with a coarse wire and go down finer and finer, and to get the grading properly is a matter of calculation, of course. (This is the same method of regulation as Figure 14, described by Dr. Wheeler).

Mr. Crocker:—I would like to add one word. I think there is a great difference between motors and dynamos in one respect, and it is a purely practical one; the dynamo is handled by men who know how to handle machines; you can afford to have dynamos in the hands of skilled workmen. Whereas motors are exactly the opposite; they are likely to be in the hands of the most ignorant people. Therefore systems of regulation which are perfectly allowable or practical in the case of a dynamo are not so allowable or practical in the case of a motor. In fact, the case spoken of with a nice balance in the position of the brushes is all right in the case of a dynamo where it can be maintained but it probably could not be maintained in the case of a motor.

In speaking of shifting the brushes I referred more to motors than to dynamos. I was not talking of dynamos or criticising dynamo regulation, I think that motor regulation is very different in the respect I have stated, and that is about the only radical difference that there is between motor and dynamo regulation.

THE PRESIDENT:—I would say that in working the carbon brushes we find we can have quite large variations on account of the resistance of the brush. The spark will be almost insignificant, because the resistance of the brush is opposed to any sparking or short-circuiting.

NOTE.—The proceedings of the General Meeting will be continued in the double number for August and September.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

MAGNETISM IN ITS RELATION TO INDUCED ELECTROMOTIVE FORCE AND CURRENT.

BY ELIHU THOMSON.

There is, perhaps, no subject which at the present time can have a greater interest to the physicist, the electrician, and the electrical engineer than the one which heads this paper. The advances which have been made in the study from its purely theoretical or scientific side, and the great technical progress in the utilization of the known facts and principles concerning magnetic inductions, can but deepen and strengthen that interest.

On the side of pure theory we find the eager collection of experimental data, to be submitted to the scrutiny of the ablest and brightest minds, to be examined and reasoned upon with the hope of finding some clue to satisfying explanations; and, on the side of practice, we find the search for new facts and relations no less diligent, though often stimulated by practical problems presented for solution. Indeed, the urgency for results is often the greater on the practical side, for theory can wait, practice cannot, at least in the United States.

We must look for continued triumphs in both directions, and the most welcome of all will be the framing of a theory or explanation which will enable us to interpret magnetic and electric phenomena. The recent beautiful experiments of Hertz on magnetic waves have opened a fertile region for investigation.

It would seem that the study of magnetism and electricity will give us the ability to investigate the ether of space, which medium has been theorized upon at great length, with the result of leaving it very much where it was before, a mysterious necessity.

Faraday says, speaking of magnetism :

"Such an action may be a function of the ether, for it is not at all unlikely that if there be an ether it should have other uses than simply the conveyance of radiations." 3,075 Vol. iii., Exp. Res.

"It may be a vibration of the hypothetical ether, or a state of tension of that ether equivalent to either a dynamic or a static condition, etc." 3,263. Vol. iii., Exp. Res.

Faraday again says, speaking of the magnetic power of a vacuum:—

"What that surrounding magnetic medium deprived of all material substance may be I cannot tell, perhaps the ether." 3,277. Vol. iii., Exp. Res.

Modern views would seem to point that through a study of magnetic phenomena we take a feeble hold upon the universal ether. Magnetism is an action or condition of that medium, and it may be that electrical actions are the expression of molecular disturbances brought about by ether strains or interferences. The close relations which are shown to exist between magnetism and light tend to strengthen such views. Indeed it would not be too much to expect that if the mechanics of the ether are ever worked out we should find the relation between sensible heat and electric currents to be as close as that of light to magnetism; perhaps find ultimately that the forms of matter, the elements and compounds are the more complex manifestations of the universal medium; aggregations in stable equilibrium. It is a difficult conception, I confess, and a most shadowy and imperfect one, yet facts and inferences which favor such views are not wanting.

Our science of electricity seems almost to be in the same condition that chemistry was in before the work of Lavoisier had shed its light on chemical theory. Our store of facts is daily increasing, and apparently disconnected .phenomena are being brought into harmonious relation. Perhaps the edifice of complete theory will not be more than begun in our time, perhaps the building process will be a very gradual one, but I cannot refrain from the conviction that the intelligence of man will, if it has time, continue its advance until such a structure exists.

I have been led to make these general allusions to electrical theory in order to emphasize the fact that in the present paper no unraveling of the mystery is to be attempted, but rather the presentation of some few considerations upon a subject of absorbing interest.

The conception of Faraday in regard to the existence of lines of magnetic force representing directions of magnetic strain or tension in a medium, has not only lost nothing of its usefulness up to the present time, but has continually been of great service in the understanding of magnetic phenomena. We need spend no time in showing, as Faraday and others have done, that these lines are always closed circuits, polarized so that the direction of the lines cannot be reversed without reversal of the actions. Nor need we take time to show that in any medium the lines are mutually repellant laterally if of the same direction of polarization. Opposing this tendency to separation or lateral diffusion of magnetic force, is the strong apparent tendency of the lines to shorten themselves in any medium. These actions are disturbed by the presentation of a better medium, as iron, instead of space or air. Lines of force will move into the better medium, having apparently the constant tendency to diminish the resistance in their paths.

The peculiar and mysterious nature of media such as iron, is to permit an extraordinary crowding of lines on account of slight resistance to their passage through them. We need not, in addition, do more than refer to the other well-known facts of an electric current developing magnetic lines encircling the conductor, as being the general type, which includes all forms of magnetic field, or electro-magnets sustained by currents, and the fact of a development when magnetic lines or circuits and material masses are in relative movement, of electromotive forces transversely to the direction of the lines of magnetism and also transversely to the direction of relative movement, as in the case of electric conductors traversing or cutting through a field, or of a field traversing or being moved across a conductor. We must not forget that even insulators, as well as conductors, cutting lines of force, have the electromotive force developed in them. The action simply develops potential difference, and this generates the current where a circuit exists. While we are in the habit of saying that a conductor moved across a field of lines, or *vice versa*, generates electric current, I think the statement incomplete. The movement only sets up a potential difference and the power expended in effecting the movement generates $C \times E$. The current is energy less the potential, or the energy expended gives the two effects of potential or pressure, and current or rate of movement. Consequently, an insulator or an open-circuited conductor, traversing a field, consumes no energy, potential difference only being produced. Nevertheless, as will be shown, the magnetic circuits or lines themselves may furnish the energy for their own movement across a conductor, and so develop current as well as potential This occurs in the efforts of lines to shorten their paths, to lessen their density, to pass to better media. Indeed, a close examination will show that wherever power is expended in developing current in a circuit, cutting lines of force, the energy expended is first employed in stretching the lines, which thus receive the energy required to permit them, in shortening, to cut the conductor and set up currents in the electric circuit in accordance with the potential difference developed in that circuit and its resistance.

I think we may also say, though I do not remember to have seen the statement so put, that whenever electric potential is set up inductively, as in self-induction, mutual induction, induction from one circuit to another and induction from magnets or magnetic field, it is set up by the movement of lines of force laterally across the body, mass or conductor in which the potential is developed, and that whenever current is set up in a wire or an existing current prolonged, or an existing current checked by induction, self-induction or induction from magnets, the action is a transfer of energy, represented by strained lines of force shortening or lessening their resistance, or lengthening and increasing the resistance in their paths. The magnetic field is like an elastic spring, it can in one condition represent stored energy--can be strained and will store energy—it can be made to relieve its strain and impart energy.

Let us examine some known phenomena in this light. Take the case of a simple wire, conveying currents say in a line away from observer, Fig. 1. There exists a free field of circular magnetism (so-called), shading off away from the wire and which is represented by concentric circles of increased diameter. The superior intensity or strength of the lines near the wire may also be represented by their thickness. This is often shown also by crowding the lines near the wire, though I am disposed to regard Fig. 1 as more nearly expressing the condition, unless we are to regard the lines as simply indicating a sort of atmosphere of magnetic effect whose density becomes less as we proceed outward from the wire, in which case either form of symbol suffices. The direction of polarization of the lines may be indicated by an arrow head pointing in a direction of right-handed rotation in the path of the lines. This is the typical figure or expression for all forms of simple magnetic circuit—the form of the lines, their length, position, density, will depend on the shape of the conductor or conductors (when more than one), and the materials surrounding or in proximity to the wire or wires.

If the current traversing the conductor is constant, the magnetic field around it is stable and static, unless other influences come in to modify it. The cutting off of the current is followed by instability of the field whereby it can and must produce dynamic effects. I say *must* because the field represents stored energy and in disappearing *must* give out that energy. To throw light on this part of the subject is one of the objects of the present paper. Cutting off the current supply in the case assumed leaves the developed magnetic lines or strains unsupported, they at once shorten their paths or circuits, collapsing upon the conductor as it were, and continuing this action, cut the section of the conductor, and apparently disappear in magnetic closed circuits of infinitesimal diameter but of great strength of polarization. It appears to me that we must either be prepared to give up the idea of lines of force or take the position that the magnetic circuits precipitate themselves in shortening their circuits, disappear upon and cut the conductor. It was Hughes who put forward the idea that an iron bar in losing its apparent magnetism really short circuits the lines in itself as innumerable strongly magnetized closed circuits among the molecules. In becoming magnetic once more these short circuits are opened or extended into the air by some source of energy applied to strain the lines, such as a current in a conductor around the bar.

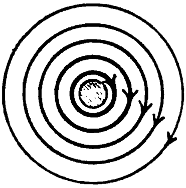


Fig. 1.

May not this idea be extended then, to include the magnetic medium, the ether itself? Does it contain intensely polarized closed circuits of magnetism which are ready to be stretched or extended under certain conditions by the application of energy, which energy is returned by the collapse of the extended circuits?

This is doubtless but a crude expression of the real condition of things, for the lines are only symbols for a condition of strain in a medium which cannot be represented in thought, as we know nothing of its real nature. There is one point in this connection which I must emphasize. The strained lines, Fig. 1, are indications of stored energy in the ether, and the lines *cannot* disappear without giving out that energy. Ordinarily it makes its appearance as the extra-current, and adds itself so as to prolong the current which extended the lines when an attempt is made to cut off such current. Were it conceivable that the current could be cut off and the wire put on open circuit while the lines still remained open or strained, the energy must still escape when the field disappears. It would then produce such a high potential as to be able to discharge from the ends of the conductor, and if the conductor were of some section, part of the energy would be expended in setting up local currents in it. The field could not disappear without an outlet for the energy it represents. But we cannot cut off a current in a wire so as to leave the wire on open circuit while the lines of the magnetic circuit remain around it without having iron, steel or the like in the magnetic circuit. We can approach that condition, however, by breaking the circuit very quickly with a condenser of limited capacity around the break. This is done in the Ruhmkorff coil primary; the condenser forms a sort of blind alley for the extra current on its beginning to flow out of the primary coil. But the condenser charges and so backs up and stops the discharge from the primary, even giving a reverse current. The lines of magnetic force collapse, however, and have their effect in the enormous potential set up in the secondary coil.

Take away the secondary coil so as to stop that outlet, the energy expends itself on the iron core and the primary coil. Take away the iron core and the energy of magnetization of the air or ether core expends itself on the wire of the primary and, possibly, also on the dielectric of the condenser to some extent. The extra current becomes in this instance an oscillatory discharge of very high period back and forth through the primary coil from the condenser, until the energy is lost in the heat of $C^2 \times R$. This conversion is doubtless rendered all the more rapid by uneven distribution of current and eddy currents set up in the wire of the coil.

The considerations just given concern the loss of field or the shortening and apparent disappearance of the magnetic lines or circuits, as giving rise to the self induction or increased potential on breaking. Where the energizing current is slowly cut off or diminished, the energy is gradually transferred to the wire in producing elevation of potential during the decrease; and the collapse and cutting of the wire by the collapsing circuits or lines is then only more gradual.

Let the current be returned to the wire after disappearance of magnetism and the lines again seem to emanate from the wire and at the same time cut it and produce a counter potential in it, which is the index of the abstraction of energy from the circuit, and of its storing up in the form of elastically strained lines of magnetism around the conductor. The effect is that of self-induction on making or upon increase of current, the measure of the amount being the energy stored in the magnetic circuits which have been extended or opened up by the current. The greater the current and the shorter the path for the lines developed around the axis of the conductor the greater the energy stored up. Hence, a circular section conductor has the highest self-induction, a tube of the same section less as its diameter increases, a flat strip has less as its width increases and thickness diminishes, a divided conductor much less than a single conductor of same shape and section. Separating the strands of a divided conductor increases the length of magnetic paths around it and so diminishes the self-induction. A striking instance of this latter fact was developed in conveying very heavy alternate currents of very low potential a distance of about three feet by copper conductors, the current being used in electric welding operations.

The conductors were built up of flat thin strips of copper for flexibility. When the strips were allowed to lie closely together the short conductor showed an enormous self-induction which cut down the effective potential at its ends near the work. By spreading apart the strips so as to lengthen a line around the conductor, the self-induction could be easily made less than 35 per cent. of what it had been before. The interweaving of the outgoing and return conductor strands as one compound conductor, gets rid almost entirely of the self-inductive effects, because neither conductor has any free space in which to develop strong magnetic forces, but is opposed in effect everywhere by the opposite current in its neighbor.

Where a number of conductors are parallel and have the same direction of current, as in a coil or in a strand, it is evident that, statically, the conductor may be considered as replaceable by a single conductor with the same external dimensions and same total current in the area occupied, the magnetic forces or lines surrounding them being of the same intensity. But with changing current strength the distribution of current in the conductor has also a powerful effect on the energy absorbed or given out in accordance with the magnetism produced. Hence the self-induction of a strand, coil or conductor of the same section varies with the rapidity of current changes, owing to the conduction being uneven.

The uneven distribution of current, or its tendency to flow on the outer parts of a conductor when the rate of variation or alternation is made great, is in itself a consequence of the fact that less energy is transformed into magnetism in this case, than when the current flows uniformly over the section or is concentrated at the centre. In other words, when a uniform current traverses a conductor of some section the circular magnetism or surrounding magnetic lines are to be found not only outside the conductor, but also beneath its exterior. Since in forming these lines on passage of current the middle of the section would be surrounded by more lines than any other part of the conductor, the current tends to keep out of that part and move near to the exterior in greater amount. Hence in rapidly alternating currents the conductor section is practically lessened, being restricted largely to the outer metal of the conductor. If the round conductor, Fig. 2, were made of iron, the magnetism interior to it and set up by a current in it would be very much greater, the section of the conductor being filled with magnetic circuits or lines around the centre. The total magnetism, external and internal, would be much greater in this case for a given current flow, and

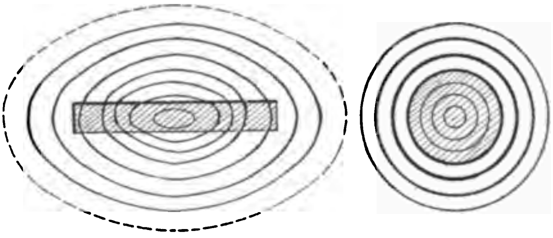


Fig. 2.

the energy absorbed and given out in formation and loss of field, or the self-induction would be much increased. This could, however, be greatly diminished by slitting the conductor radially or making it of a number of separate wires out of lateral magnetic contact, one with the other, Fig. 3. In these cases the resistance of the interior magnetic circuits would be increased, as there would be several breaks in the continuity around the centre

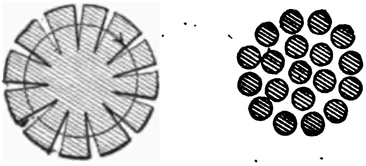


Fig. 3.

of the conductor. The total magnetism which could be set up by a current would be lessened, and the self-induction, therefore, lessened.

The moment we begin the bringing of iron into proximity with an electric conductor conveying current, we provide a better medium for the flow or development of magnetic lines or circuits. In other words, the lines may then be longer, yet equally intense, or more lines may be crowded into a given section of this metal than in space or air. Figs. 4*a*, 4*b*, 4*c*, show the effect brought about by bringing iron of different forms near to the conductor.

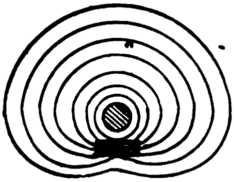


Fig. 4. *a*

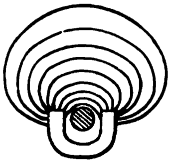


Fig. 4. *b*

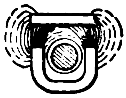


Fig. 4. *c*

It shows, in other words, the development of the ordinary electro-magnet of the horseshoe form, and the concentration of the lines in the better medium. The lines also tend to shorten and diminish the resistance to their passage, so that attraction of the iron to the conductor takes place, and if there is more than one piece of iron they tend to string themselves around the conductor in magnetic contact with one another.

When copper bars of one inch diameter are traversed by currents of 40,000 to 60,000 amperes, as in welding them, the magnetic forces just referred to become so enormous that very heavy masses of iron brought up to the bar are firmly held, even though the current be of an alternating character, changing direction many times a second.

When a conductor is surrounded by an iron ring, as in Fig. 5, the current in such conductor has an excellent magnetic medium surrounding it. A large amount of energy is then abstracted on the first impulse of the current, which goes to develop strong and dense magnetic lines through the iron ring and across the gap in it. On taking off the current the energy is returned as an extra current, and its force is many times what it would be with air alone surrounding the conductor. We have then greatly increased the self-induction, the storing of energy and opposition to flow at the beginning, the giving back of energy and assistance to the current flow on attempting to remove or stop the current. Let us now complete the ring by making it of iron, endless, Fig. 6, with the conductor in the middle.

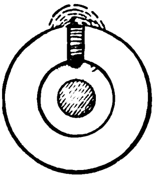


Fig. 5.

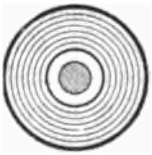


Fig. 6.

We now find that on passing current through the conductor it meets with a very strong opposing effect or counter potential. The evolution of magnetic lines or the opening out of magnetic circuits goes on at very rapid rate. Each line or magnetic circuit evolved and cutting the conductor, flies at once outward and locates itself in the iron ring. This ring can carry innumerable lines, and they do not crowd one another. It permits the lines even to lengthen in reaching it, and yet, on account of its low resistance to their passage, the lengthening is equivalent to their having shortened in other media. We will suppose the current not sufficient to exhaust this peculiar capacity for lines which the iron has. Equilibrium is reached, the conductor has widened out innumerable closed circuits and caused them to exist in the ring still closed, but in iron, not space or ether merely. The current passing has continued its action and storage of energy until, to emit another line in view of the resistance now found in the crowded iron ring, is impossible.

Now, let us cut off the current. We are surprised to find a very weak extra current, a practical absence of self-induction on breaking, or at least a giving out of energy in no wise comparable to the absorption on making. Let us put on the current as it was before. Another curious result. But little self-induction now on making, energy not absorbed.

Now cut off the current again. Same effect as before. Now let us put on the current reversed in direction. At once we find a very strong counter potential or opposing self-induction developed.

The ring has been polarized, or retained its magnetic energy, and we are now taking out one set of lines and putting in reversely polarized lines of force. This done, we break the reversed current without much effect of self-induction. The ring remains polarized and inert until an opposite flow of current be sent through. Iron is then a different medium from the ether.

The ring, once magnetized, must, in losing its magnetism, permit a closure of the lines by shortening. This involves their passage from the iron across the space in the centre of the ring notwithstanding its great resistance to the lines of force. As passage from iron to air is equivalent to lengthening of the lines it is readily seen that such lengthening may oppose more effect than a slight shortening, due to leaving iron for air or space, may give in provoking a closure and disappearance of the lines. Looked at from another standpoint, the lines in the iron may actually require a small amount of initial energy to dislodge them therefrom, so that after being dislodged they may collapse and yield whatever energy they represent.

I must reserve for the future, further consideration of the iron ring, but in thinking upon this matter I am led to imagine that the production of a magnetic line in an iron ring around a conductor may represent a sort of wave of energy, an absorption of energy on the evolution of the line from the conductor and a slight giving out of energy on the line reaching that position of proximity to the iron ring that its passage thereto may be said to be a shortening process or a lessening of its resistance.

The magnetism in air, gases and non-magnetic bodies, being assumed to be that of the ether, this medium shows no such effects as those we get with the ring. It does not become permanently polarized, as does even soft iron under the condition of a closed ring. The iron possesses coercive force, or magnetic rigidity, and a steel ring would show more of it. The molecules of the iron or steel take a set. If we were to cut the soft iron ring or separate it in any way, this introduction of resistance of air for ether in the magnetic circuit would cause the lines to collapse and set up a current in the conductor. The energy of the ring would have been restored to the latter. The curious thing is that physically the polarized ring does not present any different appearance or ordinary properties different from those of a plain ring, and will not deflect a compass needle. Its condition is discoverable, however, by the test of self-induction to currents of different direction. As a practical consideration, we may mention in this connection that a self-inductive coil for currents of one direction must be constructed differently from one to be used with alternating currents. The former must have in its magnetic circuit a section of air or the like, or be an imperfectly closed circuit, as it were. The latter should have as perfectly closed a magnetic circuit as can be made. We see here also the futility of constructing a Ruhmkorff coil core on the closed iron magnetic circuit plan, because the currents in the primary are interrupted, not reversed.

The considerations just put forward in relation to the closed iron ring, and its passive character under the condition of becoming polarized, are more important than at first appears. It has been found that the secondary current wave of a closed iron circuit induction coil or transformer, whose primary circuit receives alternating current, is lagged from its theoretical position of 90 degrees behind the primary wave, an additional 90 degrees, so that the phases of the two currents are directly opposed; or the secondary current, working lamps only in its circuit, is one-half a wave length behind the primary, instead of only a quarter wave length, as might have been expected.

But when it is understood that the iron core polarized in one direction by the primary impulse does not begin to lose its magnetism when that impulse simply weakens, but waits until an actual reversal of current has taken place, it will be seen that the secondary current, which can only be produced when magnetic lines are leaving the core and cutting the secondary coil, or when the lines are being evolved and passing into the core from the primary coil, will have a beginning at the moment the primary reverses, will continue during the flow of that impulse, and will end at substantially the same time with the primary impulse, provided the work of the secondary current is not expended in overcoming self-induction, which would introduce a further lag. Moreover, the direction of the secondary current will be opposite to that of the primary, because the magnetic circuits which are opened up by the primary current in magnetizing the core, or which are closed or collapsed by it in demagnetizing the core, will always cut the secondary coil in the direction proper for this result. Transformers of the straight core type with very soft iron in the cores and with not too high rates of alternation should approximate more nearly the theoretical relation of primary and secondary waves, because the magnetic changes in the core are capable of taking place almost simultaneously with the changes of strength of the primary current. This fact also has other important practical and theoretical bearings.

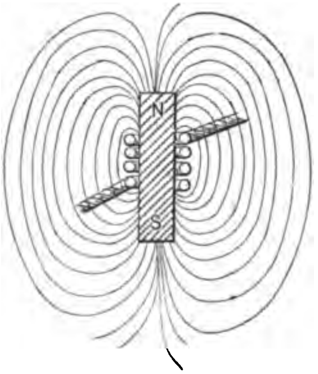


Fig. 7.

Let us assume a plain iron core, Fig. 7, magnetized as indicated, so that its poles N S complete their magnetic circuits by what is called free field or lines in space around it. Let a coil of wire be wound thereon as indicated. Now assume that the magnetism is to be lost or cease, either suddenly or slowly. An electric potential will be set up in the coil, and if it has a circuit, work or energy will be produced or given out in that circuit, and in any other inductively related to it. Hence the magnetic field represents work or potential energy. But to develop potential in the wire the lines must cut the wire. This they can do by collapsing or closing on themselves. The bar seems, therefore, to lose its magnetism by gaining it all, and in doing so all the external lines of force moving inward cut the wire. The magnetic circuits shorten and short circuit themselves in the bar, perhaps as innumerable molecular magnetic circuits interior to the iron medium. To remagnetize the bar, we may pass an electric current through the coil. The small closed circuits are again distended, the free field appears, and the lines moving outward cut across the wire coil opposite to the former direction, and produce a counter potential in the wire, and consequent absorption of the energy represented in the free field produced. As before studied, the magnetism cannot disappear without giving out the energy it represents, even though the wire coil be on open circuit, and be therefore unable to discharge that energy. The coil open circuited is static, not dynamic. In such assumed case the lines in closing cut the core and heat it. Let us, however, laminate the core or subdivide it as far as possible, and we appear to have cut off this escape for the energy. This is not really so, however. We have simply increased the possible rate of speed of closure, or movement of the lines, and so have increased for the divided core the intensity of the actions of magnetic friction and local currents in the core, the latter still receiving the energy of the magnetic circuit. This reasoning is based on the possibility in this case of cutting off the current in the magnetizing coil and retaining the magnetic field. This is of itself probably impossible with soft iron. That the core receives the energy when the coil cannot, is shown in the well-known fact that in some dynamos with armatures of bobbins on iron cores, the running of the armature coils on open circuit gives rise to dangerous heating of the cores, and that under normal work the heating is less. In the former case the core accumulates the energy represented in the magnetic changes. In the latter the external circuit of the machine and its wire coils take the larger part of the energy which is expended in doing the work in the circuit. In this case, also, the current in the coils causes a retardation of the speed of change and extent of change of magnetism in the iron cores, which keeps down the intensity of the magnetic reaction. In fact, this retardation or lag and reduction of range of magnetic change may in some machines be made so great by closing the circuit of the armature coils themselves or short circuiting them, that the total heat developed in the cores is much less than under normal load.

I wish now, in closing, to refer briefly to phenomena of moving lines of force, and to the effects of speed of movement. In order to generate a given potential in a length of conductor we have choice of certain conditions. We can vary the strength of field and we can vary the velocity. We can use a strong field and slow movement of conductor, or we can use a weak field and rapid movement of the conductor. But we find also that where the conductor has large section it is liable to heat from eddy currents, caused by one part of its section being in a stronger field than another at the same time. One part cuts the lines where they are dense and the other where they are not dense, with the result of difference of potential and local currents, which waste energy in heat. We cannot make the conductor move in a field of uniform density, because it must pass into and out of the field. The conditions just stated are present in dynamos for heavy current work, where the speed of cutting of lines is low and the armature conductor large in section.

But we find that in a transformer secondary we can use very large section of conductor, even (as in welding machines) 12 to 15 square inches solid copper, without meeting appreciable difficulty from eddy currents in it. The magnetic lines certainly cut the heavy conductor and generate the heavy current and potential needed. What difference, if any, exists? In the transformer the currents are generated by magnetic field of very low density, in which the lines are moving across the conductor with extreme rapidity. The velocity of emanation of lines around the primary coil is probably near the velocity of light, and each line passes across the section of the secondary conductor in a practically inappreciable time. There is no cause then for differences of potential at different parts of the section of the heavy secondary. To avoid eddy currents in large conductors and generate useful currents in them, we may cause the conductor to be either moved into and out of a low density field with very great speed, or better, we may cause the lines of a very low or diffused field to traverse or cut across the conductor with very high velocity.

It is a known fact that in dynamos with large section armature conductors there are less eddy currents produced in the conductors when they are provided with iron cores or wound upon iron cores, than when the conductors are made into flat bobbins moved in front of field poles. Projections existing on the armature between which the conductors are placed have a like effect, and enable us to employ heavy bars or bundles of wire without much difficulty from local currents. The reason is simple. In the armatures with coils without iron in them or without projections extending between the turns, the conductor moves into and out of a very dense field at comparatively low velocity, so that any differences of potential developed in the parts of the section of conductor have full effect and abundant time to act in setting up harmful local currents. In the cases in which iron projects through the coil or conductor the real action is that the lines of the magnetic circuits move at high speeds across the conductor, and the conductor is at all times in a field of very low density. Figs. 8 and 9 will make this plain. In Fig. 8 we have shown a smooth armature surface having a heavy conductor laid thereon, and which is at *a* just entering a dense field at the edge of the pole N and at *b* leaving the field. It will be seen that when in such a position the conductor, if wide, is subjected to varying field strength and moves at a low speed for the generation of the working potential as it passes through the field, thus giving rise to eddy currents in the conductor.

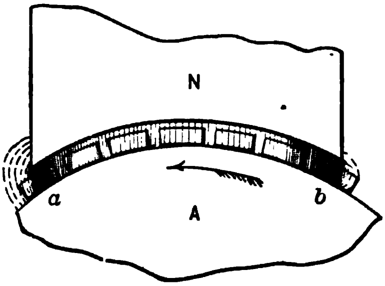


Fig. 8.

In Fig. 9 the conductors are set down between projections, in which case both armature and field poles are laminated or subdivided. As each projection leaves the edge of field pole N the lines which it had concentrated on and through it snap backward at an enormous speed and cross the gap to the next succeeding projection on the armature, cutting the whole section of the heavy armature conductor at practically the same instant. This brisk transfer of lines goes on from each projection to the succeeding one in front of the field pole, leaving a very low density of field at any time between the projections. The best results would be obtained when the armature conductor does not project beyond or quite fill the depth of groove between the projections. Of course there are other remedies for the eddy current difficulty, notably the stranding and twisting of the conductor on the armature so as to average the position of the parts of the compound conductor.

Perhaps the most extreme case of what may be called dilution of field by projections and by closed magnetic circuits in transformers would be that of a block of iron, B, Fig. 10, moved between poles N and S, and having a hole through it, into and through which a conductor is carried. The path through the iron is so good that we can scarcely consider that any lines cross the hole from N to S; yet as B moves forward there is a continual snapping transfer of lines from the right forward side of the hole to the left or backward side, cutting the conductor as they fly across, and developing an electromotive force in it. I have described this action more in detail because we have in it whatever distinction in the manner of cutting the lines of the field is to be found between wire on smooth armatures and on projection armatures and modifications thereof; and also between flat, open

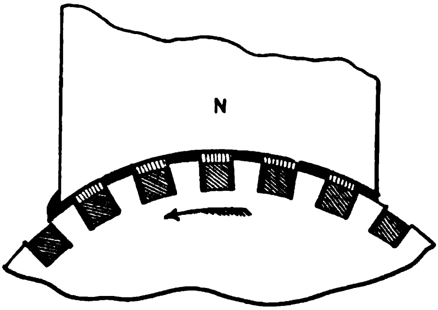


Fig. 9.

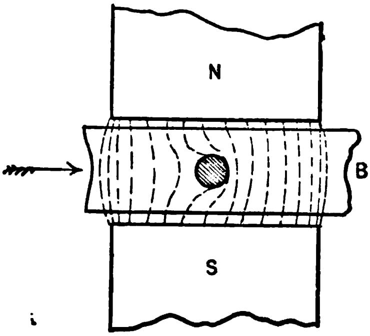


Fig. 10.

coils passing through a field and bobbins with cores of iron. The considerations advanced also bring out the relation which exists between closed iron circuit transformers and closed iron circuit (projection) dynamos, as we may call them.

I had intended at the outset of this paper to deal to some extent with the propagation of lines of magnetism undergoing retardation in reference to alternating current motor devices, transformers with limited secondary current, or constant average current, an alternating motor working with what I may term a translation lag, etc.; but it was soon found that these matters must remain over for a continuation of this paper at some future time. My endeavor has been in the present paper to deal with the lines of force theory as though it were a symbol of the reality, but I confess that it is done with many misgivings that I may have carried it too for. Yet, if we are to use the idea at all, it has seemed but right to apply it wherever it may throw any light on the subject or assist in our understanding of phenomena.

DISCUSSION.

The Chairman (Mr. Edward Weston):—Gentlemen, we have had the pleasure of listening to a very able and interesting paper by Prof. Thomson, and I am a little sorry that we have not adopted the plan of circulating the papers prior to reading them, so that every one might be fully prepared to discuss papers of this character. A paper like this requires considerable thought in its preparation, and to discuss it intelligently necessitates considerable thought. However, the matter is open for discussion and we shall be glad to hear any remarks.

Prof. M. M. Garver:—Prof. Thomson states that the phase with the closed iron was thrown back 90 degrees farther so that it was exactly opposite to that of the primary. That would furnish conditions favorable to producing a magnetic rotation by using the primary to excite one coil and the secondary to excite another at right angles.

Prof. Thomson:—No; the angular position in this case would be 180 degrees, not 90. The theoretical position is 90 + 90, which gives 180 degrees lag. That does not give you what you desire.

Dr. Otto A. Moses:—I listened with a great deal of attention and much pleasure to the very able paper of Prof. Thomson, and admire it exceedingly. But one of the strong cautions that Faraday gives, is that we should be very careful how we build scaffolding, because we have to take it down afterwards. There is an interesting hypothesis—I might almost call it nebular hypothesis—that emanated from Prof. Thomson which was at first blush exceedingly attractive, where he speaks of the expansion and drawing together again of certain rings around a conductor. That is very pretty at the first glance, but there seems to me one insuperable objection, to which I would like to call his attention. Perhaps he may remove it from me instantly. But I cannot conceive of a line of force closed upon itself expanding and producing flow. When the eléctric force emanates from a conductor in the form of a ring how can it by a recession upon itself cut lines of force and produce a flow? There would be a flow in one half of the line and there would be a corresponding negative flow in the other half. So that that hypothesis, in my mind, does not furnish a satisfactory explanation of the flow which he tried to describe.

Prof. Thomson :—I do not understand to what flow you refer particularly in this case—the flow of the lines or the flow of the current?

Dr. Moses :—The flow of the current engendering as you say this action, which twined back as it were upon itself, produces the current.

Prof. Thomson :—I do not think I have advanced any explanation of just exactly how that effect is brought about. I was dealing with the effect itself. There is something which we call the magnetic field indicating strains in the ether. Those strains appear to me to be relieved by collapsing upon a conductor. Now, it is a fact that when they collapse they give energy to the conductor, how, I don't know. I do not think I advanced any reasoning on that point.

Mr. Wetzler :—I do not like to prolong this discussion seeing that there are other papers to be read, but I merely rise to remark that some four or five years ago, in a lecture delivered before the New York Electrical Society, Mr. Mailloux, in attempting to explain the manner of electro-magnetic induction, employed an analogy somewhat similar to that which Prof. Thomson has just brought before us, likening lines of force to springs and the snapping of them on the passage of the conductor. Possibly he may have a few words to say on the subject.

Mr. C. O. **Mailloux**:—I thank Mr. Wetzler for recalling the circumstance. I did deliver a lecture before the Electrical Society, in 1883, which was reported at some length in the *Electrical World* of March 10th. That gives a fair idea of the principles which I advanced at that time. I also sent a contribution some time previous to that, to the London *Electrician*, published in July, I think, 1880, which briefly refers to the fact. The conception came to me as the result of some original investigations, which I made in 1879, while investigating a phenomenon which was thrust upon the world by an itinerant inventor by the name of Garey. He brought out a peculiar magnetic phenomenon wherein, he thought, he had observed a neutral point in the magnetic field not far off from the pole. I wrote a paper on the subject which was illustrated with some carefully prepared magnetic spectra, and I must acknowledge that the paper was sent and returned to me as not being considered fit for publication. Whether the subject was too far advanced for their ideas (laughter) or what was the matter, I do not know. At any rate I received a polite letter stating that they did not care to use the matter, and I still have the paper. I have often thought that perhaps there was a chance to immortalize myself in a mild way in seeing what has come out since, and I have several times started out to publish these spectra. One or two of them will be found published in the *Electrical World* with the report of that lecture. The others I still have in my possession.

I would say that it is too late for me now to enter at any length into the discussion of Prof. Elihu Thomson's paper, but, that perhaps, it will be better for me to send in a contribution as a sort of appendix to his paper. There are some points in which he and I would be probably found to differ. I have believed in the theory of the elasticity of lines of force for some years. I employed the term resiliency in my lecture, and I still adhere to it, as being in my opinion, somewhat more appropriate, and I have believed for a long time that when the mathematical theory of elasticity shall have been developed to a sufficient point that we shall have all of the data necessary for working out a perfect theory of magnetism. In other words, I believe that magnetism is nothing more than a manifestation due to the elasticity of the surrounding medium; that it is an elastic disturbance, and I may state in passing, that it is here that I differ with the conception advanced by Faraday and Prof. Thomson as to the shortening of the lines of force. I think that the idea of shortening is rather vague. I hardly think that we can say that lines of force shorten as such. It would probably be more appropriate to express it in some other way. What seems to us to be shortening is probably merely the manifestation of some other phenomenon.

Another point is the snapping of the lines of force. I shall endeavor to present some experiments which illustrate the fact that lines of force do snap and break, as I did in the lecture of 1883, and that the snapping is not simultaneous.

I will say in justice to Prof. Thomson, that he has elaborated the conception far beyond what I have done myself; and that he has pointed out conclusions which are perfectly logical, and which do not appear to me to be questionable for one moment, and which ought to be of the greatest theoretical and industrial value, and that I consider the paper which he has presented to us to-day as one of the most valuable papers, without any disparagement to the others.

MR. J. P. WINTRINGHAM:—I would like to ask a single question. In the closed iron ring the magnetism is retained in store and it is not destroyed until a contrary current is sent through the wire; suppose there was a very small current in the contrary direction, would the magnetism of the ring suddenly collapse at some certain point?

PROF. THOMSON:—Hardly, I think. There might be a weakening due to the passage of the current, but until the current becomes of sufficient power to practically overturn all the lines, the sudden collapse could not take place. It is true that there is a good field for experiment, to see just exactly how much current would be necessary to take out the magnetism of a closed ring. It would depend, I think, on the coercive force present. If the iron were hard, a larger current would be needed than in the case of exceedingly soft iron, which approaches perfect freedom of motion as far as these lines go.

DR. LOUIS DUNCAN:—I made some experiments on that question. In the first place, with alternating currents we used a transformer and an induction coil of about the same capacity that Prof. Thomson spoke of, one with closed and the other with open circuit. We got very much the result that he predicted in theory. That is, the difference in phase in the closed circuit transformer was about 180 degrees; in the other it was about 90. As to the collapsing of the lines of force, it is gradual. After the current begins to reverse, the number of lines of force at first gradually decrease, and then decrease faster; but it takes about one-fourth of the force with soft iron that it takes to saturate the iron.

MR. GEO. H. STOCKBRIDGE:—I shall not attempt to discuss Prof. Thomson's paper from a scientific point of view, but it seems to me that it is of interest on other grounds, as being a theoretical treatment of an important subject before the American Institute of Electrical Engineers. Such papers have been presented here before, it is true, but they are out of the line of most of our work here in America and are, for that reason, noteworthy. Allusion was made at the meeting last evening to the fact that electrical science has been developed in this country in the factories and not in the laboratories, and that the commercial interests involved have been at war with the scientific.

This has been undoubtedly a striking characteristic of our work, and has led to the charge that our methods are hap-hazard and unscientific; and in view of the silence of even our best known inventors on purely scientific phases of their work, the charge is not to be wondered at. One who is on the ground can understand why the consideration of patent rights and business emulation should keep the mouths of inventors closed, but to an outsider it is not so clear. These business considerations, again, are of far greater importance in a country where the people, less fettered by tradition, eagerly demand every improvement, and so provide rewards for him who is first in the field.

While these facts explain the nature of our inventive record, it is to be feared that the conditions made necessary by the facts —I mean the conditions of silence and secrecy and the hurry to get ahead of somebody else—have tended to make our methods *really* unscientific and hap-hazard. Some of our successful electrical inventors have learned all they ever knew in the factory, and have gone on to a happy-go-lucky success without much idea of what they were doing and what was the force with which they were dealing. It is certain, at least, that the hurried methods induced by competition have produced with some, the absolutely unscientific habit of neglecting to make a proper record of experiments. There is no excuse for lighting one's cigar with a ten dollar bill, and it is equally unpardonable to neglect to preserve for the benefit of others the details of an experiment which did not happen to prove of value to one's self.

This state of things makes its own comment upon a paper like Prof. Thomson's. If any of you should take the pains to look back through the files of the Institute's transactions, you cannot fail to note that the majority of the papers dealing with theories have been presented to the Institute by those bearing, like the reader of the present paper, a title indicating the teacher. Profs. Anthony and Rowland, Dr. Duncan and Prof. Nichols are names which will occur to you at once. In other words, the results show that laboratory training is, after all, the one which gives us the instinct toward a study of the laws, and a desire to formulate them for the general benefit. It is a matter of congratulation that the instinct and the desire have remained with Prof. Thomson, even after a protracted training in the bad scientific school of the factory.

THE CHAIRMAN (Mr. Weston):—I would like myself to make a few observations. The subject is a very interesting one, and I do not like to cut the discussion too short, but we have not very much time left. As I understand Prof. Thomson, he does not intend to present any new theory to explain the mechanical processes or movements that take place in the production of a current or a magnetic circuit. He takes advantage of the previously existing theories and brings before us a mass of facts of great importance in relation to the practical application of the principles involved. I doubt whether any one has had as clear a conception of the action of the transformer, as has been presented here to-day. Lines of force are not new, nor is their study a new subject. Faraday was not the originator of them, as is commonly supposed; but Sturgeon: one of the men who did a vast amount of useful work in the early history of electrical science, but whose name is very seldom seen or referred to. He was so overshadowed by Faraday's researches, living at the same time as Faraday, that his work has never been fully appreciated. He, I think, was the first man to call them lines of force. I may be mistaken about that. It may be that there was the equivalent of the lines of force in the work of the celebrated Gilbert. His description and also his cuts indicate that he had a pretty clear conception of the subject, but it appears Sturgeon is entitled to more credit than is generally given him. The question as to what is the actual mode of action of these forces is one that we cannot penetrate or appreciate fully yet. Science has got to wait, probably for some centuries. We are getting clearer notions of the matter, for we have now notions which did not exist a few years ago. We have got a clear conception that there is such a thing as a magnetic circuit and such a thing as magnetic resistance. We have got a notion that there is such a thing as an inductive circuit and inductive resistance, and we have got a notion that there is a conductive circuit and conductive resistance. Now, those terms are employed in a rather free manner, because there is very little doubt that the actions themselves are very closely allied and related to each other. These notions have, however, helped us in our practical work a great deal. That we shall be obliged to abandon finally the lines of force theory, I think is likely. It is simply a temporary expedient to enable us to get a fair conception of certain actions that do take place in accordance with certain laws; just as the atomic theory in chemistry will disappear unquestionably as the mathematical relations of the elements to each other become more clearly understood. They are useful hypotheses, but only hypotheses, and the application of them is not injurious. The subject is too large to discuss very fully. I had made quite a number of notes, but I shall have to stop and tender the chair to Prof. Thomson.

PRESIDENT THOMSON here took the chair and said:

I would like to make an announcement which I believe is in accordance with the ideas of the Institute, that where there is not a chance for discussion and the time is short, it would be just as well to adopt the plan that is often adopted under similar conditions in other societies; to have each one write what he wishes to say on the subject and turn it in to be published in the proceedings. I think that would enable us to get more matter into a discussion, and often times carefully selected matter.

MR. WESTON:—I would suggest at the same time that it would be very much easier for those who take part in the discussion if they had copies of the papers. It is very difficult with the pressure of other matters to keep in mind all the points that have come up, unless you have the papers before you.

THE PRESIDENT:—Many of the societies have adopted that plan and they find it works with excellent satisfaction, and I think we will have to come to it.

A paper on the Relation between the Initial and the Average Efficiency of Incandescent Lamps was then read by Mr. W. H. Peirce as follows:

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, and discussion thereon.

THE RELATION BETWEEN THE INITIAL AND AVERAGE EFFICIENCY OF INCANDESCENT ELECTRIC LAMPS.

BY WM. H. PEIRCE.

At the present day there is very little reliable published information relative to the variation with age of the economy and light giving power of incandescent lamps. However well this law of variation may be known to lamp experts, the author believes himself to be justified in saying that the majority of electrical engineers are entirely unacquainted with it. The most complete data, and the latest, at least in this country, is that given in a report in 1885, to the Franklin Institute by a committee appointed to test lamps and dynamos. At that time the successful commercial lamp was but two or three years old, and since then the development of lighting by incandescence has been so remarkable as to cause this lamp test now to be almost ancient history. When considering the possibilities of incandescent lighting in depots, offices and shops, the fact of the absence of trustworthy records of late date induced the following investigation to be made by the author in the interest of the Chicago, Burlington and Quincy Railroad Company.

There were in all 94 lamps studied, embracing 4 of the most prominent makes of lamps now on the market. Fifty-nine of these were purchased—15 of three kinds and 14 of one type of lamp. Care was exercised to obtain the commercial lamp. After the preliminary measurements had been made upon these 59 lamps, the local agents for them were invited to make examination of the test room and of the methods pursued, and as an outcome of their visits the remaining 35 lamps were tested upon their solicitation. These last lamps were in every case sent direct from their respective factories especially for this test, which fact, I am rather prone to construe as a vote of confidence by the various lamp companies in our system of measurements.

The Howell voltmeter was used for determining the volts in all measurements, and the following is a brief description of its principle: The pressure to be measured is caused to drop through the resistance A D, Fig. 1, equal to about 4,500 ohms, and a Daniell cell B is so arranged that when the key K is closed the electromotive force between C D is opposed by that of the battery, and hence no current passes through the galvanometer G, when the electromotive force between C and D is equal to that of the cell. In practice the rider C is moved along the resistance until no current flows through the galvanometer, and the electromotive force between A D is then determined from the formula:

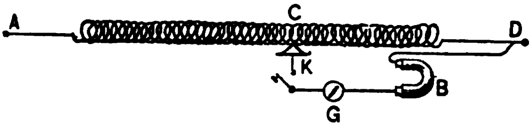


Fig. 1.

$$V = v\frac{R}{r} = 1.08\frac{R}{r};\ \text{where}$$

V=Volts between A D.
v=Volts between C D=electromotive force of cell.
R=Resistance A D in ohms.
r=Resistance C D in ohms.

Naturally the completed instrument avoids this calculation for each observation by having the rider C move along a scale reading to volts. With the instrument used it was found that when the rider pointed to 110 volts that R=4,493 and r=44.4, and hence V = 109.3. Similarly, when the instrument read 100 volts, the true reading should have been 99.4, and so when making observations a correction of .6 volts was applied. The electromotive force of the cells was 1.08 volts, as determined at the laboratory of the Edison Lamp Works. The correctness of our voltage indications being thus dependent upon the battery, a standard liable to variation, it was decided to frequently obtain new cells direct from the lamp company's laboratory. In the early part of the test considerable care was exercised in comparing the voltmeter with four or five others at hand, and just before starting the duration test, four new cells were obtained, and at intervals others were added, as shown by the following table, until ten in all were had. In table No. 1 the cells are numbered from 1 to 10, in the order of their obtainment. Those cells which checked with one another are marked O. K. The last received cells were always assumed to be correct, and where the old cells differed from them the amount of variation is given in per cent. of the electromotive force of the cell.

TABLE NO. 1.

Cell Number.	Dec. 27, 1888.	Jan. 8. 1889.	Jan. 15.	Jan. 26.	Feb. 11.	Feb. 27.	March 14.	April 1.	April 29.
1.....	O K.	O. K.	O. K.	O. K.	O. K.	O. K.	⅝% low	1¼% low	1¼% low
2.....	O. K.	O. K.	Low	Low	Low	Low	⅝% low	⅝% low	1% low
3.....	O. K.	O. K.	O. K.	O. K.	O. K.	O. K.	½% low	1% low	1% low
4.....	O. K.	O. K.	O. K.	O. K.	O. K.	O. K.	½% low	¾% low	1% low
5.....				O. K.	O. K.	O. K.	½% low	½% low	½% low
6.....				O. K.	O. K.	O. K.	O. K.	O. K.	O. K.
7.....							O. K.	O. K.	O. K.
8.....							O. K.	broken	
9.....									O. K.
10.....									O. K.

In this table it is shown that on December 27th and January 8th, all four cells were apparently correct—that is they check with one another. That on January 15th, three cells out of four; January 26th, February 11th, February 27th, five of six cells; March 14th, three of eight cells; April 1st, two of seven cells, and April 29th, four of the nine cells were O. K. And further, that the maximum variations at any time between any two cells was 1¼ per cent. It is thus seen that we are reasonably assured that our electromotive force was practically constant.

The cells were mounted on a switch-board, so that any two or all of them could be connected in multiple series by inserting plugs, and they were compared with each other by rapidly substituting first one and then another of the cells in the voltmeter while measuring the electromotive force of the test room circuit. The last two or three cells received were always used when making determinations on lamps.

The ammeter was of the Bergmann solenoid type, having a range of from .2 to 1.0 ampere. The readings of the instrument were calibrated at the beginning of the test by copper voltameters, the $CuSO_4$ solution of which had a specific gravity approximating 1.16 and the area of the cathode was 4 square inches per ampere measured. The coulomb equivalent was taken as .0003288 gr. The arrangement for calibration was as sketched in Fig. 2, where A is the ammeter, L a lamp to consume the electrical energy, P a voltmeter, and R an adjustable resistance for keeping the pressure uniform at the lamp terminals, as indicated by the voltmeter, and hence to insure constancy of current. The circuit through the voltameters was kept closed for fifteen minutes as timed by a stop watch. Extreme care was exercised in preparing the plates and the preventing of oxidation between weighings. The following table shows the current indicated by the ammeter and that obtained by the voltameters in the last made calibration by copper voltameters.

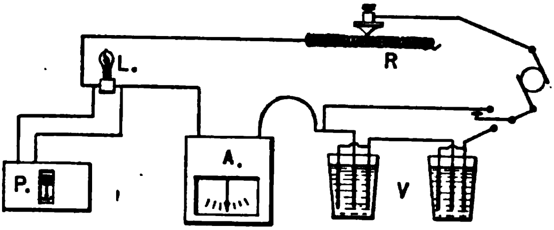


Fig. 2.

TABLE No. 2.

Readings of ammeter.	Readings of voltameters.		Readings of ammeter.	Readings of voltameters.	
.300	.297	.296	.700	.698	.692
.350	.350	.347	.750	.745	....
.400	.399	.398	.800	.791	.796
.450	.450	.451	.850	.844	.840
.500	.498	.498	.900	.898	.894
.550	.549	.545	.950	.947	.948
.600	.588	.588	1.000	.994	.998
.650	.635	.641			

This table shows that the ammeter was calibrated at every .05 of an ampere from .3 to 1.0 ampere, and that the maximum variation between the voltameter and ammeter readings was about .01 ampere, and that generally the two readings checked to within .005 ampere.

After these determinations the scale was changed to such an extent as was estimated would correct the readings of the ammeter.

The photometer was a 60-inch bar, Letheby-Bunsen, in combination with the Methven two candle slit. The Methven standard was accompanied by a certificate signed by John Methven, stating that a two-candle power light was emitted through the slit, when the quality of the gas used was such as to give from 15 to 20 candles with a flame three inches high. In order to prove the correctness of this standard when using Chicago gas, a comparison with Suggs' standard sperm candles was instituted. Ten different candles were used, two at a time, and the observations were made in accordance with the rules of the London gas referees. Table No. 3 shows that the Methven standard varied less than 1 per cent. from the power of two standard sperm candles; this conclusion being based upon 100 observations.

TABLE No. 3.

COMPARISON OF METHVEN STANDARD TWO-CANDLE SLIT WITH SUGGS' STANDARD CANDLES.

Series.	Number of Candles.	Brand of Candles.	Grains of Sperm burnt.	Rate of burning Sperm per hour for one candle.	Time of making observations.	Average of ten readings.	Corrected average reading.
1......	2	Suggs.	40	118.0	10'10¼"	1.968	1.936
2......	2	"	40	114.8	10'28½"	2.034	1.942
3......	2	"	40	116 1	10'20¼"	2.064	1.998
4	2	"	40	114.6	10'27¼"	2.010	1.920
5......	2	"	40	114.8	10'28½"	2.080	1.984
6.	2	"	40	114 9	10'26"	2.140	2.050
7......	2	"	40	118.8	10' 6¼"	2.090	2.008
8... .	2	"	40	119.2	10' 4½"	2.038	2.024
9	2	"	40	114 2	10'31"	2.068	1.966
16......	2	"	40	118.8	10'33"	2.066	1 960

Grand average of 100 observations............. ....1.984

It was recognized that in a test of this kind the four factors most liable to variation were the voltage, the current readings, the standard of light and the personal errors of the observers. It was considered that the most certain and most practical way in which to guard against such errors would be to measure the candle power, the voltage and the current upon several lamps, and to put these lamps aside to be used only as reference lamps.

Accordingly five lamps, which had previously been allowed to burn at an abnormal voltage for nearly 100 hours, had for them the above several quantities determined, and the correctness of the apparatus and personal readings were checked up by the reference lamps whenever observations on the test lamps were about to be made. Table No. 4 exhibits the readings of these lamps. It will be seen that the candle power determinations vary from about 4 per cent. below to 4 per cent. above the first readings made on the lamps, and hence this table proves conclusively that the conditions of the test were essentially the same at all times of its duration. In addition it may be said that for the determination of the candle power of these lamps the mean of five observations at the same point was always taken.

TABLE NO. 4.

READINGS OF REFERENCE LAMPS.

Date.	Lamp No. 1 101 volts.		Lamp No. 2 101 volts.		Lamp No. 3 100 volts.		Lamp No. 4 100 volts.		Lamp No. 5 100 volts.	
	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes	C. P.	Amperes
1.2 89	9.94	.505	9.24	.490	9.40	.480	9.94	.495	9.08	.490
1.8.-	10.10	.505								
1.11.-	10.20	.505								
1.15.-	10 20	.500	9.30	.495	9.40	.488				
1.16.-	10.30	.500								
1 26.-	9.80	.500	9.30	.490						
2.11.-	9 55	.500	8.90	485	9.30	.485				
2.27.-	9.92	.500								
2.28.-	9.66	.500								
3.14.-			9.30	.490						
3.15.-	10.20	.505			9.80	.485				
4.1.-	9.90	.500								
4.29.-	9.70	.505					10.1	.500	9.3	.490

The lamp test was started in August, 1888, at which time all instruments were calibrated, and preliminary measurements were made on quite a number of lamps. On account, however, of press of other work, the test was shortly afterward dropped until late in December. All apparatus and lamps remained during this interval untouched in the test room, and when again taking up the subject in December, the continued correctness of the instruments was proven by the fact that all of some ten or twelve lamps tested out then as previously.

In the preliminary measurements of new lamps the mean horizontal candle power and the watts of electrical energy consumed were determined, and from these were deduced the watts per candle.

In this paper the candle power of a lamp is taken to be the mean of twelve points 30 degrees apart in the horizontal plane of the lamp. The observed candle power is the power of the lamp at a point in the horizontal plane at right angles to the plane of the base of the filament. The reduction factor is the ratio of the mean horizontal to the observed candle power.

Curve of all Lamps, showing
Variation in Economy and Candle Power, with Life.

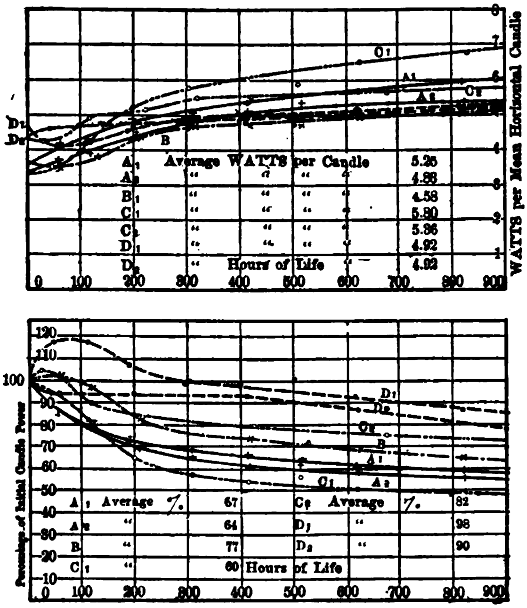


PLATE I

In the preliminary measurements three readings were made at the point where the observed candle power readings were to be made, and the lamp was marked so that this point could always in the future be determined. The lamp was then revolved upon its principal axis, and two observations were made every 30 degrees, the starting point, when again reached, also having two more readings made. The five readings made at the starting point were averaged, and the result called the observed candle-power. The observed candle power and the readings at the other eleven points were then averaged, and this result called the mean horizontal candle power. In the duration test observed candle power readings only were made, the mean horizontal candle power being always found by multiplying this reading by the reduction factor.

The wire connections to the lamp in the photometer were the same as shown in Fig. 2, excepting that no voltameters were in circuit.

A sliding curtain was hung from the two photometer screen boards, and an opening was maintained only sufficiently wide to permit the reading of the disc. Two screens of deep blue glass were so placed as to shield the person making the candle power determinations from all glare of the lights. Two observers made the measurements. The author made the photometric readings and regulated the height of the gas flame, always looking at the flame through deep blue glass, and an assistant placed the lamps in position, adjusted the voltage, made ampere readings, and recorded all observations.

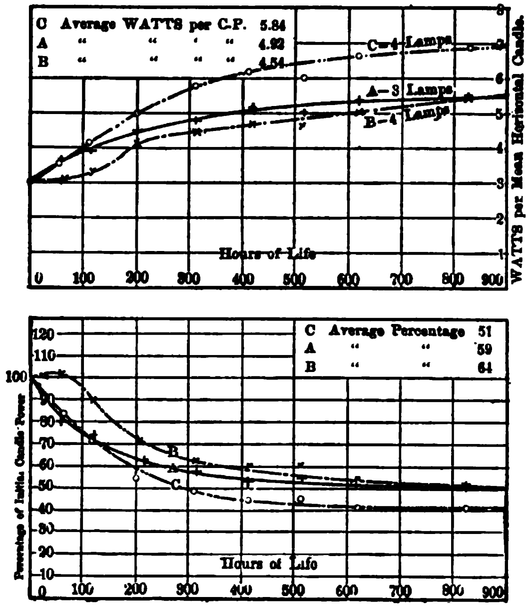

Curves of Lamps with
Initial Economy greater than 3.2 WATTS per C.P.

PLATE II

The candle power readings were always the mean of at least two observations, and if these two differed from each other more than 4 per cent., several others were made, and a mean of all observations taken as the correct reading.

A small room, 13x19 feet, in which to conduct the test was partitioned off about 40 feet from the dynamos. In this room were located the photometer and other instruments, and all test lamps. The door leading to this apartment was kept locked, no one being allowed to enter during my absence except the engineer in charge of the plant, who twice a day inspected for broken lamps.

Curves of Lamps with
Initial Economy 3.2 and 3.5 WATTS per C.P.

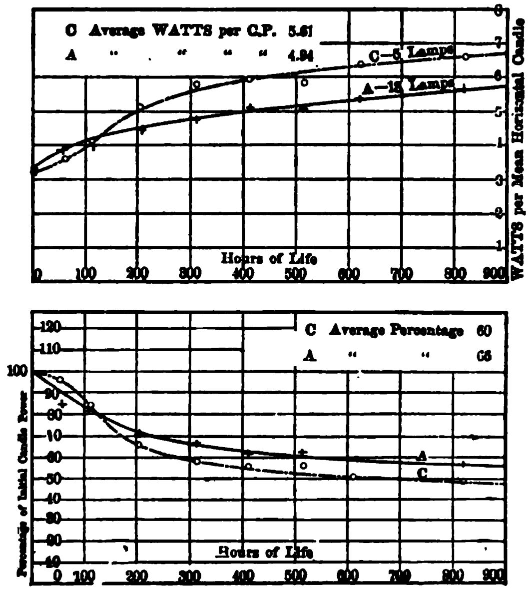


PLATE III

The lamps were wired in groups of 10 and 15, those of similar marked volts being placed together.

The volts labeled on the lamps by the manufacturers were taken as being the correct pressure at which to run them. Suitable resistance was introduced into those circuits on which were lamps requiring a less pressure than 110 volts, the normal pressure of the house circuits. No lamps were permitted nearer to each other than 12 inches, for fear of possible harm occuring to one from the heat of the others. The temperature of the air when all lamps were burning was about 104 degrees Fahrenheit, as indicated by a thermometer near the centre of the room. The current was turned off and on the lamps during the first 550 hours in a gradual manner, such as was due to the starting and stopping of the dynamo, but after this time arrangements were made to also have the lamps switched off and on once or twice daily, so that the filaments would receive the shock of sudden heating and cooling as in usual practice. Excepting when photometric measurements were being made, the lamps burned whenever the dynomos ran, which was generally 9 hours per diem, 4½ hours in the morning and 4½ hours in the afternoon, with one hour intervening. In the mains leading to the test room was interposed a variable resistance coil by which the pressure at the lamps could be adjusted independently of that of the house circuit. Two pressure indicators were connected with the test lamps, one Howell governing the engineer, and one of the United States E. L. Co.'s pattern in the office of the superintendent of telegraph. In the latter place was a push button with which to ring a bell and thus call the attention of the engineer if the voltage varied immoderately. As a matter of fact, the pressure was generally within one volt of the normal as shown by the indicators, which instruments were from time to time ad-

Curves of Lamps with
Initial Economy between 3.5 and 3.8 WATTS Per C.P.

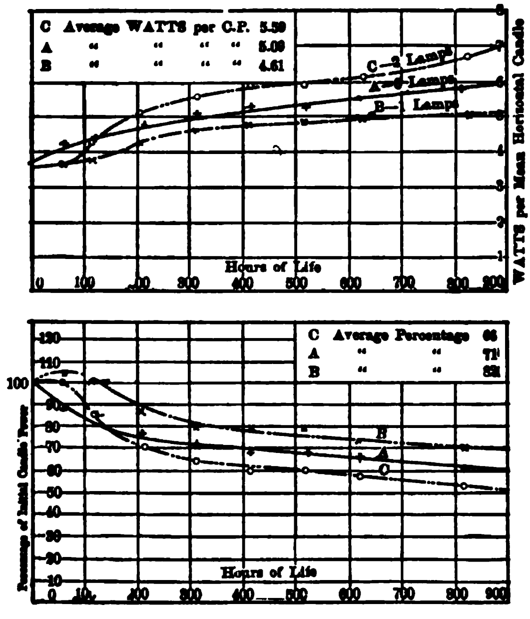


PLATE IV

justed if any variations were found between them and the voltmeter in the test room. A written statement was daily sent in by the engineer as to the exact hours of burning and the failure of lamps.

The duration test was started January 2, 1889, and was continued until April 29, 1889, thus permitting those lamps first entered to burn about 820 hours. The lamps contributed voluntarily by the various companies did not burn this long, because when they were received the test on the purchased lamps was about 200 hours under way.

Curves of Lamps with
Initial Economy between 3.8 and 4.1 WATTS per C.P.

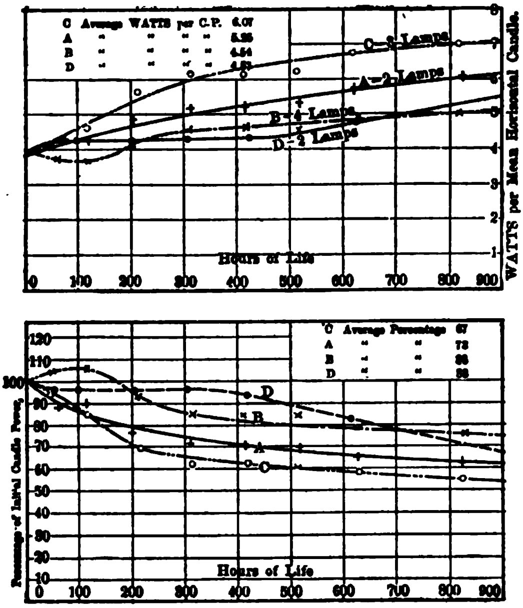


PLATE V

It was aimed to make candle-power and efficiency measurements of the lamps after they had lived 55, 110, 200, 300, 400, 500, 600 and 820 hours.

In the accompanying curves each make of lamp is designated by a letter: A_1, B_1, C_1, D_1, have reference to the purchased lamps; A_2, C_2, D_2, indicate those tested at the request of the lamp companies. In either case similar letters refer to like make of lamps.

Three series of curves have been constructed. In the first of these, Plate 1, is shown the variation with age in the economy and candle power of all the lamps. Each curve represents a batch of from 10 to 15 lamps, thus the C_1 lines relate to the 15 lamps purchased, and the C_2 curves indicate the influence of life upon the lamps contributed by the C lamp company.

The upper set of curves illustrates the economy of the several lamps at their different periods of life. Thus the A_1 curve indicates that these 15 A lamps when new, consumed 3.54 watts per mean horizontal candle, that after 900 hours, 6.1 watts of electrical energy were necessary to produce one candle-power of light, and further that during 900 hours the mean economy of the lamps was 5.25 watts per c. p.

Curves of Lamps with
Initial Economy between 4.1 and 4.4 WATTS per C.P.

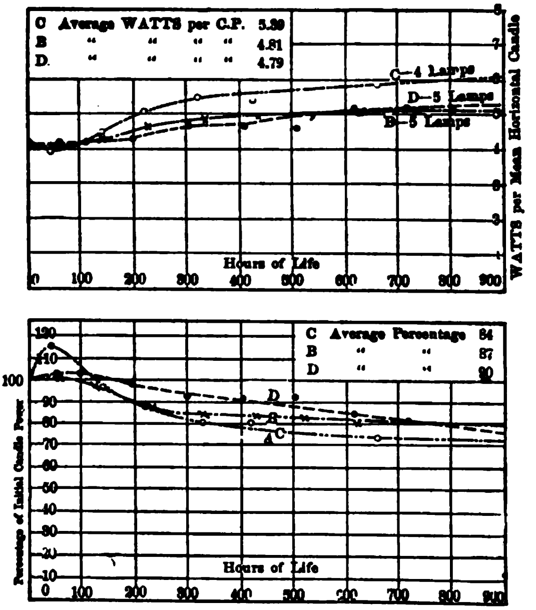


PLATE VI.

The lower diagrams indicate the effect of age upon the light giving power of the lamps. Thus the A_1 curve demonstrates that these A lamps after 900 hours gave but 58½ per cent of the initial candle power, and that the mean intensity of light during 900 hours was 67 per cent. of the new lamps.

In the construction of the second series of curves, Plates II to to VIII inclusive, the lamps have been divided into groups according to their initial economy. We have 7 sets of comparative

Curves of Lamps with
Initial Economy between 4.4 and 4 7 WATTS per C P.

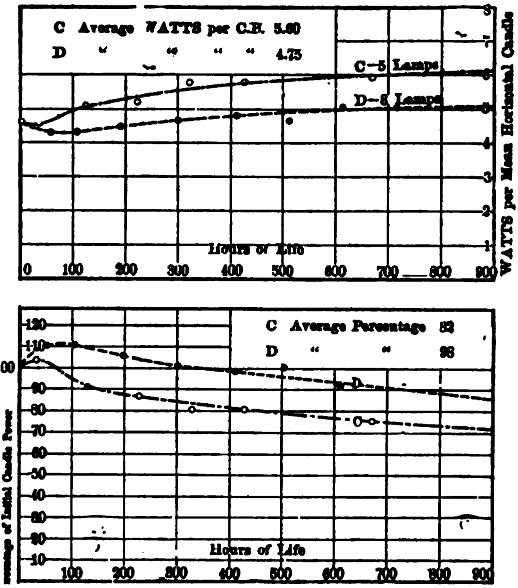


PLATE VII

Curves of Lamps with
Initial Economy between 4.7 and 5.0 WATTS per C.P.

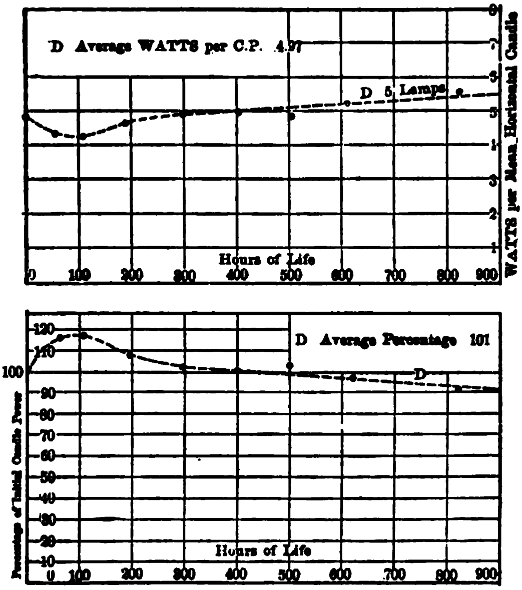


PLATE VIII

curves, the various lamps in each set having approximately the same initial economy.

In the third series, Plate IX, we have a summation of all previous diagrams and tables. These curves show the relation between the initial and average efficiency of the incandescent electric lamps studied, and are plotted with the initial economy of the lamps as abscissæ, and the corresponding averages

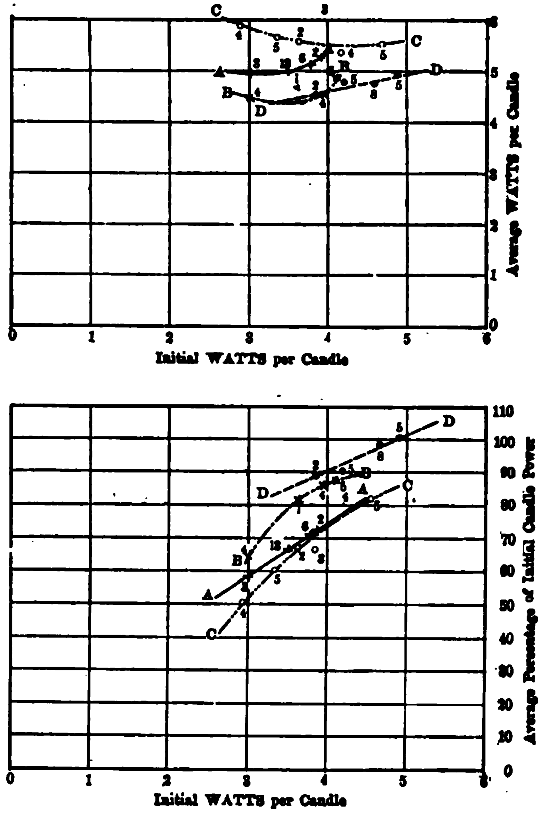


PLATE IX

deduced from the preceding curves as ordinates. The figures at the plotted points have reference to the number of lamps used in determining each point.

From these curves we learn that the varying of the initial economy between the limits of 3 and 5 watts per candle does not greatly affect the average economy, but it does have a very marked effect upon the average candle power derived from the lamp.

DISCUSSION.

Prof. Nichols:—I would like to ask Mr. Peirce if he would tell us something about the agreement between individual lamps. It seems as though the number of lamps is rather small from which to draw a general conclusion. It has been the impression that duration tests are only to be relied upon when the number of lamps used is very large. The need of just such work as this I think is very great, and the reliance we can put upon it would depend upon the meaning which we can give to these averages; that is, whether it is found that all the lamps of a single group behave in the same way. If the lamps were made from widely different material it would seem desirable to go to work again with a very much larger number of lamps.

Mr. Peirce:—Some of the different makes of lamps ran along very evenly. The lamps from factories which make a practice of sorting their lamps, gave very even results, and those from factories which did not sort their lamps would diverge in candle power very considerably. In one instance, a lamp supposed to be 16 candle power varied from about 16 to about 30. I think there is too small a number here to try to establish a law, but there is a sufficient number to show very nearly which way the straws blow. Take for instance the B curve; that is almost a straight line. You have got 20 lamps there. I said that the C, the D and the A curves were the most satisfactory, because they seemed to show a general law; but the B is not so satisfactory, because it is a very much contorted curve. I don't believe that an essentially different result from that shown could be deduced if the number of lamps under test were largely increased, for with but one or two exceptions all the lamps of a single group behaved after a manner which could have readily been predicted from a set of curves like those shown in plate IX. It would, undoubtedly, be more satisfactory if a larger number of lamps were tested, for then not only could a greater range of initial conditions be obtained for each make of lamps—say lamps burning at from 2 to 5.5 watts per candle—but also lamps could be obtained which were not made at the same time. This must not, however, be interpreted to mean that this last condition was not met by the lamps under test, for it is known to me, barring lamps A_2 D_2, that in some instances a period of several months elapsed between the manufacture of lamps of the same group. I believe that if Prof. Nichols will examine the curves closely he will readily discover that uniformity which he is seeking.

A paper read before the American Institute of Electrical Engineers, New York, May 22d, 1889, with an introductory note by Edward L. Nichols, and discussion thereon.

THE EFFICIENCY OF THE ARC LAMP.

BY HATSUNÉ NAKANO.

Note.

The interesting results recently obtained by Mr. Merritt in the application of the method of Melloni to the investigation of the efficiency of the incandescent lamp have made it seem desirable to extend that method to the study of the arc lamp also.

Existing data concerning the efficiency of this source of light are exceedingly imperfect, being based chiefly upon measurements with the Bunsen photometer. Ordinary photometric determinations of the light giving value of sources of illumination differing widely in temperature from the standard candle are necessarily at fault; and I have endeavored in a recent paper[1] to call attention anew to the character of the errors involved in comparison of the light of the electric arc with that of other sources of artificial illumination.

The efficiency of the arc lamp has been expressed hitherto in terms of the candle-power of light produced per unit of energy expended. The estimate has been based in a few instances upon the mean spherical candle-power, as actually determined; more frequently upon the candle-power emanating from the lamp in a single, especially selected, direction, and in too many instances upon the "nominal candle-power." Such estimates of the efficiency of the arc are of small value. They afford us widely varying results which cannot be definitely compared, and even when they are based upon the most complete and careful determinations of candle-power and energy, they do not indicate the ratio of light-giving radiation to the total energy expended

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[1] The Efficiency of Methods of Artificial Illumination. Transactions of the American Institute of Electrical Engineers, Vol. 6, 1889.

within the lamp. It seemed, therefore to be of some importance to obtain the measurements of the ratio of luminous to total radiation in the case of the arc lamp by a method the results of which would be directly comparable with those already obtained for the incandescent lamp.

The investigation was undertaken, at my suggestion, by Mr. Hatsuné Nakano, M. E., of the Imperial University of Japan, the results of whose measurements are given in the following pages.

E. L. N.

On the Efficiency of the Arc Lamp.

The following experiments were made in the Physical Laboratory of Cornell University during the winter of 1888-89, the object in view being to determine the efficiency of the arc lamp.

It is a well-known fact that the comparison of the candle-power of differently colored lights, such as the candle and the electric arc, is only a rough approximation. By means of one of the methods recently described by Mr. Merritt,[2] however, we can determine the efficiency of the arc independently of its candle-power. The method in question enables us to find the ratio of the light-giving radiation to that of the total radiation of the lamp.

In his first experiments Mr. Merritt measured the heat given out by a lamp, which was immersed in the water contained in a calorimeter. It is evident that this method, though very ingenious, cannot be employed conveniently in the case of the arc lamp. His second method is, however, applicable to arc lamps as well as to incandescent lamps. This method and the arrangement of the apparatus were fully described by Mr. Merritt in his paper, read before the American Association for the Advancement of Science at their meeting in Cleveland. It was also described by Professor Nichols in his paper read before the American Institute of Electrical Engineers, March 12, 1889.

The application of the method to the arc lamp was as follows: The rays of the light were allowed to fall upon the face of a delicate thermopile which was in circuit with a sensitive galvanometer. The deflection of the galvanometer was taken as a

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[2] Ernest Merritt: Some Determinations of the Energy of the Light from Incandescent Lamps. *American Journal of Science*, Vol. 37, p. 167.

measure of the energy of the total radiation which fell upon the face of the pile.[8] After the deflection due to the total radiation had been measured, a glass vessel of rectangular shape one decimeter thick, containing a strong solution of alum, was placed between the lamp and the thermopile, and the reduced deflection of the galvanometer observed. The use of the alum cell was to cut off the "dark heat," and allow only the rays of the visible spectrum to reach the face of the thermopile. But a certain small percentage of the longer wave-lengths passed through, and a considerably larger percentage of luminous rays were cut off by the cell. To determine the correction for the former source of error, a cell containing an opaque solution of metallic iodine in bi-sulphide of carbon was placed between the lamp and the alum cell. Any deflection now obtained was due to the dark rays passing through the alum cell, for the iodine cut off the luminous rays entirely, but allowed the dark rays to pass through. It was found that the dark rays which passed through the alum cell were exceedingly weak—almost imperceptible in most cases.

The correction for the second source of error, viz., the absorption of the luminous rays by the alum cell, was determined photometrically. The average of some thirty measurements gave 26 per cent. for the value of the correction.

In my first measurements, the lamp (the centre of the arc) and the thermopile were placed in the same horizontal plane. The lamp used was a "rack-feed" arc lamp requiring 45 volts and 9 ampères for its normal operation. Experiments were made with a dozen or more of different sizes of carbons, ranging from 2-16 up to about 13-16 inch in diameter.

The efficiency, or the ratio of the energy of the luminous radiation to that of the total radiation, luminous and non-luminous, in this plane was found to be exceedingly small, varying from about

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[8] John Ericsson in his "Contributions to the Centennial Exhibition," 1876, says that the calorific energy imparted to a thermopile is not proportional to the arcs through which the needle of the galvanometer sweeps, as stated by Melloni, but that for deflections not exceeding 15 degrees. the calorific energy imparted to the pile by radiant heat is very nearly as the square root of the versed sine of the angle of deflection; the deflection of the needle at the termination of the first degree exceeding the energy transmitted, in the ratio of 100 to 89, and beyond 90 minutes the energy becoming greater than the deflection in a constantly increasing ratio. Ericsson does not state, however, what kind of galvanometer he used in his investigations. The one used by me was a low resistance reflecting galvanometer of Sir William Thomson's pattern. It had been tested by Mr. Merritt, who found that the deflections were proportional to the energy imparted to the pile. In my experiments this proportionality was assumed to be true.

5 per cent. to a little less than 15 per cent., the ratio increasing as the diameter of the carbons used decreased. This relation between the efficiency and the diameter of the carbons, which is shown graphically by means of the curve in Fig. 1, ceased to exist, however, when the point was reached at which the whole length of the carbons was rendered hot by the current passing through them. After this point was reached the efficiency fell off again.

Table 1 gives the results of these measurements. In the first column the diameters of the carbons (in thousandths of an inch) are given. The second column shows the "mean horizontal efficiency;" that is to say, the ratio of the deflection produced by the total radiation of the lamp to that due to the light-giving radiation alone, after the proper corrections for absorption and diathermancy have been applied. Each value is the mean of a series of observations. The third column gives the potential difference between the terminals of the lamp.

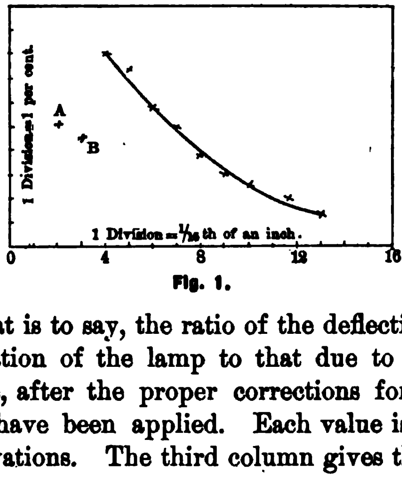

Fig. 1.

TABLE I.

Diameter of Carbons. (In inches.)	Horizontal Efficiency.	P. D. (Volts.)
.832	.01484	42
.730	.02162	35
.605	.02527	43
.563	.03125	38
.500	.03898	38
.432	.05014	38
.370	.05861	28
.305	.07333	31
.250	.0813	56
.185	.0458	25
.124	.0514	30

The curve C D, given in Fig. 1, shows the relation between the efficiency, measured in the horizontal plane, and the diameters of the carbons. The ordinates represent efficiencies, and abscissæ the diameters. It will be noticed that the points A and B corresponding to 2-16 and 3-16 inch carbons, are far below the curve, showing that the relation is modified as soon as the carbons become too small to carry the current without heating.

In the electric arc, as Professor Nichols has shown in the paper already referred to, "the entire light giving area is included between the line surrounding the positive carbon, which is at red heat, and the corresponding line upon the negative carbon. Now the surface of total radiation is much larger than that from which the light-giving rays emanate. It includes in addition to the incandescent surfaces near the arc, all those portions of the carbons which are heated, either by conduction or by the current. Were the distribution of total radiation in the vertical plane identical with that of the light-giving rays, the measurement of their ratio with the axis of the thermopile in any plane which passes through the arc would give us the efficiency of the lamp, but the curve of distribution of total intensity is not the same as that of candle-power, and the ratio in question is a function of the angle which the axis of the pile makes with the horizon. It becomes necessary, therefore, to make an exploration of the entire zone through which the lamp sends out rays, determining the ratio of luminous to total radiation for each angle and then to integrate the results."[4]

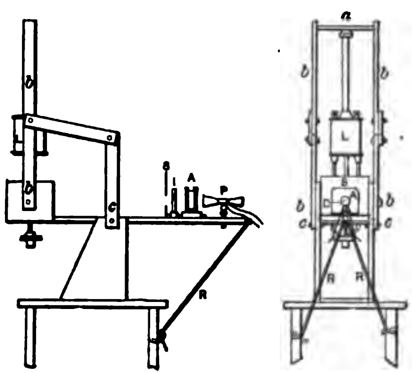

Fig. 2.

With such an object in view the piece of apparatus which is shown in Fig. 2 was devised. The lamp was hung from a horizontal beam *a*, supported by two upright pieces *b b*, which could be kept always vertical by means of a "parallel motion"

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[4] Edward L. Nichols. l. c.

arrangement. The alum bath A and the thermopile P were also carried upon a board, pivoted at *c c*, so that it could be turned to any desired angle. All necessary changes of angle could be made without altering the distance between the arc and the thermopile, or the relation of the alum cell to either of them. By means of this apparatus measurements were made at angles of

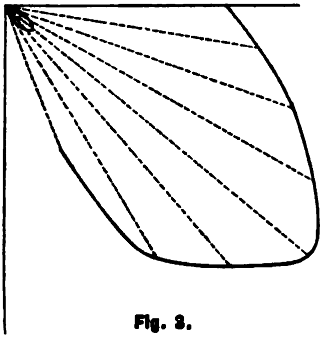
Fig. 3.

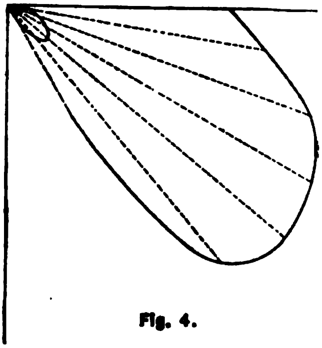
Fig. 4.

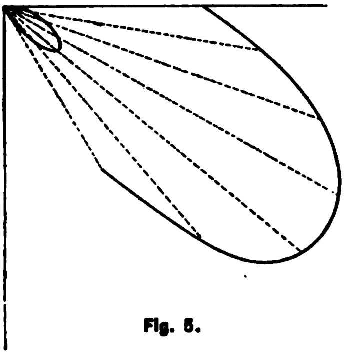
Fig. 5.

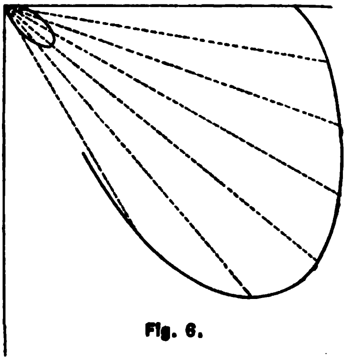
Fig. 6.

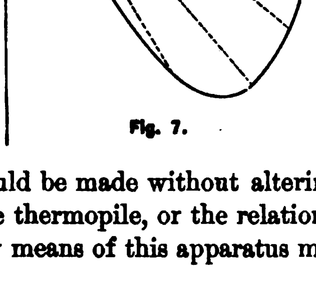
Fig. 7.

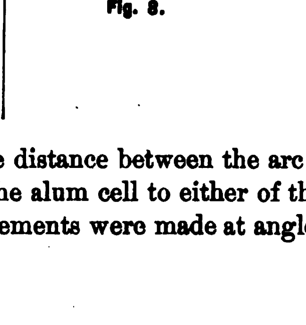
Fig. 8.

0°, 10°, 20°, 30°, 40°, 50°, 60° and 63° below the horizonta planes. The following carbons were used:

(1) Uncoated Carbons................diameter — .832"
(2) " " .... " .563"
(3) Copper-plated Carbons............ " .500"
(4) " " .... " .450"
(5) Uncoated Carbons................ " .370"
(6) Copper-plated Carbons............ " .250"

The results of these measurements are given in tables 2, 3, 4, 5, 6 and 7, and curves showing the distribution of luminous and total radiation, emanating from the lamp under the above conditions, are given in Figs. 3, 4, 5, 6, 7 and 8.

The radii show the positions for which measurements were taken and distances measured along these lines from the origin to each curve give the intensity of luminous and total radiation in each position. The curves of luminous radiation are similar in form to those of candle-power. Note, for purposes of comparison, the set of curves of candle-power at various angles published by M. Schreihage,[5] of the Polytechnic School at Braunschweig, who has made extended measurements of lamps with different sizes of carbons. In the following tables *D* is the deflection for total radiation, *d* that for light-giving radiation (corrected).

Table II.

Carbon (unplated), diam. — .832 inch.

Angle	d	D	$\frac{d}{D}$	*P. D.* (Volts).
0°	4.	220	.0182	44
10°	6.8	265	.0256	42
20°	16.4	300	.0547	42
30°	27.3	350	.0780	40
40°	35.1	390	.0900	40
50°	27.3	330	.0827	38
60°	20.3	300	.0677	39
63°	5.4	125	.0432	39

Table III.

Carbon (unplated) diam. — .563 inch.

Angle.	d	D	$\frac{d}{D}$	*P. D.* (Volts.)
0°	6.8	165	.0412	41
10°	10.8	195	.0554	42
20°	23.9	245	.0956	43
30°	32.8	264	.1242	39
40°	39.2	274	.1441	42
50°	28.4	252	.1137	41
60°	10.8	128	.0625	40

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[5] Schreihage, *Centralblatt fur Elektrotechnik*, No. 22, 1888. See also *La Lumiere Electrique*, T. 29, p. 585.

TABLE IV.

Carbon (copper-plated), diam. — 5 inch.

Angle.	*d*	*D*	$\frac{d}{D}$	*P. D.* (Volts.)
0°	5.4	110	.0491	45
10°	12.2	140	.0872	45
20°	21.5	172	.1250	45
30°	29.7	190	.1563	45
40°	33.8	195	.1733	45
50°	20.3	155	.1310	45
60°	4.1	93	.0441	45

TABLE V.

Carbon (copper-plated), diam. — .45 inch.

Angle.	*d*	*D*	$\frac{d}{D}$	*P. D.* (Volts.)
0°	10.8	197	.0548	45
10°	20.3	225	.0901	45
20°	29.7	242	.1228	45
30°	39.2	260	.1506	45
40°	41.9	270	.1552	45
50°	27.3	255	.1059	45
60°	12.2	180	.0676	45
63°	5.4	110	0492	45

TABLE VI.

Carbon (unplated), diam. — .37 inch.

Angle.	*d*	*D*	$\frac{d}{D}$	*P. D.* (Volts.)
0°	10.	180	.0555	40
10°	14.9	190	.0789	42
20°	24.4	200	.1220	38
30°	31.8	217	.1474	37
40°	39.2	230	.1696	40
50°	42.	240	.1750	42
60°	33.8	205	.1659	42
63°	10.8	115	.0869	38

TABLE VII.

Carbon (copper-plated), diam. — .25 inch.

Angle.	*d*	*D*	$\frac{d}{D}$	*P. D.* (Volts.)
0°	13.5	160	.0844	37
10°	22.9	185	.1238	37
20°	39.2	225	.1742	39
30°	45.9	255	.1800	38
40°	41.9	230	.1822	38
50°	29.7	150	.1980	39
60°	4.1	75	.0547	38

The distribution of total radiation differs widely from that of luminous radiation, so that the efficiency expressed by their ratio depends upon the angle of measurement. The values of the efficiency in each case for the various angles at which measurements were made have been calculated. They are shown in the diagram of efficiencies in Fig. 9.

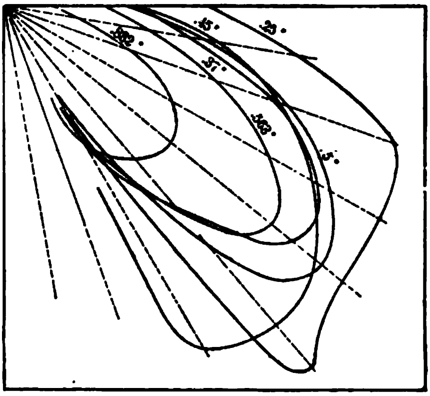

Fig. 9.

In order to determine the mean or "hemispherical" efficiency of the lamp the ratio of the radii of two semi-circles, the areas of which equal the areas of the curves of total and luminous radiation was found by means of a planimeter. The following are the values thus obtained:

TABLE VIII.

Diameter of carbons.	Spherical efficiency.
.882	.0687
.563	.1100
.500	.1266
.450	.1380
.370	.1554
.250	.1660

The above table forms the basis of the curve shown in Fig. 10, which represents the relation between the diameter of the carbons and the "hemispherical" efficiency of the lamp. The diagram is a straight line. Thus we see that the hemispherical efficiency varies inversely as the diameter of the carbon.

It will be interesting to compare this with the result of M. Schreihage's experiments. He measured the mean spherical candle-power with the different sizes of carbons, and found the same relation to hold true, as will be seen from the following table:

TABLE IX.

M. SCHREIHAGE'S MEASUREMENTS OF CANDLE-POWER AND CURRENT DENSITY.

Cross-section of positive carbon. mm.	Diameter d. m.m.	Spherical c. p.	d. X c. p.	Current density.
40	7.12	417	3,358	0.134
95	11.0	383	3,128	0.066
134	13.6	254	3,462	0.047
194	16.1	189	3,045	0.032
254	18.0	161	2,905	0 25

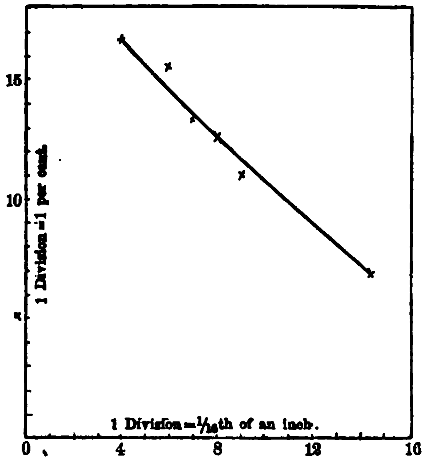

FIG. 10.

M. Schreihage states that this relation is not completely satisfied in the case of the mean horizontal candle-power, especially when the carbons are very small or very large. We have seen that this statement applies also to the experiments upon the horizontal efficiency described in the first part of this paper.

Physical Laboratory of Cornell
University, May 6, 1889.

DISCUSSION.

The President:—The subject of the paper just read is certainly one of considerable importance, and some of the conclusions are unexpected, you may say. As there are persons here having experience in this line, we would like to hear from them.

Mr. Prescott:—I would simply like to ask Prof. Nichols if this law that he discovered is independent of the current density. I understand the law to be that the efficiency of the arc lamp depends on the diameter of the carbon without reference to the current density.

Prof. Nichols:—I should have said that these experiments were made with constant current and as nearly as possible with constant electromotive force to the arc. The arc was maintained at about 45 volts, and the current was maintained at about 9 amperes; but quite a little range of voltage and current in the lamp showed surprisingly small changes in the efficiency. Probably toward the end of the test there was not the same care taken to hold the current and voltage constant that would have been taken if the efficiency had been found sensitive to changes in this respect, but the law holds true strictly where the current and voltage are maintained constant.

Prof. M. M. Garver:—In some experiments I made with arc lamps for 20 amperes we would use 7-16 carbon in summer, but in the winter, in order to make them last longer, we used the larger carbon. We found in our experience that the light was not as good, and consequently we would have to increase the current. However, when we got hard carbons they lasted better. We observed, also, in that case, without measurements, that the light was not so good with the hard carbon as with the soft carbon. I would like to ask if anything of the kind was observed.

Prof. Nichols:—I would say simply that it was not the intention to go into the question of different carbons. In starting the investigations we simply took such carbons as happened to be available in the laboratory, of different sizes. It was taken up originally as practice work, and I did not know at that time that it would ever come to the light, because I did not expect as good a result as was obtained; but all the carbons were probably carbons of good quality. They were of different makes, American and European.

President Thomson:—I would add a word just on this point from my own experience. I think, undoubtedly, the density and hardness of the carbon has an effect on the luminosity, and practical experience, without even referring to refined measurements, is that if a carbon is entirely too hard for the current, the luminosity is so much lessened that people complain at once, although the voltage and current remain the same. I can confirm what Prof. Nichols says also in regard to changes in the length of arc not changing the luminosity very much beyond a certain separation, when the light seems to go out freely, and as it mostly emanates from the carbon ends themselves, a little separation, more or less, will not make a great difference in the effects. It would be interesting in this connection to know exactly what was the combustion of carbon in the different cases. Of course, a part of the cost—the practical cost—is the rate of consumption of carbon, and sometimes we can sacrifice a little light by using a larger carbon and getting a longer life, since the carbon costs something in the running of the lamp. This gives rise to the question as to whether the combustion of the carbon itself does not have to do with the efficiency of illuminating power—whether, in other words, the carbon is not to be regarded in part as a piece of coal, which, if it burns rapidly, gives out more light in a given time than if it burns slowly; consequently with a low current density and small combustion of carbon we lose that effect and depend on the current altogether to produce the light. At the rate of burning of carbon in the arc light there must be a considerable amount of light, due entirely to the combustion of the carbon, without reference to the current, the current merely maintaining the carbon at that temperature which permits this effect to exist.

Mr. Weston:—I understand Prof. Nichols did not make any experiment with any other lamp except this arc lamp. Experience has shown, I think, that the efficiency of an arc lamp is very much increased where the power of the light is increased. If I remember rightly, some experiments I made myself in 1875 and 1876, showed that to a very marked extent, and I think experiments probably earlier than mine showed it. So that the efficiency, as it stands for an ordinary commercial lamp to-day, is not anything near what we can get if we make the lights larger. How far that would extend it is somewhat difficult to say, and as Prof. Nichols has all the appliances for making these measurements, including dynamos, I think it would be a very nice thing if we could get some facts in regard to efficiency of lamps, running up to 100 or 200 amperes instead of 9 or 10. It would probably throw a great deal of light on the matter. The small amount of useful luminous energy compared to the total energy employed, is a sad thing, always. That, together with the low efficiency of the steam engine shows how much we have to do. There is no lack of work in this field.

PROF. NICHOLS:—I would like to say that in presenting this paper I have presented it not as an exhaustive investigation of this subject, but rather as pointing out a line of work which, I think, may be a good deal of benefit to the study of the light-giving power of various illuminants, and that undoubtedly in order to make it complete it ought to be worked out for the efficiency as a function of the length of arc, independently again, as a function of current, and then those could be combined, so as to give it as a function of the energy for a constant length of arc. In short, it ought to be worked out under all the conditions under which lamps can be used. Here we have only worked it out for a single condition of affairs, because that was convenient to us—the ordinary long arc lamp of to-day.

PROF. GEYER:—May I ask Prof. Nichols whether any attempt was made to see if in the part which he calls luminous energy any appreciable amount, was energy other than luminous—that is, what is ordinarily called actinic. I should think that in a light centre, as an arc light, an appreciable amount might be other than luminous, which would reduce all those figures slightly; but the effect would probably be greater on those which are lowest on the column, and probably less on those which are on top of the column.

PROF. NICHOLS:—Mr. President, I would say that very careful corrections were made for the dark rays—the waves of longer wave length than the red which were able to pass through the alum, but the energy of the ultra-violet rays is so very small when measured on the thermopile that we regarded it as unworthy of mention.

DR. MOSES:—While suggestions are flying thickly, I would like to recall some experiments which might have bearing on future experiments in this direction. On one occasion, in order to get an absolutely uniform light I tried the effect of various colored media on an arc, and I found that with a monochromatic light, the general effect is more uniform throughout different areas; you did not see the sudden sharp flash of light that occurred by the reflection from the crater. Now, if experiments were carried on perhaps by allowing the arc to burn its way through a disc of mica, you would get a perfectly uniform monochromatic atmosphere and that would perhaps be the best medium for making these experiments. Monochromatic light may give uniformity of result.

The following paper on "The Spiral Coil Voltameter," by H. J. Ryan, was then read by Professor Nichols:

A paper read before the American Institute of Electrical Engineers, New York, May 22, 1889. and discussion thereon.

THE SPIRAL COIL VOLTAMETER.

BY HARRIS J. RYAN.

During the present year at the Physical Laboratory of Cornell University, the copper voltameter has been used somewhat extensively in laboratory practice by students for calibrating Thomson graded galvanometers, tangent galvanometers, and other electrical measuring instruments where a single determination suffices as a calibration. For practical instruction students are made to check their results on a Thomson gravity balance, and by means of the large galvanometer at the Magnetic Observatory.

The plate form of copper voltameter has been thoroughly and admirably investigated by Mr. Thomas Gray,[1] of the Physical Laboratory of the University of Glasgow. We are indebted to Mr. A. W. Meikle[2] and to Lieutenant Anderson[3] for clear descriptions, based upon the results of Mr. Gray, for the use of this form of voltameter, whereby determinations may be made so accordant, that one can scarcely think of more to be desired.

With all this, however, it has been our experience that students are slow to obtain accordant results when making their first attempts to follow Mr. Gray's methods. It seemed desirable to make use of a form of voltameter that required the least consistent time and care to construct and prepare for operation. Again, there should be a wide allowable range of current density, through which deposits would be as firm and adherent as possible. It was with a view of realizing this that we finally adopted and made use of the spiral coil form herein described.

A wire coil as a cathode seemed to present many advantages. Wire of high conductivity, good for this purpose, is generally

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[1] *Phil. Mag.*, 1886 and 1888.

[2] *Lond. Elec.* and *Elec. Rev.*

[3] *N. Y. Electrical Engineer*, October, 1888.

available in any desired size. A wire can be cleaned properly with great ease and readiness by fastening one end in a vise, holding the free part in one hand and sandpapering it with the other. By beginning at the vise to clean, it is not necessary to touch the cleaned wire with the hands throughout the complete operation of polishing and coiling into a spiral on a cylinder of the proper diameter, about which has been rolled clean paper. In this manner gain and loss coils are constructed.

Figs. 1 and 2 show the final form of voltameter adopted. The coils are hung vertically. The cathode is made of a smaller diameter and is arranged inside and concentric with the anode. The diameter of the anode is made about 3 c.m. to 4 c.m. larger than that of the cathode. The loss coils prepared as above are ready for use, since for well-known reasons their changes of weight are never accordant. The surface of the gain coil must not be touched by the hand. It is easy to handle it by the extending terminal, by means of which it is suspended in the voltameter. After polishing, it is washed by plunging into a jar of water containing a little H_2SO_4.

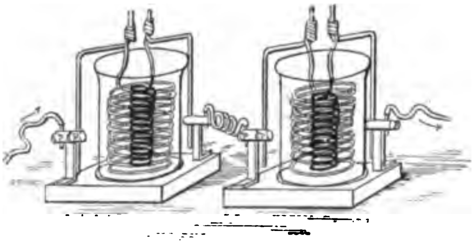

Fig. 1.

It is then rolled on filter or blotting paper to remove all but a mere film of the water. The coil is then dipped in 95 per cent. alcohol, removed and the excess of alcohol allowed to drip into the jar of the same. By rolling the coil on clean filter or blotting paper again, nothing but a mere film of alcohol remains, and that is thoroughly evaporated in a few moments, leaving the coil entirely dry.

Coils that have been laid away and become corroded can be readily cleaned, as is well known and directed, by plunging them into a mixture of strong HNO_3 or HCl, one hundred parts of the former to one of the latter, removing them quickly to a distance, such that the vapors of the acid shall not reach the coil during the rest of the preparation. This consists in washing, first thoroughly in water, and then proceeding as at first described by plunging in the acidulated water, alcohol, etc. After weighing, the coils are ready for the voltameter.

It has been customary with us to use a coil made of two and one-half metres of No. 16 wire, having the surface area of one hundred square centimetres.

For great strengths of current a number of these are arranged in parallel.

For every four amperes one wire is necessary. At the end of the deposit the gain coils are immediately removed, and plunged first into clean water and then into the acidulated, from which they are dried by means of alcohol in the manner above described. When dried they are at once ready to weigh.

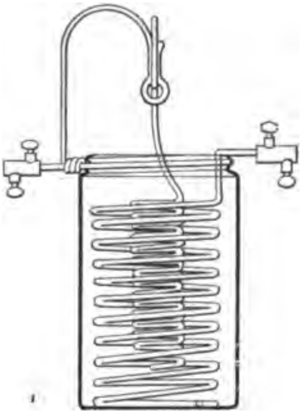

Fig. 2.

The copper sulphate, water and acid, need not necessarily be chemically pure. The density of the voltameter solution should be not less than 1.10 and not more than 1.18.

The question now comes to us as to what degree of accuracy and precision of results we may depend upon from the above form of voltameter.

The successful use of the copper voltameter has come from an appreciation and an understanding of the fact that when copper is immersed in an acidulated solution it passes slowly into solution in an irregular way. Mr. Gray has done much work in this particular, and has shown that the going into solution of copper on the cathode is curiously assisted by the current. From the extensive work done with the plate form of voltameter at Glasgow results have been gained with regard to the amount of copper that goes into solution for a certain current density and a certain temperature, so that it is a comparatively easy thing to make determinations with assured absolute accuracy to within a tenth or even a twentieth of one per cent. This is remarkable, though nevertheless true.

A Thomson deca-ampere balance was calibrated by means of the copper voltameter under Mr. Gray's direction at Glasgow.

When the same was received and set up at Ithaca in the physical laboratory of the University, it was compared with the large standard tangent galvanometer at the Magnetic Observatory, and found to agree with the same to within one-tenth per cent.

Surely this same or even a greater degree of absolute accuracy could be obtained with less trouble and experience by making use of the spiral coil cathodes. Mr. Gray has shown that the amount of copper which goes into solution, unaided by the current, is so small that we can in almost all cases neglect it. He has shown us how much it is when a plate is used as a cathode. There are strong reasons, however, for believing that when a wire which presents a regularly curved surface, is used as a cathode, the copper from it goes into solution in a more regular way, and probably in all cases to a considerably less degree. Again by the use of the spiral coil in a voltameter we find an advantage in the fact that the plane of each turn of wire is disposed horizontally, so that by convection the solution is not allowed to become weak near the cathode, nor dense near the anode.

This fact was first pointed out by Mr. Shaw, who speaks of the very accordant manner in which platinum wires disposed horizontally are found to act as cathodes in copper electrolysis.

In a valuable paper by Mr. Shaw[4] on "The Verification of Faraday's Law of Electrolysis with Reference to Silver and Copper," the very great range of current densities for the deposition of copper on platinum wires of 8 centimetres to 800 centimetres per ampere has been brought to bear on the ratio of silver and copper deposits when the currents are the same. From data taken from this paper, the writer has plotted curves that show the relation of current density to copper deposit throughout Mr. Shaw's work at the Cavendish Laboratory and Emmanuel College.

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[4] British Asso. Meeting, 1886. *N. Y. Electrician and Electrical Engineer*, 1887.

The work extended over a year, and was done with much care. These curves are shown in Fig. 3. The ordinates represent the amount of silver deposited by the same current that deposits unit weight of copper in a given time, corresponding to current densities, as represented in square centimetres per ampere on the the abscissæ axis. As is seen, with the exception of two, the curves are in remarkable accordance, differing by not more than 1·34 per cent. With regard to the two curves that fall above and below the rest, we would say that their position can easily be attributed to accidental causes; the error in the indications of the silver voltameter of a 1·5 of 1 per cent. either way would account for their position. Again, Mr. Shaw used no acid in the voltameter solutions, a fact that will account for irregularities. Of these lines the heavy one was drawn as a fair average of them all. A glance at this line will derive some interesting results:

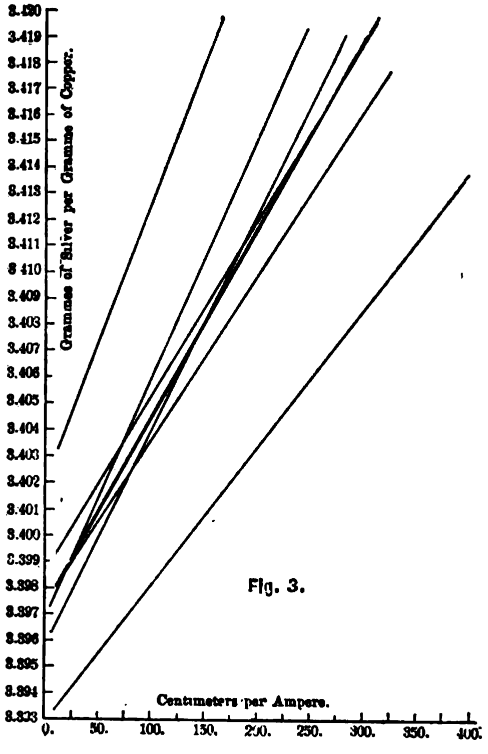


Fig. 3.

We see that with a current density of 50 square centimetres per ampere the ratio of silver to copper deposited is 3.401. In the early part of Mr. Shaw's work, in a long series of observations, this same identical ratio as a mean for a mean current density the same as above was found.

Mr. Gray when at work on the same ratio determined the same identical value.

Finally the writer has found that if we take the values for the chemical equivalents for silver and copper as quoted by Wurtz and those given by Meyer, that the calculated values for this ratio are 3.401 and 3.399 to 3.401.

Therefore, since we know with great accuracy the value of the electro-chemical equivalent of silver, we have an equal assurance that with a *current density of one ampere* for *every* 50 *square centimetres exposed*, the *amount* of *copper deposited* will be .0003287 *grammes* per *coulomb.*

Again, we see that in a range of current density from 50 centimetres to 300 centimetres per ampere, this line indicates a change in the amount of copper deposited from .0003287 to .0003270. It is to be remembered that these results were obtained by the use of a solution almost saturated, and without acid. Returning to Mr. Gray's work we find that for the same range of current density the amount of copper deposit changes from .0003287 to .0003272 for a temperature at which Mr. Shaw probably worked.

It is well-known that copper goes into solution under the action of the current from the cathode at a much greater rate without acid than with, and especially at a greater rate when the solution is dense than when working under the proper conditions for measuring current. A comparison of Mr. Gray's and Mr. Shaw's results brought us to the belief that less copper should be dissolved from cathodes of the spiral form than those of the plate form.

To determine this, four cells were arranged by Mr. Genung, a student in the Sciences at Cornell. Two of these had cathodes and anodes of the plate form and the other two, those of the "spiral coil" form. The large plate and coil and the small plate and coil had areas of 100 centimetres and 10 centimetres, respectively. About half an ampere was passed through them for a time, amounting to two and one-half hours.

Every half hour they were taken out, dried and weighed, the solutions all intermixed, and the deposits continued for another half-hour, etc.

The solution had a density of 1.1, and an addition of $\frac{1}{4}$ per cent. acid. An examination of the intermediate results gives the same result that one gets by looking at the total gain of the coils and plates taken separately. Mr. Genung's results were as follows:

Temperature, 20 degrees.

Coils.		Plates	
Small	Large.	Small.	Large
10 c.m.	100 c m.	10 c.m.	100 c. m.
1.8220	1.8182	1.8196	1.8138

The result shows that in going from a density of 20 centimetres to 200 centimetres per ampere, the amount of copper per coulomb changed from .0003289 to .0003283 for the coils, and for the plates, .0003285 to .0003275.

The result with the plates does not differ materially from that found by Mr. Gray for a similar range.

Mr. Genung also made a number of comparisons of the spiral coil volameter at different current densities, and at a mean temperature of 23 degrees centigrade, with the standard tangent galvanometer at the Magnetic Observatory. They were made with great care in every particular but one, and that was in the matter of time. This was taken from his own watch, that had not been compared with our standard chronometer. Again, to make and break the circuit, a large switch capable of carrying 250 amperes was operated, which added incidental errors, since the duration of each deposit was only 1,800 seconds; the results are interesting, because of their accordance and the manner in which they were obtained, and are given in the following table:

Date.	Size.	Current.	Gain in grammes.		Grammes per ampere per hour.
			Coil No. 1.	Coil No. 2.	
May 8 & 9.	110 c.m.	.6788	.8985	.8977	1.176
“ “ .	“ “	1.312	.7699	.7704	1.177
“ “ .	“ “	1.882	1.1060	1.1055	1.178
“ “ .	“ “	2.446	1 4364	1 4372	1.178
“ “ .	116 “	3.008	1.7686	1.7676	1.179
“ 6 & 7.	“ “	3 532	2.0755	2.0758	1.179
“ “ .	“ “	4.016	2.3639	2.3639	1.180
“ “ .	“ “	4.495	2.6386	2.6326	1.176
“ “ .	“ “	4.930	2.8932	2.8910	1.176
“ “ .	“ “	5 263	3.0750	3.0808	1.173
“ “ .	“ “	5.701	3.3437	3.3453	1.176
8 & 9.	110 “	6.432	......	3.7815	1 165*

* Deposit rough copper lost from both coils.

Fortenbaugh and West, also students in the laboratory, making use of a watch belonging to the former, known to the writer to have been keeping correct time, and otherwise precisely the same apparatus that Mr. Genung used, obtained the following result:

Duration	40 m.	
Current	1.314	
Coil No. 1 gained	1.086	Area,
" " 2 "	1.085	90 sq. c.m

From this the number of grammes per ampere per hour is computed to be 1.182. And we know that for the above current density it should be 1.183. The galvanometer at the observatory is subject to a fluctuation in its indications by an amount that almost covers the above, due to variations of horizontal intensity throughout the succession of day and night.

The writer and Mr. E. G. Merritt, Fellow in Physics, Cornell University, hope to make a thorough investigation of the "Spiral Coil Voltameter" in its use with silver and copper shortly.

From the ease with which those inexperienced in the use of the copper voltameter obtain consistent results, it was thought well to recommend its use in the meantime to others.

DISCUSSION.

MR. WESTON:—For some years I had occasion to do a great deal of work with a copper voltameter and I was finally compelled to give it up. I found it to be exceedingly unreliable; and no doubt part of that is due to the fact that the circulation of the electrolyte is not as perfect in some cases as in others. I should expect that the better operation of this voltameter as compared with the plate voltameter was largely due to the fact that the electrolyte circulated much more freely and prevented the formation of secondary compounds and the deposition of them with copper. It is, I know, the general impression that copper generally goes down in an extremely pure condition when precipitated by the electric current. That is not a fact at all. The presence of extremely minute traces of organic matter in an ordinary electric bath will change the whole character of the copper. One-tenth of one per cent. of the weight of the sulphate of copper of ordinary gelatine added to the solution will render the deposit absolutely useless for all practical purposes, so that it is almost as brittle as glass. If the density of the current is kept up where it was, before the addition of the organic matter, great streaks of white material having a silvery lustre will manifest themselves on the electrodes. I do not think that anything has appeared in print in regard to this, but it certainly is a very interesting fact and I think that the peculiar change in the physical properties of the copper so deposited is due to the formation of a peculiar body which was observed by the eminent French chemist Wurtz a few years ago and which he called hydrite of copper. Therefore I have abandoned the copper voltameter. It requires considerable care not only in the preparation of electrodes to properly secure the accuracy to which Prof. Nichols has shown it is capable of being brought, but it also requires care in the selection of the salt. Silver has been so thoroughly tested as to show that with proper care it is capable of giving excellent results. In the case of silver, if the organic matter could be kept in solution, whether there would be any action or not, I do not know. I know it is true of nickel. The addition of an extremely small percentage of organic matter to a nickel solution renders it incapable of yielding any satisfactory results. That was called to my attention by a case of this kind some years ago. Some nickel platers had a large tank of solution and those tanks were usually made of wood lined with asphalt. Occasionally they would leak, and to save their solution they generally emptied it into barrels. The parties in question put the solution into barrels which had been lined with gelatine. When they put their solution back again, after repairing the tank—it was in a day or so—the solution was ruined. No one could tell what it was due to. It was assumed from the general appearance that somebody had wilfully put something into it to destroy it. It showed the presence of a minute trace of organic matter. That, however, did not throw very much light on the subject, because nickel solutions in those days were frequently used with anodes of muslin which had not been washed, but upon very careful inquiry I found that they had been using these barrels lined with gelatine, I therefore made an experiment by the addition of gelatine to the nickel solution and found exactly the same results. In nickel it is comparatively easy to get rid of it; in copper it is very difficult. In nickel a certain addition of permanganate of potash will enable you to oxidize the organic matter and get rid of it. Therefore in using the copper voltameter it is not only necessary to consider the form of the electrodes, but also the purity of the solution. What other substances, such as albumen, would do, I don't know. Gelatine has a very marked effect. The silver voltameter of course, will not retain organic matter if used in the ordinary way. If a silver voltameter is used with care there is not any difficulty from the particles floating away. To avoid that, it is well to take a little freshly precipated oxide of silver which has been carefully washed, and add it to the ordinary nitrate of silver of commerce and you will immediately get your solution into a condition where it yields you quite a nice deposit. It is not very close grained, but it does not show the disposition to float away in small particles. By saturating it with a little oxide of silver, you get rid of that trouble, and it seems to me by far the most reliable voltameter.

THE PRESIDENT:—I am sure that we are very much gratified to hear from Mr. Weston on this subject. It is a subject, I know, to which he has given a very large amount of attention. I do not think there is any one more capable of speaking on deposition generally, both from its practical standpoint and from its use in calibration.

MR. MAILLOUX:—I have had considerable experience with the deposition of copper in a practical way, to the extent of depositing it in very large quantities and at fair rates, and sometimes at very excessive rates, several times faster than is supposed to be possible, and sometimes at very slow rates. I have often had occasion to notice the discrepancy between the theoretical rate of deposit corresponding to a given amount of current, and the practical results; and where the rate of current is excessive, that this difference would be very great. I think, as the result of my observation and experience, that it is possible to get very accurate results with the copper voltameter, but in order to do so I think we should restrict ourselves to certain conditions or take them into consideration. In the first place, I think that the copper used should be absolutely pure. Now, I have found repeatedly in depositing copper industrially for electrotyping or electroplating, that the physical properties of the deposit, its ductility, etc., greatly depended on the character of the metal used as the anode. You take for instance, copper which is itself the result of electrical deposition in electrical refineries, and from that you deposit copper and compare it with an old copper anode such as are supplied to electrotypers, and there is a very marked difference in the character of the deposit, and a considerable variation in the amount of the copper deposited. I think that it was John T. Sprague, of England, who first pointed out the fact that in depositing solutions there is always a tendency to deposit hydrogen, and it has been my experience that in depositing copper it is only under certain conditions that you are entirely free from depositing a certain amount of hydrogen with the copper. You can only avoid that by having a solution which is sufficiently dense for the amount of current and of proper temperature, and sufficient acidity, and of course, circulated. The circulation is probably one of the most important conditions that determine the theoretical accuracy of the voltameter, and that is perhaps the reason why Mr. Ryan's method gives better results than the other. Under these circumstances it is possible to deposit copper without depositing hydrogen. If hydrogen is evolved it may unite with the copper, but you do not get the weight that you would if it were all copper. That is probably where the main error arises. If the anode and solution are pure, I have found, as Mr. Weston states, that the purity of water has much to do with the accuracy of result; and you must use pure sulphate of copper and pay strict attention to the conditions. Under those circumstances, if the current density is low, the solution not too cold and properly agitated, I have no doubt at all that it can be made as accurate as any other voltametric method.

MR. WESTON:—There is one little remark I would like to make in regard to the addition of acid. Some years ago it used to be customary to use a neutral solution. It was thought more satisfactory, because under those conditions the two electrodes were more uniform. Of late years it has been customary to add a small amount of free acid. and that comes right in line with the remark I made a little while ago as to the probable formation of compounds of copper. In electroplating you invariably find that double salts are used—double sulphate of nickel and ammonia for nickel plating or double chloride; and for silver it is double cyanide of silver and potassium. It was supposed to be due there to the fact that cyanide of silver is insoluble, but that is not entirely the reason for it. The truth is that the nearer you come to a double salt the freer you seem to be from these secondary compounds or bodies precipitated. I think that is a fixed fact. Sulphate of nickel, and sulphate of iron, and those metals that are highly electropositive are readily decomposed by the current at first, but if they are not combined with some other salts they reach a certain condition in which the solution won't give any more metal. They will give nothing but sub-salts. I have seen deposits from nickel baths, certainly one-eighth of an inch, which were nothing else but sub-salts, and that condition is not brought about immediately but gradually in the solutions. Now, with the electro-negative metals, such as gold and silver, that does not seem to be so marked. In silver we cannot get any indication of it at all in the nitrate, but the metal there does not go down in a condition that suits the practical plater. The copper voltameter, of course, has a decided advantage when we come to measure heavy currents. In nickel salts, iron salts and zinc salts, when the solution is a solution of normally pure salts, the metal will go down, but we all know that these normal salts can be converted into salts that give no acid reaction. By the addition of a certain amount of oxide they form basic salts. Oxide of nickel and oxide of zinc and oxide of iron, freshly precipitated, are all soluble in the normal salts. An addition, therefore, of a very minute trace of oxide to the solutions, so as to make them basic in their character, immediately renders the solution incapable of giving satisfactory results. That condition will be brought about—notwithstanding the fact that you start with a normal salt—particularly where the anode is larger than the cathode. In zinc salts, even when the anode and cathode have the same surfaces, it is usually brought about. Care should be taken in the copper voltameter to obtain a considerable excess of free acid.

Mr. Mailloux:—The currents I measured were seldom below 100 and mostly up to 300 amperes, and I do not think silver will be very useful there.

Dr. Moses:—In corroboration of several statements that were made I would recall the fact that the presence of nitrogen in the cyanide compound has some bearing on changing the physical condition of the copper. There is a very remarkable difference between copper deposited from a cyanide solution and copper deposited from a sulphate of copper solution. They look like two distinct metals. The statement of Mr. Weston is highly interesting and ought to have some bearing on the construction of voltameters. With regard to the discrepancies and difficulties that occur in the use of the sulphate of copper, from the very long and interesting experiments that Prof. Nichols made some ten years ago, I think he ought to be thoroughly satisfied that sulphate of copper is not the right thing.

Mr. E. T. Birdsall:—I am sure every one who has patience to use the copper voltameter will heartily thank Mr. Ryan if he has made their labors easy. I also think it is possible that this form of the helix of copper wire, if it is as good as the figures appear to indicate, will be a valuable hint to those companies that use electrolytic meters. All electrolytic meters that I know of at present use plates which are somewhat in the form of the old copper voltameter, and although these meters are said to be accurate, yet if they are changed into the form of a helix of copper wire in place of the plates, we shall have perfection perfected.

PROF. NICHOLS:—I would like to say that Mr. Ryan's results have been corroborated throughout the year by student after student. We have no more trouble with the copper voltameter since the adoption of this method. Formerly, as I stated, we expected errors of 1, 2, 3 or 4 per cent., according to the skill of the man, but now we get errors of $\frac{1}{10}$, $\frac{2}{10}$ and $\frac{3}{10}$ per cent. If a man gets to $\frac{3}{10}$ per cent. we tell him something is wrong.

THE PRESIDENT:—The next paper is on "The Personal Error in Photometry," by Prof. Nichols. Prof. Cross's paper on the Measurements of Telephonic Currents will not be read at this meeting, as the Professor is unable to be present.

A Paper read before the American Institute of Electrical Engineers, New York, May 22nd, 1889.

THE PERSONAL ERROR IN PHOTOMETRY.

BY EDWARD L. NICHOLS.

Whenever, in the course of photometric work, different observers have occasion to compare the same pair of lamps, it will be found that the results of their readings with the Bunsen photometer differ by an amount very much larger than the apparent mean error of a single observation.

Ordinarily, these differences are cloaked by the accidental errors due to the fluctuations of the lights under observation, and they have, consequently, not received the attention which they merit. There has been, indeed, so far as I know, no systematic attempt to determine the precise nature and importance of this personal error, the existence of which has doubtless been recognized by very many observers.

The introduction of the storage battery and the adoption of the incandescent lamp, supplied from the constant source thus afforded, as a secondary standard, has, however, entirely removed the large accidental errors resulting from uncontrollable fluctuations in our standard of illumination, and the personal errors just alluded to accordingly stand out in their true importance. I have very recently had occasion to make some experiments upon this subject, the results of which may be of some interest to those who have occasion to work with the Bunsen photometer.

The character of the errors with which one meets continually, whenever the reading of two or more observers are brought into direct comparison, may be very well shown by means of two sets of readings made by Mr. B. W. Snow and myself, upon the same pair of lamps. These observations were made under precisely similar and uncommonly favorable conditions. The two lamps in question were of the same type, and being the only lamps in circuit with a large storage battery, they could be maintained at an almost absolutely constant voltage. These two sets of readings are not presented here as an example of what may be done with the Bunsen photometer in the comparison of constant sources of light. They are indeed of no great accuracy, and contain larger accidental errors than some observations to which I shall have occasion to call attention presently. They exhibit, however, very clearly the existence of the personal errors of observation which are to form the subject of this paper.

TABLE I.

Two parallel sets of 10 readings upon the candle power of an incandescent lamp.

(1) (B. W. S. observing.)			(2) (E. L. N. observing.)		
Readings.	Differences.		Readings.	Differences.	
C. P.	C. P.	PER CENT.	C. P.	C. P.	PER CENT.
12.12	.112—	.91	12.56	.044—	.35
12.32	.088—	.72	12.60	.084—	.67
12.16	.072—	.59	12.36	.156—	1.25
12.16	.072—	.59	12.56	.044—	.35
12.28	.048—	.39	12.64	.124—	.99
12.40	.168—	1.38	12.60	.048—	.67
12.28	.048—	.39	12.56	.044—	.35
12.08	.152—	1.30	12.40	.116—	.93
12.12	.112—	.92	12 32	.196—	1.35
12.40	.168	1.24	12.56	.044—	.35
AVERAGES.					
12.232	.104	.852	12.516	.089	.717

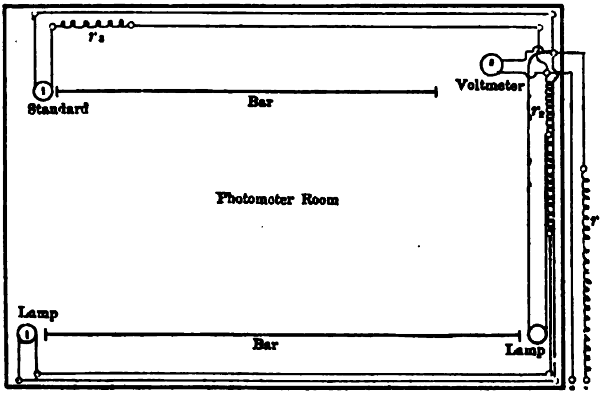
It will be seen from the above table that while the apparent mean error of a single observation is only about one-tenth of a candle, and the probable error of each set only .022 candle, the two averages themselves differ by .284 candle.

The following experiments were made for the purpose of determining the nature of this personal error:—

Three incandescent lamps of the same type and voltage were selected, care being taken to choose specimens, the carbons of which were as straight as possible. These lamps gave 16 candles at 110 volts. They were connected in multiple to the terminals of a storage battery giving a potential difference of 120 volts. Adjustable resistances of german silver wire were placed in the line leading to each lamp and in one of the mains leading from the battery. (See figure.)

The lamps, together with the above mentioned resistances, were set up in a photometer room which contained two Bunsen photometers. The connecting wires were of such length that the lamps could be moved from one photometer bar to the other without breaking connections. One of the lamps was set up as a comparison standard at the end of the shorter bar. Each of the other lamps was then compared with this standard, the resistances being adjusted until the intensities of these lamps were found to bear the same relation to the standard. In other words, they were brought to the same intensity by a method which eliminated the systematic personal errors which it was my purpose to study.

The two lamps were then placed at the ends of the longer photometer bar, the length of which was 400 centimeters. It was divided into 800 equal parts. The lamps had been measured with the plane of the filament at right angles with the photometer bar, and care was taken in setting them up in their new position, to place them at the same angle and presenting the same face to the bar as before.



A closed circuit voltmeter between the terminals of the battery enabled the observer at the beginning of a set of readings to bring the lamps to the proper potential. In order to insure greater constancy in the degree of incandescence throughout the series of tests, the lamps were maintained at a potential considerably below the normal, their candle-power being about 12 instead of 16 candles.

A number of observers, all of whom were accustomed to photometric measurements, and some of whom had had extended experience, were asked to make a set of 10 readings each, upon the two lamps. The result of these two readings are given in table II.

TABLE II.

Ratio of two incandescent lamps, previously adjusted to equal brightness. The measurements are made in the usual manner, by means of the Bunsen photometer.

I_r and I_l are the intensities of the right-hand and left-hand lamps respectively.

Observer.	Ratio—$\frac{I_r}{I_l}$	Personal error.
A	1.0590 ± .0040	— .0558
B	0.9704 ± .0044	+ .0331
C	1.0021 ± .0022	— .0189
D	1.0191 ± .0072	— .0159
E	1.0182 ± .0039	— .0150
F	1.0902 ± .0057	— .0870
G	1.0733 ± .0053	— .0701
H	1.0293 ± .0042	— .0261
I	1.0297 ± .0050	— .0263
J	1.0220 ± .0027	— .0188

The true value of the ratio, $\frac{I_r}{I_l}$ determined previously by comparing each lamp separately with the standard, was 1.0032 ± .0015, which value was used in computing the personal error.

It will be seen from inspection of the table that none of the averages fall at the middle of the bar, nor are they distributed around it in such a manner that the most probable value of the entire series, calculated by least squares under the assumption that only fortuitous errors exist, approximates to unity as it should do, were the lamps of equal brightness and were there no systematic errors to vitiate the result. On the other hand the readings lie, with a single exception, on one side, namely to the left-hand of the centre of the photometer bar. It will be seen, moreover, that the probable error calculated for each set separately, without taking cognizance of any systematic error, is very small in comparison with the differences between the mean results of the various sets, and especially in comparison with the variations of those averages from unity.

The Bunsen disc with which these observations were made, was mounted in the usual manner, the two sides of the disc being viewed simultaneously by means of two plane mirrors set at the proper angle behind the former. It was noted that the almost universal habit in reading was to use the two eyes independently, one eye fixed on each side of the disc. It seemed probable, therefore, that the personal error arose from the unequal sensitiveness of the observer's eyes, in that he would unconsciously set the disc at too great a distance from the lamp which illuminated the side of the disc which was being observed by his more sensitive eye. It appears, if we accept this explanation, that the right eye was the more sensitive in all the cases under observation, excepting one, that of the writer (see table II. B), whose readings would indicate the opposite peculiarity.

In order to reverse the relation of the observer to the photometer bar, without altering any of the other conditions of measurement, a large mirror was set up opposite the photometer at a distance of about 40 centimeters. The observer by sitting with his back to the bar, could then see the images of the Bunsen disc in the mirror, and his right and left eyes were reversed with reference to the lamps. Sets of 10 readings were made in this manner by some of the same observers as before, the object being to test the hypothesis just stated. The results thus obtained were, however, complicated by the unforeseen circumstance that the method of using the eyes in observation was not the same when the image of the disc was seen in the mirror as when it was viewed directly. The observer, when using the mirror, no longer used his eyes independently, but scanned the image as a whole, so that both eyes had a share in determining the brightness of each side of the disc. The result was to produce a change in his settings, not by introducing a systematic error, equal and opposite to that occuring in the first series of observations, as had been expected, but by eliminating the error in question. In a word, the settings with the mirror were in better agreement than those made by direct observation, and they gave results much more nearly in accordance with the known equality of the lamps.

In my own case, however, an exception to the above statement must be noted, very possibly as the result of an effort to repeat with the mirror the precise method of observation followed in making the direct settings. I continued to use my eyes independently, viewing the image of one side of the disc, with the right eye, the other with the left. There resulted a set of readings, such as I had expected to obtain from all the observers, the mean of which lay as far from the centre of the photometer bar as the mean of the direct readings, but upon the other side. (Compare sets "B," table II. and III.)

The results obtained with the mirror are given in the following table:—

Table III.

Ratio of the two lamps determined by observing the images of the Bunsen disc in a large mirror; the observer placed with his back to the photometer bar:

Observer.	Ratio.. $\frac{I_r}{I_l}$	Personal error.
A	1.0070 ± .0026	.0023
B	1.0444 ± .0011	.0414
C	0.9964 ± .0053	.0068
F	1.0141 ± .0025	.0109
G	1.0004 ± .0056	.0023
H	1.0134 ± .0045	.0102
I	1.0004 ± .0031	.0023

The observations with the mirror were followed by four sets of direct readings made with one eye bandaged. It was found that although more fatiguing, these monocular settings were made with a feeling of certanity on the part of the observer which had not accompanied the settings made with both eyes. In the latter case, indeed, a conflict between tendencies to set the disc in two distinct positions had been very apparent, the observer unconsciously choosing now one, now the other, as the true positions. The set of readings given in full in table IV., will serve to illustrate the result of this tendency. The instances in which the observer seems thus to have temporarily changed in his judgment of the true setting, are printed in heavy type.

Table IV.

No.	Reading.	No.	Reading.
1	1.033	6	**1.048**
2	1.022	7	**1.053**
3	1.021	8	1.023
4	1.023	9	1.033
5	**1.050**	10	1.030

This tendency to vacillation disappeared almost entirely when only one eye was used. The readings obtained with a single eye not only agreed much better among themselves than those made under like conditions, using both eyes, but the results obtained with the left eye were identical with those obtained with the right eye, and both seemed to be entirely free from the systematic personal error that had been found to vitiate readings made in the usual way.

The results of such a set of monocular readings, made by Mr. Snow and myself, all other conditions remaining the same as in previous trials, are given in table V.

It will be seen that the probable error of each set is much smaller than in the sets of observations made with both eyes; also that the readings of two observers whose mean results from similar sets with both eyes had differed by 8 per cent. (see sets "A" and "B," table II.), are in complete agreement.

Finally, it will be noted that the results of these four monocular series differ from the accepted value of the ratio of intensities of the two lamps (1.0032) by an amount less than the probable error of each set.

TABLE V.

Observer.	Eye used.	Ratio.	Personal error.
B. W. S.	(Right.)	1.0028 ± .0019	.0004
B. W. S.	(Left.)	1.0001 ± .0019	.0031
E. L. N.	(Right.)	1.0001 ± .0017	.0031
E. L. N.	(Left.)	1.0031 ± .0018	.0001

It had been my intention to extend the experiments described in this paper to a much larger number of observers. The evidence obtained, however, seemed quite sufficient to establish the existence of the personal error in photometry, and to show that it was in general very far from being a negligible quantity. Frequent repetitions showed, that in my own case at least, the error was very nearly constant; and I think that it would be quite possible to establish a personal equation, and to apply the proper correction.

A much better plan, however, would be to so modify the photometer car itself as to insure the use of both eyes in the inspection of each side of the illuminated disc. Several forms of photometer, already in use, partially fulfill the necessary requirement. Any device which would bring the images of the opposite faces of the disc into a vertical line in the field of view would doubtless serve to eliminate the error in question. Observations with such a photometer would correspond in character to the "monocular" readings given in table V. Not only would they be free from the systematic error to which the ordinary form of Bunsen photometer is subject, but the accidental errors would be much smaller, and the degree of uncertainty which attends the determination of candle-power by our present methods would be in great measure diminished.

Physical Laboratory of Cornell
University, May 17, 1889.

A lecture before the American Institute of Electrical Engineers, at the College of the City of New York, May 22, 1889.

ON MODERN VIEWS WITH RESPECT TO ELECTRIC CURRENTS.

BY PROF. HENRY A. ROWLAND.

As, a short time since, I stood in a library of scientific books and glanced around me at the works of the great masters in physics, my mind wandered back to the time when the apparatus for a complete course of lectures on the subject of electricity consisted of a piece of amber and a few light bodies to be attracted by it. From that time until now, when we stand in a magnificent laboratory with elaborate and costly apparatus in great part devoted to its study, how greatly has the world changed and how our science of electricity has expanded both in theory and practice until, in the one case, it threatens to include within itself nearly the whole of physics, and in the other to make this the age of electricity.

Were I to trace the history of the views of physicists with respect to electric currents it would include the whole history of electricity. The date when the conception of an electric current was possible was when Stephen Gray, about 170 years ago, first divided bodies into conductors and non-conductors, and showed that the first possessed the property of transmitting electrical attractions to a distance. But it was only when the Leyden jar was discovered that the idea of a current became very definite. The notion that electricity was a subtle fluid which could flow along metal wires as water flows along a tube, was then prevalent, and, indeed, remains in force to-day among all except the leaders in scientific thought. It is not my intention to depreciate this notion, which has served and still serves a very important purpose in science. But, for many years, it has been recognized that it includes only a very small portion of the truth and that the mechanism by which energy is transmitted from one point of space to another by means of an electric current is a very complicated one.

Here, for instance, on the table before me are two rubber tubes filled with water, in one of which the water is in motion, in the other at rest. It is impossible, by any means now known to us, to find out, without moving the tubes, which one has the current of water flowing in it and which has the water at rest. Again, I have here two wires, alike in all respects, except that one has a current of electricity flowing in it and the other has not. But in this case, I have only to bring a magnetic needle near the two to find out in which one the current is flowing. On our ordinary sense the passage of the current has little effect; the air around it does not turn green or the wire change in appearance. But we only have to change our medium from air to one containing magnetic particles to perceive the commotion which the presence of a current may cause. Thus this other wire passes through the air near a large number of small suspended magnets, and, as I pass the current through it, every magnet is affected and tends to turn at right angles to the wire and even to move toward it and wrap itself around it. If we suppose the number of these magnets to become very great and their size small, or if we imagine a medium, every atom of which is a magnet, we see that no wire carrying a current of electricity can pass through it without creating the greatest commotion. Possibly this is a feeble picture of what takes place in a mass of iron near an electric current.

Again, coil the wire around a piece of glass, or indeed, almost any transparent substance, and pass a strong current through the wire. With our naked eye alone we can see no effect whatever, as the glass is apparently unaltered by the presence of the current; but, examined in the proper way, by means of polarized light, we see that the structure of the glass has been altered throughout in a manner which can only be explained by the rotation of something within the glass many millions of times every second.

Once more, bring a wire in which no current exists nearer and nearer to the one carrying the current, and we shall find that its motion in such a neighborhood causes or tends to cause an electric current in it. Or, if we move a large solid mass of metal in the neighborhood of such a current we find a peculiar resistance unfelt before, and if we force it into motion we shall perceive that it becomes warmer and warmer as if there was great friction in moving the metal through space.

Thus, by these tests, we find that the region around an electric current has very peculiar properties which it did not have before, and which, although stronger in the neighborhood of the current, still extend to indefinite distances in all directions, becoming weaker as the distances increase.

How great then the difference between a current of water and a current of electricity. The action of the former is confined to the interior of the tube, while that of the latter extends to great distances on all sides, the whole of space being agitated by the formation of an electric current in any part. To show this agitation, I have here two large frames with coils of wire around them. They hang face to face about 6 feet apart. Through one I discharge this Leyden jar, and immediately you see a spark at a break in the wire of the other coil, and yet there is no apparent connection between the two. I can carry the coils 50 feet or more apart, and, yet, by suitable means I can observe the disturbances due to the current in the first coil.

The question is forced upon us as to how this action takes place. How is it possible to transmit so much power to such a distance across apparently unoccupied space? According to our modern theories of physics there must be some medium engaged in this transmission. We know that it is not the air, because the same effects take place in a vacuum, and, therefore, we must fall back on that medium which transmits light and which we have named the ether. That medium which is supposed to extend unaltered throughout the whole of space, whose existence is very certain but whose properties we have yet but vaguely conceived.

I cannot, in the course of one short hour, give even an idea of the process by which the minds of physicists have been led to this conclusion, or the means by which we have finally completely identified the ether which transmits light with the medium which transmits electrical and magnetic disturbances. The great genius who first identified the two is Maxwell, whose electro-magnetic theory of light is the centre around which much scientific thought is to-day revolving, and which we regard as one of the greatest steps by which we advance nearer to the understanding of matter and its laws. It is this great discovery of Maxwell which allows me, at the present time, to attempt to explain to you the wonderful events which happen everywhere in space when one establishes an electric current in any other portion.

In the first place, we discover that the disturbance does not take place in all portions of space at once, but proceeds outwards from the centre of the disturbance with a velocity exactly equal to the velocity of light. So that, when I touch these wires together so as to complete the circuit of yonder battery, I start a wave of ethereal disturbance which passes outwards with a velocity of 185,000 miles per second, thus reaching the sun in about eight minutes, and continue to pass onwards forever or until it reaches the bounds of the universe. And, yet, none of our senses inform us of what has taken place unless we sharpen them by the use of suitable instruments. Thus, in the case of these two coils of wire, suspended near each other, which we have already used, when the wave from the primary disturbance reaches the second coil, we perceive the disturbance by means of the spark formed at the break in the coil. Should I move the coils further apart, the spark in the second coil would be somewhat delayed, but the distance of 185,000 miles would be necessary before this delay could amount to as much as one second. Hence the effects we observe on the earth take place so nearly instantaneously that the interval of time is very difficult to measure, amounting, in the present case, to only $\frac{1}{150,000,000}$ of a second.

It is impossible for me to prove the existence of this interval, but I can at least show you that waves have something to do with the action here observed. For instance, I have here two tuning forks mounted on sounding boxes and tuned to exact unison. I sound one and then stop its vibrations with my hand, instantly you hear that the other is in vibration, caused by the waves of sound in the air between the two. When, however, I destroy the unison by fixing this piece of wax on one of the forks, the action ceases.

Now, this combination of a coil of wire and a Leyden jar is a vibrating system for electricity and its time of vibration is about 10,000,000 times a second. This second system is the same as the first, and therefore its time of vibration is the same. You see how well the experiment works now because the two are in unison. But let me take away this second Leyden jar, thus destroying the unison, and you see that the sparks instantly cease. Replacing it, the sparks reappear. Adding another on one side and they disappear again, only to reappear when the system is made symmetrical by placing two on each side.

This experiment and that of the tuning forks have an exact analogy to one another. In each we have two vibrating systems connected by a medium capable of transmitting vibrations, and they both come under the head of what we know as sympathetic vibrations. In the one case, we have two mechanical tuning forks connected by the air; in the other, two pieces of apparatus which we might call electrical tuning forks, connected by the luminiferous ether. The vibrations in one case can be seen by the eye or heard by the ear, but in the other case they can only be perceived when we destroy them by making them produce a spark. The fact that we are able to increase the effect by proper tuning demonstrates that vibrations are concerned in the phenomenon. This can, however, be separately demonstrated by examining the spark by means of a revolving mirror, when we find that it is made up of many successive sparks corresponding to the successive backward and forward movements of the current.

The fact of the oscillatory character of the Leyden jar discharge was first demonstrated by our own countryman, Henry, in 1832, but he pursued the subject only a short distance, and it remained for Sir William Thomson to give the mathematical theory and prove the laws according to which the phenomenon takes place.

Thus, in the case of a charged Leyden jar whose inner and outer coatings have been suddenly joined by a wire, the electricity flows back and forth along the wire until all the energy originally stored up in the jar has expended itself in heating the wire or the air where the spark takes place and in generating waves of disturbance in the ether which move outward into space with the velocity of light. These ethereal waves we have demonstrated by letting them fall on this coil of wire and causing the electrical disturbance to manifest itself by electric sparks.

I have here another more powerful arrangement for producing electro-magnetic waves of very long wave length, each one being about 500 miles long. It consists of a coil, within which is a bundle of iron wires. On passing a powerful alternating current through the coil, the iron wires are rapidly magnetized and demagnetized and send forth into space a system of electromagnetic waves at the rate of 360 in a second.

Here, also, I have another piece of apparatus [a lamp] for sending out the same kind of electro-magnetic waves; on applying a match, we start it into action. But the last apparatus is tuned to so high a pitch that the waves are only $\frac{1}{50,000}$ inch long, and 55,000,000,000,000 are given out in one second. These short waves are known by the name of light and radiant heat, though the name radiation is more exact. Placing any body near the lamp so that the radiation can fall on it, we observe that when the body absorbs the rays it is heated by them; the well-known property of so-called radiant heat and light. Is it not possible for us to get some substance to absorb the *long* waves of disturbance, and so obtain a heating effect? I have here such a substance in the shape of a sheet of copper, which I fasten on the face of a thermopile, and I hold it where the waves are the strongest [near the coil while the alternating current is passing through it]. As I have anticipated, great heat is generated by their absorption, and soon the plate of copper becomes very warm, as we see by this thermometer, by feeling it with the hand or even by the steam from water thrown upon it. In this experiment the copper has not touched the coil or the iron wire core, although if it did they are very much cooler than itself. The heat has been produced by the absorption of the waves in the same way as a blackened body absorbs the rays of shorter wave length from the lamp; and, in both cases, heat is the result.[1]

But in this experiment, as in the first one, the wave-like nature of the disturbance has not been proved experimentally. We have caused electric sparks, and have heated the copper plate across an interval of space, but have not in either of these cases proved experimentally the progressive nature of the disturbance; for a ready means of experimenting on the waves, obtaining their wave length and showing their interferences, has hitherto been wanting. This deficiency has been recently overcome by Prof. Hertz, of Carlsruhe, who has made a study of the action of the coil, and has shown us how to use it for experiments on the ethereal waves, whose existence had before been made certain by the mathematics of Maxwell.

I scarcely know how to present this subject to a non-technical audience and make it clear how a coil of wire with a break in it can be used to measure the velocity and wave length of ethereal waves. However, I can but try. If the waves moved very slowly, we could readily measure the time the first coil took to affect the second, and show that this time was longer as the distance was greater. But it is absolutely inappreciable by any of our instruments, and another method must be found. To obtain the

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[1] The thermopile was connected with a delicate mirror galvanometer, the deflections of which were shown on a screen.

wave length Prof. Hertz used several methods, but that by the formation of stationary waves is the most easily grasped. Mr. Ames holds in his hand one end of a spiral spring, which makes a very heavy and flexible rope. As he sends a wave down it, you see that it is reflected at the further end, and returns again to his hand. If, however, he sends a succession of waves down the rope, the reflected waves interfere with the direct ones, and divide the rope into a succession of nodes and loops, which you now observe. So a series of sound waves, striking on a wall, form a system of stationary waves in front of the wall. With this in view, Prof. Hertz established his apparatus in front of a reflecting wall, and observed the nodes and loops by the sparks produced in a ring of wire. It is impossible for me to repeat this experiment before you, as it is a very delicate one, and the sparks produced are almost microscopic. Indeed, I should have to erect an entirely different apparatus, as the waves from the one before me are nearly $\frac{1}{4}$ mile long, the time of vibration of the system being very great, that is, $\frac{1}{10,000,000}$ of a second. To produce shorter waves we must use apparatus tuned, as it were, to a higher pitch, in which the same principle is, however, employed, but the ethereal waves are shorter, and thus several stationary waves can be contained in one room.

The testing coil is then moved to different portions of the room, and the nodes are indicated by the disappearance of the sparks, and the loops by the greater brightness of them. The presence of stationary waves is thus proved, and their half wave length found from the distance from node to node, for stationary waves can always be considered as produced by the interference of two progressive waves advancing in opposite directions.

However interesting a further description of Professor Hertz's experiments may be, we have gone as far in that direction as our subject carries us; for we have demonstrated that the production of a current in a wire is accompanied by a disturbance in the surrounding space; and, although I have not experimentally demonstrated the ethereal waves, yet I have proved the existence of electric oscillations in the coils of wire and the ether surrounding it.

Our mathematics has demonstrated, and experiments like those of Professor Hertz have confirmed the demonstration, that the wave disturbance in the ether is an actual fact.

The closing of a battery circuit, then, and the establishment of a current of electricity in a wire is a very different process from the formation of a current of water in a pipe, though, after the first shock, the laws of the flow of the two are very much alike. But even then, the medium around the current of electricity has very strange properties, showing that it is accompanied by a disturbance throughout space. The wire is but the core of the disturbance, which latter extends indefinitely in all directions.

One of the strangest things about it is that we can calculate with perfect exactness the velocity of the wave propagation and the amount of the disturbance at every point and at any instant of time; but as yet we cannot conceive of the details of the mechanism which is concerned in the propagation of an electric current. In this respect our subject resembles all other branches of physics in the partial knowledge we have of it. We know that light is the undulation of the luminiferous ether, and yet the constitution of the latter is unknown. We know that the atoms of matter can vibrate with purer tones than the most perfect piano, and yet we cannot even conceive of their constitution. We know that the sun attracts the planets with a force whose law is known, and yet we fail to picture to ourselves the process by which it takes our earth within its grasp at the distance of many millions of miles and prevents it from departing forever from its life-giving rays. Science is full of this half knowledge, and the proper attitude of the mind is one of resignation toward that which it is impossible for us to know at present and of earnest striving to help in the advance of our science, which shall finally allow us to answer all these questions.

The electric current is an unsolved mystery, but we have made a very great advance in understanding it when we know that we must look outside of the wire at the disturbance in the medium before we can understand it: a view which Faraday dimly held fifty years ago, which was given in detail in the great work of Maxwell, published sixteen years since, and has been the guide to most of the work done in electricity for a very long time. A view which has wrought the greatest changes in the ideas which we have conceived with respect to all electrical phenomena.

So far, we have considered the case of alternating electric current in a wire connecting the inner and outer coatings of a Leyden jar. The invention of the telephone, by which sound is carried from one point to another by means of electrical waves, has forced into. prominence the subject of these waves. Furthermore, the use of alternating currents for electric lighting brings into play the same phenomenon. Here, again, the difference between a current of water and a current of electricity is very marked. A sound wave, traversing the water in the tube, produces a to and fro current of water at any given point. So, in the electrical vibration along a wire, the electricity moves to and fro along it in a manner somewhat similar to the water but with this difference:—the disturbance from the water motion is confined to the tube and the oscillation of the water is greatest in the centre of the tube, while, in the case of the electric current, the ether around the wire is disturbed and the oscillation of the current is greatest at the surface of the wire and least in its centre. The oscillations in the water take place in the tube without reference to the matter outside the tube, whereas the electric oscillations in the wire are entirely dependent on the surrounding space, and the velocity of the propagation is nearly independent of the nature of the wire, provided only that it is a good conductor.

We have then, in the case of electrical waves along a wire, a disturbance outside the wire and a current within it, and the equations of Maxwell allow us to calculate these with perfect accuracy and give all the laws with respect to them.

We thus find that the velocity of propagation of the waves along a wire, hung far away from other bodies and made of good conducting material, is that of light, or 185,000 miles per second; but when it is hung near any conducting matter, like the earth, or inclosed in a cable and sunk into the sea, the velocity becomes much less. When hung in space, away from other bodies, it forms, as it were, the core of a system of waves in the ether, the amplitude of the disturbance becoming less and less as we move away from the wire. But the most curious fact is that the electric current penetrates only a short distance into the wire, being mostly confined to the surface, especially where the number of oscillations per second is very great.

The electrical waves at the surface of a conductor are thus, in some respects, very similar to the waves on the surface of the water. The greatest motion in the latter case is at the surface while it diminishes as we pass downward and soon becomes inappreciable. Furthermore, the depth to which the disturbance penetrates into the water increases with increase of the length of the wave, being confined to very near the surface for very short waves. So the disturbance in the copper penetrates deeper as the waves and the time of oscillation are longer, and the disturbance is more nearly confined to the surface as the waves become shorter. I have recently made the complete calculations with respect to these waves, and have drawn some diagrams to illustrate the penetration of the alternating current into metal cylinders. The first diagram represents the current at different depths in a copper cylinder, 45 cm. diameter, or an iron one 14⅛ cm. diameter, traversed by an alternating current with 200 reversals per second. The first and second curves show us the current at two different instants of time, and show us how the phase

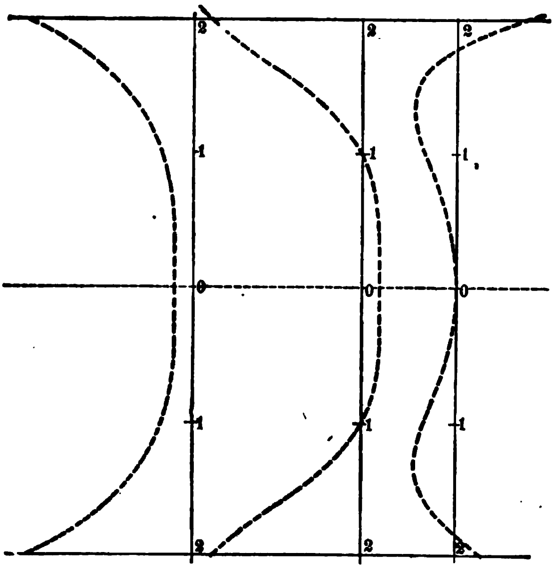


DIAGRAM 1.

changes as we pass downward into the cylinder. By reference to the third curve we see that it may be even in the opposite direction in the centre of the cylinder from what it is at the surface. The third curve gives us the amplitude of the current oscillations at different depths, irrespective of the phase, and it shows us that the current at the centre is only about 10 per cent. of that at the surface in this case. The second diagram shows us the distribution in the same cylinders when the number of reversals of the current is increased to 1,800 per second. Here we see that the disturbance is almost entirely confined to the surface, for at a depth of only 7 mm., the disturbance almost entirely vanishes.

There are very many practical applications of these theoretical results for electric currents. The most obvious one is to the case of conductors for the alternating currents used in producing the electric light. We find that when these are larger than about half an inch diameter they should be replaced by a number of conductors less than half an inch diameter, or by strips about a quarter of an inch thick, and of any convenient width. But this is a matter to be attended to by the electric light companies.

Prof. Oliver J. Lodge has recently, in the British Association, drawn attention to the application of these results to lightning rods. Almost since the time of Franklin, there have been those who advocated the making of lightning rods hollow, to increase the surface for a given amount of copper. We now know that these persons had no reason for their belief, as they simply drew the inference from the fact that electricity at best is on the surface. Neither were the advocates of the solid rods quite correct, for they reasoned from the fact that electricity in a state of steady flow, occupies the whole area of the conductor equally. The true theory, we now know, indicates that neither party was entirely correct and that the surface is a very important factor in the case of a current of electricity so sudden as that from a lightning discharge. But increase of surface can best be obtained by multiplying the number of conductors, rather than making them flat or hollow; and, at the same time, Maxwell's principle of enclosing the building within a cage can be carried out. Theory indicates that the current penetrates only one-tenth the distance into iron that it does into copper. As the iron has seven times the resistance of copper, we

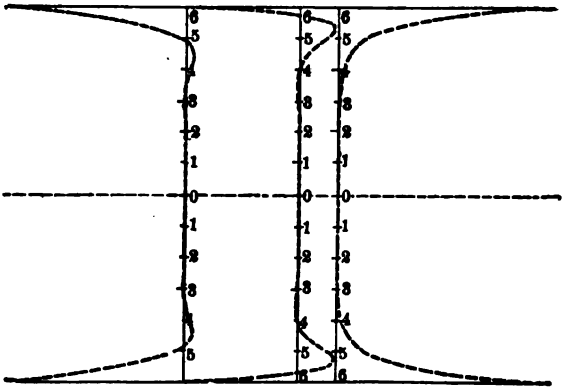


DIAGRAM 2.

should need 70 times the surface of iron that we should of copper. Hence I prefer copper wire about a quarter of an inch diameter and nailed directly to the house without insulators, and passing down the four corners, around the eaves and over the roof, for giving protection from lightning in all cases where a metal roof and metal down spouts do not accomplish the same purpose.

Whether the discharge of lightning is oscillatory or not, does not enter into the question, provided it is only sufficiently sudden. I have recently solved the mathematical problem of the electric oscillations along a perfectly conducting wire joining two infinite and perfectly conducting planes parallel to each other, and find that there is no definite time of oscillation, but that the system is capable of vibrating in any time in which it is originally started. The case of lightning between a cloud of limited extent and the earth along a path through the air of great resistance is a very different problem. Both the cloud and the path of the electricity are poor conductors, which tends to lengthen the time. If I were called on to estimate as nearly as possible what took place in a flash of lightning, I would say that I did not believe that the discharge was always oscillating, but more often consisted of one or more streams of electricity at intervals of a small fraction of a second, each one continuing for not less than $\frac{1}{100,000}$ second. An oscillating current with 100,000 reversals per second would penetrate about $\frac{1}{60}$ inch into copper and $\frac{1}{600}$ inch into iron. The depth for copper would constitute a considerable portion of a wire $\frac{1}{4}$ inch diameter; and, as there are other considerations to be taken into account, I believe it is scarcely worth while making tubes, or flat strips, for such small sizes.

It is almost impossible to draw proper conclusions from experiments on this subject in the laboratory such as those of Professor Oliver J. Lodge. The time of oscillation of the current in most pieces of laboratory apparatus is so very small, being often the $\frac{1}{100,000,000}$ of a second, that entirely wrong inferences may be drawn from them. As the size of the apparatus increases, the time of oscillation increases in the same proportion, and changes the whole aspect of the case. I have given $\frac{1}{100,000}$th of a second as the shortest time a lightning flash could probably occupy. I strongly suspect it is often much greater, and thus departs even further from the laboratory experiments of Professor Lodge, who has, however, done very much toward drawing attention to this matter and showing the importance of surface in this case. All shapes of the rod with equal surface are not, however, equally efficient. Thus, the inside surface of a tube does not count at all. Neither do the corrugations on a rod count for the full value of the surface they expose, for the current is not distributed uniformly over the surface; but I have recently proved that rapidly alternating currents are distributed over the surface of very good conductors in the same manner as electricity at rest would be distributed over them, so that the exterior angles and corners possess much more than their share of the current, and corrugations on the wire concentrate the current on the outer angles and diminish it in the hollows. Even a flat strip has more current on the edges than in the centre.

For these reasons, shape, as well as extent of surface, must be taken into account, and strips have not always an advantage over wires for quick discharges.

The fact that the lightning rod is not melted on being struck by lightning is not now considered as any proof that it has done its work properly. It must, as it were, seize upon the discharge, and offer it a easier passage to the earth than any other. Such sudden currents of electricity we have seen to obey very different laws from continuous ones, and their tendency to stick to a conductor and not fly off to other objects depends not only on having them of small resistance, but also on having what we call the self-induction as small as possible. This latter can be diminished by having the lightning rod spread sideways as much as possible, either by rolling it into strips, or better, by making a network of rods over the roof, with several connections to the earth at the corners, as I have before described.

Thus we see that the theory of lightning rods, which appeared so simple in the time of Franklin, is, to-day, a very complicated one, and requires for its solution a very complete knowledge of the dynamics of electric currents. In the light of our present knowledge the frequent failure of the old system of rods is no mystery, for I doubt if there are a hundred buildings in the country properly protected from lightning. With our modern advances, perfect protection might be guaranteed in all cases, if expense were no object.

So much for the rod itself, and now let us turn to other portions of the electrical system, for we have seen that, in any case, the conductor is only the core of a disturbance which extends to great distances on all sides. Were the clouds, the earth and the streak of heated air called the lightning flash all perfect conductors we could calculate the entire disturbance. It might then consist of a series of stationary waves between the two planes, extending indefinitely on all sides but with gradually decreasing amplitude as we pass away from the centre. The oscillation, once set up, would go on forever, as there would be no poor conductors to damp them. But when the clouds and the path of the lightning both have very great resistance, the energy is very soon converted into heat and the oscillations destroyed. I have given it as my opinion that this is generally the case and that the oscillations seldom take place, but I may be wrong, as there is little to guide me except guess work. If they take place, however, we have a ready explanation of what is sometimes called a back stroke of lightning. That is, a man at the other end of the cloud a mile or more distant from the lightning stroke sometimes receives a shock, or a new lightning flash may form at that point and kill him. This may be caused, according to our present theory, by the arrival of the waves of electrical disturbance which might themselves cause a slight shock or even overturn the equilibrium then existing and cause a new electric discharge.

We have now considered the case of oscillations of electricity in a few instances and can turn to that of steady currents. The closing of an electric current sends ethereal waves throughout space, but that after the first shock the current flows steadily without producing any more waves. However, the properties of the space around the wire have been permanently altered, as we have already seen. Let us now study these properties more in detail. I have before me a wire in which I can produce a powerful current of electricity and we have seen that the space around it has been so altered that a delicately suspended magnetic needle cannot remain quiet in all positions but stretches itself at right angles to the wire, the north pole tending to revolve around it in one direction and the south pole in the other. This is a very old experiment, but we now regard it as evidence that the properties of the space around the wire have been altered rather than that the wire acts on the magnet from a distance.

Put, now, a plate of glass around the wire, the latter being vertical and the former with its plane horizontal, and pass a powerful current through the wire. On now sprinkling iron filings on the plate, they arrange themselves in circles around the wire and thus point out to us the celebrated lines of magnetic force of

Faraday. Using two wires with currents in the same direction we get these other curves, and, testing the forces acting on the wire, we find that they are trying to move towards each other.

Again, pass the currents in the opposite directions and we get these other curves and the currents repel each other. If we assume that the lines of force are like rubber bands, which tend to shorten in the direction of their length and repel each other sideways, Faraday and Maxwell have shown that all magnetic attraction and repulsions are explained. The property which the presence of the electric current has conferred on the luminiferous ether is then one by which it tends to shorten in one direction and spread out in the other two directions.

We have thus done away with action at a distance, and have accounted for magnetic attraction by a change in the intervening medium as Faraday partly did almost fifty years ago. For this change in the surrounding medium is as much a part of the electric current as anything that goes on within the wire.

To illustrate this tension along the lines of force, I have constructed this model which represents the section of a coil of wire with a bar of iron within it. The rubber bands represent the lines of force which pass around the coil and through the iron bar, as they have an easier passage through the iron than the air. As we draw the bar down and let it go, you see that it is drawn upward and oscillates around its position of equilibrium until friction brings it to rest. Here, again, I have a coil of wire with an iron bar within it with one end resting on the floor. As we pass the current and the lines of magnetic force form around the coil and pass through the iron, it is lifted upwards although it weighs 24 pounds and oscillates around its position of equilibrium exactly the same as though it were sustained by rubber bands as in the model. The rubber bands in this case are invisible to our eye but our mental vision pictures them to us as lines of magnetic force in the luminiferous ether drawing the bar upward by their contractile force. This contractile force is no small quantity, as it may amount, in some cases, to one or even two hundred pounds to the square inch, and thus rivals the greatest pressure which we use in our steam engines.

Thus the luminiferous ether is, to-day, a much more important factor in science than the air we breathe. We are constantly surrounded by the two, and the presence of the air is manifest to us all; we feel it, we hear by its aid and we even see it, under favorable circumstances, and the velocity of its motion as well as the amount of moisture it carries, is a constant topic of conversation with mankind at large. The luminiferous ether, on the other hand, eludes all our senses and it is only with imagination, the eye of the mind, that its presence can be perceived. By its aid in conveying the vibrations we call light, we are enabled to see the world around us, and by its other motions which cause magnetism, the mariner steers his ship through the darkest night when the heavenly bodies are hid from view. When we speak in a telephone, the vibrations of the voice are carried forward to the distant point by waves in the luminiferous ether, there again to be resolved into the sound waves of the air. When we use the electric light to illuminate our streets, it is the luminiferous ether which conveys the energy along the wires as well as transmits it to our eye after it has assumed the form of light. We step upon an electric street car and feel it driven forward with the power of many horses, and again it is the luminiferous ether, whose immense force we have brought under our control and made to serve our purpose. No longer a feeble uncertain sort of medium, but a mighty power, extending throughout all space and binding the whole universe together, so that it becomes a living unit in which no one portion can be changed without ultimately involving every other portion.

To this, ladies and gentlemen, we have been led by the study of electrical phenomena, and the ideas which I have set forth constitute the most modern views held by physicists with respect to electric currents.

A paper read before the 36th meeting of the American Institute of Electrical Engineers, New York, June 25th, 1889, and discussion thereon.

SOME RECENT ELECTRICAL WORK ON THE ELEVATED RAILROADS AND ITS BEARING ON THE RAPID TRANSIT PROBLEM.

BY LEO DAFT.

It is particularly noticeable that all who have, so far, considered the question of rapid transit for the city of New York, have assumed that it is so deficient in these facilities that the question was of the most immediate and vital importance, and, while I do not for a moment wish to underestimate the urgency of the demand to meet the rapid growth which has already almost overtaxed the elevated system, we should not lose sight of the fact that New York has even to-day, all things considered, a better rapid transit system than any other city in the world. In evidence of this it is only necessary to cite the facts that one may enter a well-appointed car at the Battery, and, traveling through the best atmosphere of the city, be carried with remarkable smoothness to Fifty-ninth street in 26 minutes, or at the average speed of 12 miles per hour including stops, and in about this manner, the elevated system contrives to carry one-half million passengers per diem for a lower rate of fare than is done in any other city for similar accommodations, and with a smaller proportion of accidents than may be found on any steam road in the world making an equal number of stops per one hundred miles run. On a recent holiday, namely, April 30th last, 835,721 passengers were carried on this much abused system without noticeable detention or accident—but New York wants more, and in view of the rapidly growing traffic which has already almost exceeded the ability of the system, it must be admitted that the demand is a pressing one, as this one half million will grow to one million in a short time, and it is exceeding improbable that any steam motor can be devised to permit of this increase, at least on the present structures, without subjecting them to quickly destructive strains. With steam, therefore, the only avenue open for increasing the passenger capacity, namely the use of longer trains, is obviously out of the question, since it is a notorious fact that the present motors are taxed to their utmost.

To the mind of the mechanical engineer, having in view the ordinary coefficients of tractive ability, there is no remedy for this, but I think I shall be able to produce at least some little evidence that, owing to certain effects which have not yet been satisfactorily explained, an electric motor may be made capable of solving the problem at least so far as the ultimate strength of the present elevated structure will permit, inasmuch as the effects alluded to certainly do increase the tractive ability beyond that obtained by the use of any coefficients which may be found in the works of that eminent authority, D. K. Clark, or still later in the excellent treatise on the "Economic Theory of Railroad Location," by A M. Wellington.

That these coefficients are not absolutely trustworthy is evident not only in the hesitancy with which they are treated by these authors, but also from the fact that varying coefficients are to be found in use by practical men, where the conditions do not differ in any appreciable degree. This is noticeable to the extent of nearly twenty per cent—*vide* D. K. Clark and A. M. Wellington. I will not, therefore, make pretense to great precision on this point, as the matter is one yet under investigation, but to quote an extreme case I trust I may be pardoned for going back so far as the year 1882 to the first series of experiments in which this increased tractive capacity was observed and carried to its apparent maximum.

During a series of experiments with small motors, made at the works of the Daft Company, at Greenville, N. J., in the year above quoted, I constructed an adjustable incline upon which a small motor was caused to ascend with a gradually increasing gradient until the extraordinary angle of 2,900 feet per mile, or 54.9 per cent., was reached. Up to this point the motor, weighing 450 lbs., plus one man, weighing 150 lbs., total 600 lbs., was readily started from a position of absolute rest at the bottom, and ascended the gradient, being occasionally stopped midway and started again with perfect ease. When this final gradient was reached, however, the ascension was less easy and certain, though it was accomplished several times, but an attempt to exceed it resulting in failure this was regarded as the maximum under those circumstances.

Now let us for a moment consider what this actually indicated. A motor having a total weight of 600 lbs. will have a rolling resistance certainly equal to 10 lbs. per ton, making 2.25 lbs. for this item; at 54.9 per cent. the grade resistance will be 320.4, making a total of 331.65 lbs., or 55.27 per cent. of the total weight, considerably more than double the ultimate working limit of adhesion under the most favorable condition cited by Wellington in the work already quoted; but as these conditions, namely, a perfectly dry rail with drivers absolutely free from any lubricant, are so rarely found in practice as to be scarcely worth taking into account in every day work, it may fairly be considered as equal to three times the ultimate effort usually developed before slipping occurs.

A prominent engineer in dismissing the idea of increased traction due to the passage of a current between the adhesive surfaces, falls into the error of assuming that the effect would be precisely the reverse of that which has been observed, and in support of his argument cites the case of the lessened friction between the metallic points and the cylinder of chalk noted in a well-known instrument. I need hardly, however, call the attention of this audience to the entire absence of any proper analogy between these two cases. That the motor should not have ascended this gradient I am well aware—indeed, out of the commonest respect for such authorities as Messrs. Clark and Wellington, it ought to have stopped at the bottom of the gradient meekly rotating its wheels, but I was confronted by the indisputable fact that it did go up, and did it every day with perfect regularity for a number of days, before many gentlemen, among whom were mechanical engineers who could not account for that state of thing at all—and frankly, neither could I, but as a cold fact it outweighed all the arguments in the universe. In illustration of my own doubts on the subject I may mention a little instance that occurred at that time; I had written to a mechanical engineer of my acquaintance stating the facts, and received in reply such an astonishing array of diagrams and mathematical proofs that this could not be done that I actually went into the yard and had the motor perform the feat several times before I felt strong enough to write my friend in a half apologetic way, acknowledging the soundness of his views, but being compelled to reiterate the statement that the thing was being done just as I stated.

But seriously speaking, the significance of this test can scarcely be overestimated, showing as it undoubtedly did, the possibilities of working under conditions which have been hitherto considered impracticable.

Now I do not for a moment pretend to assert that either dynamic or static adhesion may be increased to this extent by a large locomotive under the conditions of ordinary practice, as I am fully aware from subsequent experiments that the observed effect in this instance was due to a very large current of low potential passing through the very small contact-areas presented by wheels only one foot in diameter, impinging upon a light sixteen-pound rail, the effect is, however, undoubtedly obtainable to the extent of at least thirty per cent., under the ordinary conditions of railroad practice, since I have observed it many times with a dynamometer by causing the current to pass from wheel to rail in one series of measurements and through a cable connected with the motor in another series, and in this way results as high as thirty-five per cent. increase have been many times noted. In addition to this it is more than probable that the tractive efficiency of electric locomotives is higher than that of ordinary steam locomotives for mechanical reasons, and in this connection I cannot do better than quote Professor John E. Sweet, in a recent article in the *American Machinist*, since it is put in a direct, clear, and simple manner. But as Professor Sweet declines to admit the increase to be due to electrical action, I quote him simply in evidence of one of the mechanical advantages of the electric motor :

> **TRACTIVE EFFICIENCY OF ELECTRIC LOCOMOTIVES.**
>
> **Reference has been made to the fact that the electric locomotive will draw a considerably heavier load than the steam locomotive of the same weight, and the suggestion advanced that it is due to some electric condition of the rails and wheels, while I believe it has been clearly shown that certain things slide one upon the other much more freely when a current of electricity is passing from one to the other than when not. I do not know whether, in the case of the Ninth avenue cars, electricity passes from the wheels to the track, but if it does, the result is quite the reverse from the action assumed in the case of the locomotive wheels, and it does not seem at all necessary to refer the matter to any electrical or magnetic theory, as the mechanical conditions alone are such as to explain the apparent inconsistency, and in this way : The power of the electric motor is constant and the steam intermittent. If the steam locomotive has but a single cylinder, and the power required to draw the train be 700 lbs, the force exerted by the piston would be zero when the crank was at the dead center, and 1,100 lbs. at the maximum ; so that while the electric motor doing the same**

work would exert only 700 lbs. toward slipping the wheels, the steam locomotive with one cylinder would at times exert a force of 1,100 lbs. This assumption does not take into account the varying steam pressure, nor the angularity of the connecting rod, which in certain positions would make it more, nor the weight of the reciprocating parts and the inertia of the drivers, which would make it less, though, as slipping takes place in starting the train, the inertia would have but little influence.

It is true that the steam locomotives are not single cylindered, and at first sight it would seem that that makes a vast difference, and that with two cylinders the impulse is almost constant; but even assuming the pressure to be constant throughout the stroke, as was assumed with the single cylinder, the rotary effect of the two cylinders is not constant, and (unless I am badly mixed in my diagram) with a mean 1,409 lbs., or double the mean of the single cylinder, there will be a minimum of 1,100 lbs, and a maximum of 1,571 lbs. This variation, together with the varying steam pressure on the piston, it would seem, is sufficient to account for the extra efficiency of the electric motor, if I am correct in my assumption that the limit in both cases is the slipping of the drivers.

I have dwelt longer on this branch of the subject than it might seem to warrant for the reason that an eminent authority, in a paper read some three or four years ago, emphatically declined to admit this line of argument or the existence of this effect, and, in strongly advocating a multiple motor system, declared that increased weight was the only possible means of increasing the tractive ability of the motors and that to propose any other plan was simply to shut our eyes to plain mechanical and engineering truths. I am pleased to observe that in a later paper the truth of the effects which I had discovered and strenuously insisted upon early in 1883, have been acknowledged by the same authority.

Referring to the plan of multiple motors where it was proposed to use one or two machines on each car, while there can be no question that the advantages are theoretically very marked in some directions, I think the multiplication of motive parts, necessarily much smaller and more liable to derangement than those on one machine of equivalent power, together with enormously increased first cost, and cost of attendance and repairs, present practical disadvantages from which any railroad manager would naturally shrink; and, notwithstanding the able manner in which this plan has been presented and its obvious theoretical solution of a much vexed problem, I do not see any reason for changing my original plan; on the contrary, exhaustive experiments recently conducted lead me to the conclusion that the problem may be readily solved in another way.

Lest any misunderstanding should exist as to the object of this series of tests, permit me here to remind you that this motor was not expected to solve the problem by towing longer trains at higher speeds than are at present attained. It was simply for the purpose of proving that a machine weighing little more than one half the present motors would equal, or nearly equal, their performance in that respect, and, while it must be admitted that in the matter of acceleration on the heavier gradients the steam motors still show somewhat greater ability, the result of the test indisputably proves that, with motors of the same weight as those now employed, at least six car trains on the Ninth Avenue, and seven to eight car trains on the Sixth Avenue, may be propelled at a considerably higher average speed than at present, and when the absence of reciprocating parts is taken into account, the advantage on the side of the electric motor, in lessening the destructive strain on the structure, will form an important factor in the economy of operation. That the absence of this destructive strain is no idle dream of the mechanical engineer has been fully realized by many competent observers of the electric trains, and it has been a subject of general remark that the noise of the electric train is merely that inseparable from the rolling of wheels over the track, the motor itself making no more noise than one of the cars in motion.

I refer to these points particularly because so many disappointments have been experienced by the public, who seem to expect the electrical genie to wave his magic wand over the earth and cause all mechanical difficulties heretofore experienced to vanish in a moment, hence it is pleasant to be able to refer to the actual accomplishment of some of the most desirable advances in municipal rapid transit.

But the principal object is to show without fear or favor that it is now possible to run, and compete with steam on the Ninth Avenue railroad; and, of course, any of the others with a certain concrete economy based upon a series of tests made under circumstances to the last degree prejudicial for an economical showing and without taking into account the great gains which would inevitably result from the use of a large central station, with dynamos of the highest efficiency, and with the obvious advantage resulting from the operation of a number of motive units under varying loads from a central station or dwelling upon such abstract economies as freedom from cinders, dropping water, smoke and noise.

Before proceeding to a review of the actual accomplishment, allow me to briefly describe the motive machine in question. The motor, named "Ben Franklin," consists of a frame having two driving wheels coupled by discs outside the bearings, with two cast-steel cut split gears mounted on the back axle and an electric motor pivoted on pedestals at the rear of the cab and supported on similar pedestals with a screw adjustment for raising the whole machine to remove the armature, and wrought steel pinions in nearly vertical engagement with the gears.

It is evident that a considerable degree of resilience is not permissible with such an arrangement, but a sufficient amount to prevent excessive hardness in working is obtained by placing alternating plates of rubber and iron in the pedestals, above and below the bearings. A similar arrangement at the driver-bearings cushions the machine without interfering with the mesh to an appreciable degree, in fact what little wear has so far been observed between pinions and gears shows remarkable uniformity of engagement.

Before the motor was placed in the cab it was deemed advisable to make a few Prony brake tests of its ultimate ability; it was therefore placed in the central station at 15th street and a series of tests made, resulting in the development of 128 horse power. A few higher readings than this were apparently taken but as some little doubt is felt as to the accuracy of the observation, those were rejected and the above accepted as the ultimate reading.

The piece of track upon which the experiments have been conducted is eminently adapted for testing the tractive ability of a motor for this purpose, since there is only one piece of level track, 2200 feet in length, the rest, embracing a distance of 1.846 miles, consisting of gradients varying between 11.3 and 98.7 feet per mile, and in one instance, namely, at 30th street, the start is made only a few feet from the bottom of a 98.7 foot gradient, thus testing the tractive capacity of the motor to the utmost.

In October last, experiments with this machine were begun on this piece of track, which, through the courtesy of Col. Hain, was placed at my disposal from 9 o'clock at night until 4 o'clock the next morning, and the first experiment consisted in taking a light load of one or two cars over the road to test the conductor and switch connections. This was gradually increased night after night, until one of eight Ninth Avenue cars, each weighing

12 tons, was towed over the entire road, going up the 98.7 feet grade at a speed of 7½ miles per hour, and making the entire distance in 7′, 35″, or at an average speed of 14.6 miles an hour. Although a drizzling rain prevailed and the rails were moist, there was an entire absence of slipping.

I regret that at the time of this experiment we were not using the dynamometer, but taking the ordinary coefficient of rolling friction for a light train of this kind, namely, .004, and assuming the grade resistance to be at the same rate per cent. of the total weight of the train as the rate per cent. of the grade, and the rate per cent. of the grade being 1.86, we get the following, $\frac{1.86 \times 4781 \times 660}{33000}$ 95.8 h. p. to which should be added at least 15 per cent. for friction and a so-called "head resistance" of the motor, equalling 14.37, or a total of 110.17 h. p. at 15th street.

The maximum velocity obtained by that train on the level in passing 23d street was 24 feet per second, or 16.36 miles per hour.

These tests were continued for several weeks at intervals of two or three nights, during which time a great number of experiments were tried as to the ability of the motor to make the switches, start on gradients, etc., and I am able to state, as a simple matter of record, that, though the tests were of the most exacting character, absolutely the only repairs or delay during all this time and subsequent tests were two, once when a spool had to be removed from developing a contact, which caused a delay of one night, and the other time when a moving axle wore a hole through the insulation of an end spool and caused a delay of fifteen minutes. With these two exceptions the motor has been ready to run at any moment during the whole time, in spite of the roughest treatment and being left on the track exposed to the weather for months past.

I think I am entitled to say that these results, which were of course carefully watched by the Manhattan railway officials, led to our obtaining permission to run in the day time between steam trains—a permission which you may be sure would not have been granted on a road which has a world-wide reputation for its almost military discipline and precision of management—if the motor had not earned the right to some confidence. On the 28th day of January, 1889, we began running between the hours of 11 and 2 in the day time, which was continued with a three-car and occasionally a four-car train, running express until the 12th day of February, when we were requested to take a train of flat cars loaded with iron to the weight of an average four-car train with seventy-five to one hundred passengers, in lieu of the light train formerly employed. With this train, weiging in all 70 tons, I made several trips over the road between the ordinary steam trains, but finding the brakes hardly trustworthy enough for such critical work with trains only three minutes from us on either side, it was decided to discontinue this service and a four-car Sixth avenue train was substituted. With this train several trips were made, but in consequence of the absence of air brakes it was not considered advisable to proceed with the ordinary "stop run," and the remainder of the work was done at night. But I may say that during this period of 30 days with one exception above noted, not the slightest detention was caused to the regular trains, as we were always able to switch onto the track, make our trip and get back onto the switch at 50th street without interfering with the steam trains in the least.

A number of experiments made about this time show that the mean speed, with a three-car empty train, running express on the up-town track, was about 23 miles per hour, though on the level, the ability of the motor with a similar train is nearly 28 miles per hour. This is by no means the limit of speed which the motor is capable of attaining, for as will be seen by reference to diagram No. 1, the work of acceleration was still in progress when the gradient at 24th street was encountered, hence it seems perfectly fair to assume that, even with that 40 ton train, the maximum level speed is not shown. With a lighter load the speed would undoubtedly be much greater, but for prudential reasons it was not deemed well to push this matter of speed to its extreme limit, especially in view of the fact that the machine was not designed for a speed of over 25 miles per hour, and any derangement resulting from excessive peripheral armature speed might have prevented my ability to present a clean record to-night.

To any one traveling on a structure similar to the Ninth avenue elevated road a speed of 28 miles per hour, particularly at night, and at an altitude of thirty feet, seems to be sufficient for all practical purposes, and I must confess that in driving the motor at that speed under those conditions, I was conscious of a marked absence of any desire to greatly increase it; neither does it seem to be necessary that greater speeds than thirty miles per hour should be attained in the city limits (by express trains) to meet all the proper requirements of rapid transit, and when the great power which experiment shows to be required for the accelerative effort between the frequent stations, and with the comparatively heavy trains on the Sixth and Third avenues are taken into account, it is extremely improbable if this speed will ever be exceeded between stations on these structures.

Referring to the methods adopted in making the measurements which are plotted on the diagrams, it should be noted that wherever any doubt existed as to the effect of the personal equation in the observations, changes were made in the observers, and the experiments repeated, so as to leave scarcely a possibility of any material error, and in order that the always objectionable average statement should not contribute to confuse the result, the diagrams were taken from a number of similar ones which, after careful examination were found to most fairly represent the mean effects. They are, therefore, within the limits of error in observation, the exact results of actual performance.

In order to measure the speeds with sufficient accuracy for all practical purposes, I divided the track into nineteen sections of 500 feet, each section being provided with a thin copper plate attached to the guard rail at the side of the track, and connected with the copper rod or insulated side of the conductive system. This plate was made as short as possible to insure a complete contact without occupying an appreciable interval. A copper brush attached to the motor in such a manner as to travel on the top of the guard rail was connected with one terminal of a chronograph of the well-known type, which we made for this purpose, consisting of a paper covered cylinder rotated at a uniform speed by clock work, and a stylus actuated by an electro-magnet so as to penetrate the paper at every closure of circuit. This instrument was carefully tested and the results verified by experiments before the readings were accepted. The indicator cards were taken at the central station by Mr. Idell and his assistants, after a careful comparison of watches so as to insure synchronism in observing the effects of acceleration and gradients. The dynamometer used was a very fine specimen of the liquid type owned by the Manhattan railway, and made by Mr. Shaw, of Philadelphia, and after careful calibration by Mr. Shaw and Mr. Idell, was found to be accurate within an inappreciable amount. The dynamometric readings I have omitted from these observations for the reason that they were found to throw so much light upon the ordinarily accepted coefficients of train resistance under many of the conditions which the text-books have not yet considered, and especially the formulæ relating to the subject of acceleration. that time would not permit a thorough digest of the results, hence it was deemed better to leave this part of the subject for later consideration, and I trust you will shortly hear from Mr. Idell on this subject, in his usually able and thorough manner.

Referring to the diagram No. 1, representing the run of a two-car express train, having a total weight of 40 tons, it will be seen that the initial indicator card is exceedingly high as compared with the apparent effort required. The reason for this is that our plan in taking the cards was to make the first one at the instant of closing the circuit, thus giving a card due to the simple dead resistance of the motor which, with a ratio of peripheral velocities necessary for this work and a speed of 30 miles per hour, would necessarily be low and occupy at least a few seconds in developing an economic working resistance, unless a somewhat complicated arrangement was resorted to. But this point will be later referred to.

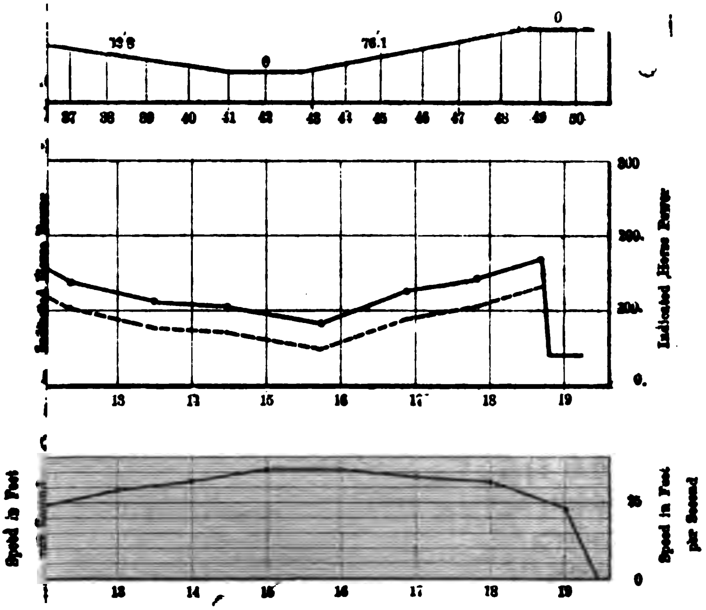
Following the speed line it will be seen, as might be expected, since we see that the accelerative effort is rapidly diminishing towards the foot of the gradient, that the indicator cards become a minimum at the maximum velocity of the train, but quickly rise beyond 27th street, where the gradient is 98.6 feet per mile. The effect, however, of acquired momentum is here very evident, since neither the indicator cards nor the declining velocity are at all proportionate to the resistance imposed by the gradient, and the speed of the train varies but little from this point to 46th street, being only slightly increased on the down grade between 34th and 42d streets, due to a change in the combination for the purpose of checking what would otherwise have been too great speed for safety. The dips in the indicator line between 36th and 37th streets is occasioned by the cards having been taken at the instant of the change in combination ; at 46th street the decrease in speed is the result of encountering the gradient, 76.1 feet per mile, and between 48th and 49th streets the circuit was opened for the stop at 50th street, when, as will be seen, the indicator line quickly drops to the friction card only. This run of 1.846 miles was made in 4 minutes and 51½ seconds, as recorded by the chronograph. This is at the mean speed of 22.86 per hour, with a maximum of 28.4 miles per hour indicated on the up grade between 44th and 46th streets. The speed in passing 23d street, the end of the level stretch, was 28 miles per hour.

The mean power exerted at the central station for propulsion during this trip was 103 horse power, which will compare not unfavorably with steam practice.

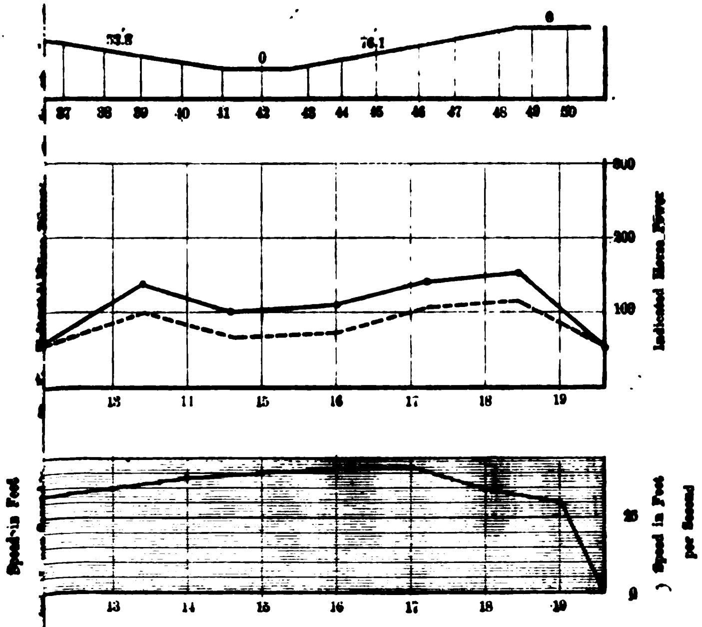
Turning to diagram No. 2, representing a four-car express train, having a total weight of 70 tons, or about the average weight of a four-car loaded Ninth avenue train, we are confronted with the curious fact that the initial indicator card is lower than that shown with the two-car train, but a careful examination of the two diagrams will show that the card was taken a few seconds after the stop and not, as in the case of the two car train, at the instant of closing the circuit. The effect, therefore, of the rapid development of working resistance is very clearly shown and is a most important factor in considering the high initial cards, since they are of only an instant's duration and hence comparatively unimportant in their effect upon the total power required to run a considerable number of trains at one time. In this run the characteristics of the two-car run are very nearly reproduced, though it is particularly noticeable that the indicator line does not show so large an increase as one might, at first thought, expect from doubling a number of cars, but this is accounted for by the greater ratio which the friction and load of the motor presents to the two-car train.

The total time of the trip with this train, consisting of 4 Sixth avenue cars, each weighing 15 tons, plus the motor, weighing 10 tons, was six minutes and six seconds, with a mean speed of 18.15 miles per hour, and with a maximum speed of 25.24 miles per hour between 42d and 43d streets, which is, however, almost equal between 24th and 25th streets, when, as above noted, acceleration was still in progress. The mean power exerted at the central station on this run was 129.3, resulting from 91 ordinate readings. This result is almost exactly the same as the running time of the express steam trains of equal weights over the same distance and may be repeated at any time.

Comparing the two former with diagram No. 3, representing a four car "stop run," several of which were made chiefly for the purpose of demonstrating the ability of so light a motor to stop at the foot of a gradient of 98.7 feet per mile, as at 30th street, with a train of 70 tons; and ascend the gradient from a position of absolute rest with ease. These "stop-runs" with this train,

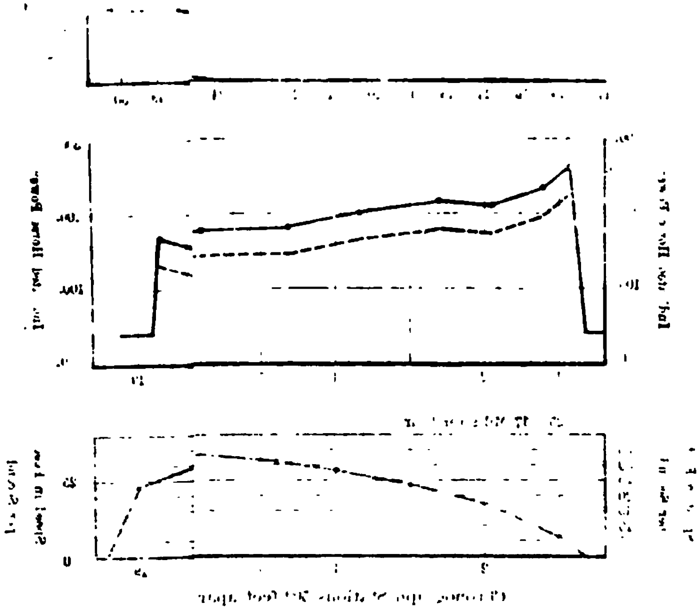


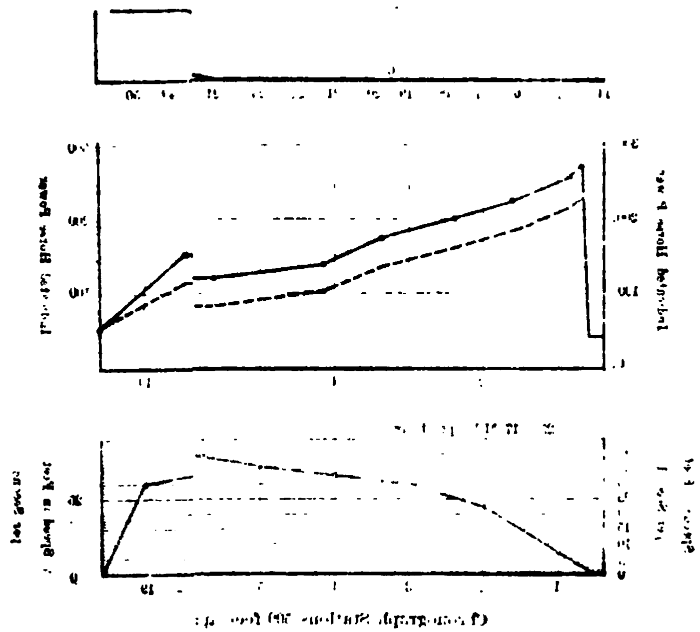
Time 6 min. 6 sec.



Time 4 min. 51¼ sec.

LROADS—Leo Daft. June 25th, 1889.





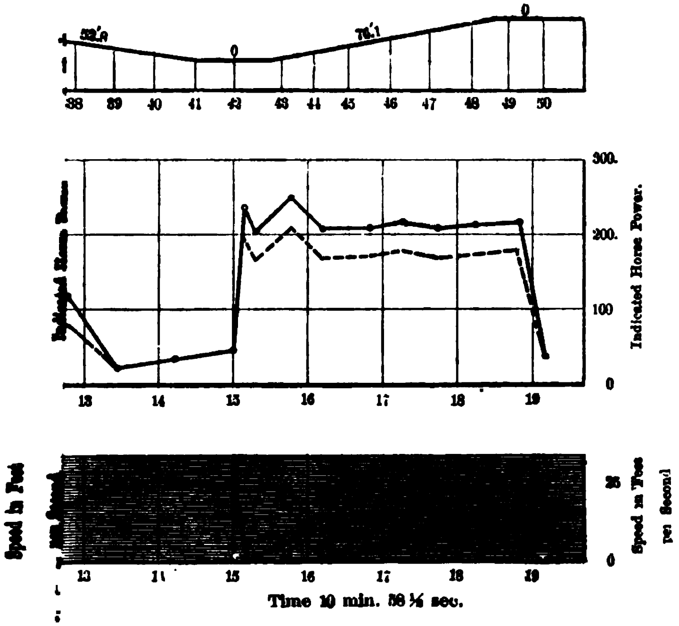
were made many times without encountering any difficulty what ever, and at no time during the whole tests has the motor shown indications of overload. That the four-car "stop-runs" were not made in schedule time will be seen by the diagram, but a careful inspection of the same will show the exceedingly prejudicial effect of the absence of an efficient brake system, compelling the opening of the circuit two or three blocks before the station is reached, in order to ensure a proper stop at the platform. This is graphically shown on the indicator lines of the diagram where the circuit is seen to be open at 19th street, for the stop at 23rd, thus considerably lowering the speed, and of course stopping the rapid acceleration shown on the speed line at the third chronograph station. This disadvantage was so marked that I do not present the four-car "stop-run" as evidence of anything but the ability of the motor heavily loaded to ascend the gradients under the exacting conditions of an exceedingly difficult piece of track.

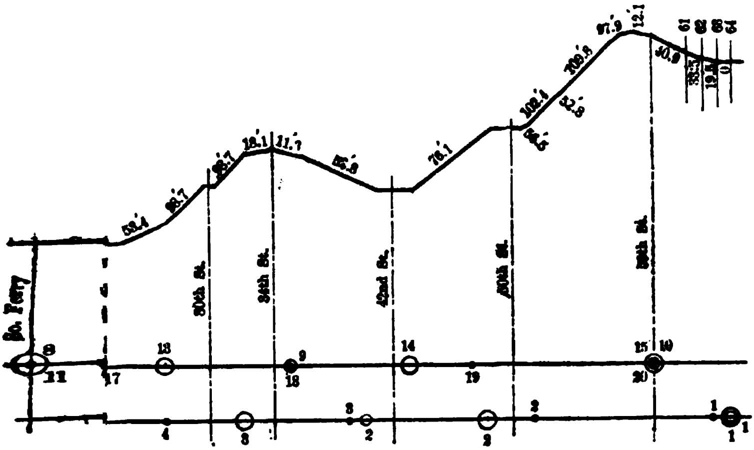
That there is not a marked difference between the ten ton motor and the eighteen ton locomotive in the initial effort on the level, may be seen by comparing a run observed by a Manhattan railroad official, on March 9th, last, with a steam motor and three-car train with 75 passengers, or a total of about 57½ tons; we see that while the steam motor required one minute and thirty-nine seconds to make the distance from 14th to 23d streets, the electric motor in diagram No. 3, accomplished the trip, with a train weighing 14 tons more, in one minute and fifty seconds, or only eleven seconds added for the increased 14 tons. Surely not a discouraging exhibit, considering the fact that while the absence of powerful brakes compelled the opening of the circuit at 19th street, the throttle of the steam motor was probably open up to 22d street, although on this point I have no direct evidence —but that is the usual practice.

Before dismissing this diagram, I will again remark that while the mean speed was not equal to the ordinary schedule of the Ninth avenue road, it is nearly equal to that of the Third avenue, with 22 ton motors and 80 ton trains. In considering the apparent mean expenditure of power at the central station, it should be borne in mind that the efforts exerted by these motors on such rapidly varying gradients, and over such short distances on the elevated railroads, are not by any means represented by the ordinary coefficients of train resistance and effects of gravity, as it has been before pointed out very clearly by others that, while the up grades always exert their full effect, the advantage generally obtained from down grades on long runs by acquired momentum, is here nearly always lost by the position of the gradients with reference to the stops. It has been shown by Messrs. Sinclair and Campbell that on the Third avenue road with the 80 ton trains and 22 ton motors, 170 horse power is not infrequently exerted, hence to consider this problem merely by the ordinary coefficients of rolling friction and gravity resistance would, and does frequently, lead to very serious errors in computing the amount of power actually used, on short railroads, with frequent stops.

However it is not my purpose to enter into the niceties of train resistance, since the best authorities differ widely on these points, and theoretical economics form no part of the present subject. But as I think I am presenting to you for the first time a critical examination of actual competition with steam motors on steam roads, in the work of towing entire trains, I trust you will not fail to recognize that electricity is at last measured and weighed against its most formidable opponent in railway practice.

For many years the electric motor struggled hard to prove its ability to compete with even the most costly competitors, namely, manual labor, and even here the introduction of the dynamo machine was necessary to achieve the victory; then quickly followed the long competitive battle with horses, in which it is evident the equine has been signally defeated, and to wrest a victory from the steam locomotive in its own domain is quite another matter, but I think you will agree with me, that proof of a large economy in the vital item of fuel, under decidedly adverse circumstances, may be so construed without dangerous optimism; and the result of all these tests has seemed to prove that the measurements might be laid bare without in the least shaking the confidence of competent judges in the ability of the electric motor to replace steam for service, at least as severe as that of the elevated railroad systems; and in amplification of the results obtained, we have here a diagram (No. 4) graphically representing the present service of the Ninth avenue elevated road, where the positions and number of trains for all hours of the day are plotted, and the actual horse-power required, as shown by the indicator cards.





ADS—Leo Daft, June 25th, 1889.

No. 1.

10 three-car trains to be run under six-minute headway between 59th street and South Ferry.

Train No.	Location.	H. P. Required.
10	Starting at 59th street	200
9	Up grade to 34th street	150
8	Running on level	130
7	" "	130
6	Starting at Battery place	190
5	" Rector street	190
4	Running on level	130
3	" "	130
2	Down grade to 42nd street	—
1	On switch at 59th street	—
		1250
	Add 10% for contingencies	125
	Total	1375

ESTIMATED AMOUNT OF POWER REQUIRED TO EQUIP, ELECTRICALLY THE NINTH AVENUE DIVISION OF THE MANHATTAN ELEVATED RAILWAY.

10 four-car trains to be run under six-minute headway between 59th street and South Ferry.

Train No.	Location.	H. P. Required.
10	Starting at 59th street	200
9	Up 52 feet grade to 34th street	200
8	Running on level	150
7	" "	150
6	Starting at Battery place	200
5	" Rector street	200
4	Running on level	150
3	" "	150
2	Down grade (52 feet) to 42d street	—
1	" " into switch 59th street	—
		1400
	Add 10% for contingencies	140
	Total	1540

15 four-car trains to be run under four-minute headway between 59th street and South Ferry.

Location of trains when train No. 15 has been under way two minutes.

This gives larger results than when train No. 15 is starting from 59th street.

Train No.	Location.	H. P. Required.
15	Down 105 feet grade to 50th street	—
14	Up 52 feet grade to 34th street	200
13	Running on level	150
12	Starting at Christopher street	200
11	Running on level	150
10	Starting at Cortlandt street	200
9	" South Ferry	200

Train No.	Location.	H. P. Required.
8	" Battery place	200
7	" Barclay street	200
6	Running on level	150
5	" "	150
4	Up grade from 23d street to 30th street	200
3	Down grade to 42d street	—
2	Up 105 feet grade to 59th street	250
1	On switch	—
		2250
	Add 10% for contingencies	225
	Total	2475

20 four-car trains to be run under three minute headway between 59th street and South Ferry.

Advancing trains one minute from start.

Train No.	Location.	H. P. Required.
20	Down grade to 50th street	—
19	" " 42d street	—
18	" " 30th street	—
17	Running on level	150
16	" "	150
15	" "	150
14	Starting at Franklin street	200
13	" Barclay street	200
12	" Battery place	200
11	Running on level	150
10	Starting at Rector street	200
9	Running on level	150
8	Starting at Warren street	200
7	Running on level	150
6	" "	150
5	" "	150
4	Starting at 30th street	250
3	" 42d street	200
2	Up grade to 59th street	220
1	On switch at 59th street	—
		2870
	Add 10% for contingencies	287
	Total	3157

RESUME.

And to obtain the average for the day.

Train No.	Cars on Train.	Headway.	Time A. M. A. M.	Time in Service. hs.	min.	H. P.	H. P. hours.
10	4	6 minutes.	5.09 to 6.03	0	54	1540	800
20	4	4 "	6.03 to 6.15	0	12	3157	631
21	4	3 "	6.15 to 7.00	0	45	3157	2367

Train No.	Cars on Train.	Headway.	Time.	Time in Service. hs.	min.	H. P.	H. P. hours.
			A. M. A. M.				
20	4	4 minutes.	7 00 to 7.24	0	24	3157	1315
21	4	3 "	7.24 to 7.41	0	17	3157	1052
20	4	4 "	7.41 to 9.03	1	22	3157	4209
			P. M.				
10	3	6 "	9.03 to 4.15	7	12	1375	9900
			P. M.				
20	4	4 "	4.15 to 5.07	0	52	3157	2735
22	4	3 "	5.07 to 5.53	0	46	3157	2420
20	4	4 "	5.53 to 6.13	0	20	3157	631
10	4	6 "	6.13 to 9.00	2	47	1540	3880
				15 hs.	51 min.		29940

Average h. p. per hour $\frac{29940}{16}$ = 1871 h. p.

To which must be added the power consumed in engine and dynamo friction and loss in line.

1871
Station friction, 300
2171 h. p.

Evaporation, 7.5 water for 1 lb. of coal.

Hence coal consumption,

2171 x 16 x 2.2 = 38 + 3 tons for banking, = 41 tons.
at $2.25, = $92.25 per day.

From this table it will be seen that for five hours out of the sixteen of daily service 2870 h. p. would be required, or adding 10 per cent. for contingencies, 3157 h. p. during these times, when twenty and twenty-one four-car trains would be in operation under three minutes headway, being reduced to 1250 h. p., or, adding 10 per cent. for contingencies, 1375 h. p., for the seven hours and twelve minutes intervening between 9.03 A. M. and 4.15 P. M., when ten trains of three cars are in service under six minutes headway, while during fifty-four minutes of the early morning, between 5.09 A. M. and 6.03 A. M., and the late evening service of two hours and forty-seven minutes, between 6.13 P. M. and 9 P. M., in which case ten four-car trains are in service, 1400 h. p. would be required, or, again adding 10 per cent., 1540 h. p.

It should be remarked that the power indicated for this service might be very advantageously divided up into four engines of 800 h. p. each, as an examination of the traffic table will show that for only five hours per diem would the whole of them be required, and during the long period of seven hours and twelve minutes, between 9.03 A. M. and 4.15 P. M., two of the engines would be ample for the service.

The results are, however, plotted, and show that the total horse power per diem equals 29,940, or, divided by 16, the number of hours of service, 1871 h. p. per hour, and adding 300 h. p. for dynamo and engine friction, the total should be 2171 h. p. per hour, which, at 2.2 lbs. of coal per horse power per hour, equals 38 tons of fuel consumed per diem, and adding three tons for banking the fires, equals 41 tons, at $2.25 per net ton, or a total of $95.25 per diem for fuel alone.

In this connection I should mention that the coal here referred to is a mixture which may be obtained at the dock in New York for about $1.80 per ton, and which is shown by Barrus in his work entitled "Boiler Tests," to be capable of evaporating 7½ lbs. water per pound in a properly constructed furnace.

There are several makers of compound engines in this country who will guarantee the performance of their engines on 16 lbs. of steam per horse power per hour. It is thus evident that the amount of coal above referred to is a safe one for modern practice on so large a scale. The present locomotives on the Ninth avenue require on an average 40 tons of coal per day for motive power and braking, but experience has shown that it is economical in small locomotive practice to use coal of the best quality; hence the cost of this 40 tons of coal is $200 per day, or more than double the cost of that for the electric motive system.

It is, of course, necessary to add to the electric estimate a number of charges, such as wages of firemen and engineers at the central station, which would probably bring this item up to about $150 per diem, considering other items balanced by their practical equivalent in the present service and the cost of repairs, depreciation, etc., to be about equal, which is not far from the fact.

It is thus evident that without considering the future obvious advantages which must necessarily accrue from the use of a great central station equipped with dynamos as before stated, and a conductive system ample for all requirements of the road, that with the comparatively wasteful central station arrangement we have here considered, involving the use of small dynamos of a very old type placed at the extreme end of the conductive system, and suffering from many incidental disadvantages, that it is possible to run the Ninth avenue elevated road with electric motors at an actual and considerable saving of fuel to-day, and if this is not the only example of such a practical demonstration, as opposed to direct steam propulsion, it is at least the first I have been able to find on record.

The experience of these extended tests and almost innumerable indicator cards, together with countless observations which have been made during the past eight months, have naturally borne fruit in eliminating many objectionable features which only such an examination could develop; and though, as has been said, it is no part of the object of this paper to treat with other than actually accomplished work, it seems but proper to add that the experience gained in this direction has been embodied in another motor, now nearly complete, capable of not only doing such work as is at present accomplished, but of solving, at least for the near future, the vexed problem of extended traffic for the elevated railroads, and, though no actual tests of it have yet been made, there can be little doubt, judging from the performance of the present machine, that it will be capable of towing at least a six-car train on the Ninth avenue elevated road with a considerably increased average speed and with a comparatively low initial effort, due to a change in the proportions of parts and manner of winding, to meet the peculiar conditions shown in the diagram.

In this arrangement the braking of the motor and train has been electrically provided for, also, of course, the lighting, but as this machine is not actually finished, I will not further refer to it other than to call your attention to a part of the proposed plant, namely, the conductor, a section of which has already been placed on the Ninth Avenue elevated road at the curve below 14th street.

There are also economies of braking and in the manipulation of the train, to which I cannot at present further allude, which would, however, in all probability, considerably reduce the working expenses. In this motor the experience which we have had in handling has led to great simplification of the regulating and reversing gear, so that one man may handle the train without the least assistance.

That the plan first suggested by the late Sir William Siemens of braking the train by converting the motor into a dynamo, and so returning a part of the stored up energy of the train to the conductive system, would result in considerable economy, there can be no doubt, provided a thoroughly satisfactory method of practical application were applied, but it is an item that does not appear very conspicuously in any of the quantitative examinations which I have yet seen of any electric system, and while I do not wish to be understood as lightly estimating such an obvious economy, before accepting it as so important a factor as its theoretical consideration by some electricians would indicate, I should like to see a few carefully digested observations of actual performance with motors of high powers.

In conclusion, I trust that if the foregoing facts are not accepted as conclusive proof that the problem is already solved, that they may at least furnish a sufficient amount of tangible material to assist in the early and complete accomplishment of our most sanguine hopes.

DISCUSSION.

THE CHAIRMAN (Vice-President Francis R. Upton):—The question of quick transit in New York City is one of importance to all of us, and I think that this paper should bring forward a number of speakers. We will be pleased to hear remarks.

MR. CHAS. G. CURTIS :—I would like to ask Mr. Daft if he could estimate the average efficiency of the motor as they have been running it, taking fully into account the results obtained.

MR. DAFT :—I have not done that. The object of these tests was to demonstrate that we could run economically in the matter of fuel; and such estimates as we have of its efficiency I am hardly prepared to show to-night. In fact the experiments have not been entirely concluded; and, as I say, this work was intended simply to demonstrate the concrete and absolute economy in the matter of fuel. It is quite easy to deduce from these diagrams a low efficiency in some cases, especially on initial and accelerated effort; but the concrete economy is that which I specially refer to, and that is absolute.

THE CHAIRMAN :—Are there any other questions to be asked or remarks to be made on this subject? The great advance in electrical engineering and the possibility of carrying people, both on surface and elevated roads by means of electricity, is a matter of great importance at the present time.

MR. CURTIS :—Do you pass the entire current through the tread of the driving wheel of the locomotive so that the total current is occupied in increasing the traction.

MR. DAFT :—Yes.

MR. CURTIS :—With regard to the question which Mr. Daft brought out, of how much the calculable total horse-power consumed on the elevated railroads is, I may say that I had a little experience which was rather amusing, because it tends to show how easy it is to make a very great and a very ridiculous error in figuring upon such a problem unless every element is carefully considered. About four years ago, in the winter time, I was very much interested in the subject of running the elevated railroads by electricity, and I had the means of obtaining reliable data as to their speed, coal consumption, and everything that was known at the time, and I figured out in the ordinary way what the average horse power of those locomotives was; for the purpose of getting the average coal consumption. It turned out that the average coal consumption, as I determined it, was about twenty pounds of coal per horse-power hour; and that agreed with a calculation which had been made before, and with the results obtained at that time by electricians concerning the substitution of electricity for steam. When I made that calculation it occured to me that the element of inertia—the overcoming of the weight or inertia of the train—was a matter that ought to be considered, but I put it aside, assuming that it could not amount to a very considerable portion of the whole power. About a month later something brought the subject to my mind again, and upon revising my calculations and allowing for that element, I found it to be three times as much as the entire tractile effort, assuming the maximum speed on trains at that time as twenty-three miles an hour; taking the weight of the trains and figuring the energy that must be put into the train to overcome this inertia; dividing that by the total distance traveled in a given time (which I think was about 1,100 feet per minute) it showed that about 58 or 60 horse-power was expended simply in overcoming the inertia of the train. That was in addition to the tractile effort. I went over the figures again, very carefully, and I think they were published in the *Electrician and Electrical Engineer*, where the original figures, which were not more than four or five hundred per cent. wrong, had appeared about two months before. About four or five months later, Mr. Campbell wrote me that he was going to make some tests on an elevated railroad locomotive. I did not see the tests, but he sent me the results of them. He found the average horse-power throughout the run to be between 70 and 80, which was within 10 or 15 per cent. of what I figured it. I do not mention it by way of taking the slightest credit for any accuracy in the calculation, but simply to show how easy it was to figure the whole thing out, and get it right when you gave to every element the importance that it ought to have. The same calculations showed that a great deal of the power, anywhere from 100 to 200 horse-power, was necessary to start the train with the acceleration that they required in order to make schedule time on the elevated railroad. Mr. Campbell's experiments determined the remarkable fact that those locomotives developed a horse-power with about five and a half pounds of coal per hour. These figures were published in the *National Car Builder* at the time, and were so startling that they were denied by a great many civil engineers. I think the question of whether it is better to have a single locomotive to each train or to make each car its own locomotive is a matter that can be determined only by experiment. Of course, as has been said, there are very strong theoretical advantages in favor of having each car its own locomotive. It dispenses with the main locomotive and the heaviest part of the train under the present steam system; and it enables you to increase the length of the train indefinitely. I understood you to say, Mr. Daft, that your machine was about one half the weight of the present locomotive.

Mr. Daft:—It was rather more than one half of the weight. The machine weighs ten tons, and I believe the locomotives on that road weigh a little over eighteen tons. Some of them are a little more than that, but I think that a fair average.

Mr. Curtis:—You only use one rate of gearing?

Mr. Daft:—Only one.

Mr. Curtis:—Have you any friction clutch?

Mr. Daft:—Nothing of the kind on the present motor.

Mr. Curtis:—May I ask if you know about how the counter-electromotive force compares with the direct when you are running at full speed?

Mr. Daft:—I have not the figures here to-night, and cannot give you those particulars now. I have particularly confined myself to this question of coal economy, and I propose, if I have the opportunity later, to deal with the other part of the subject.

Mr. Joseph Wetzler:—Mr. Daft alluded several times to the effect of the absence of brakes on his train, and also states that in the new train which he is equipping he proposes to introduce proper brakes. I would like to inquire what form those brakes will take, and whether they are direct electric brakes, or those in which electricity is merely used as an auxiliary, the momentum of the train being utilized to create a vacuum, as has already been done.

Mr. Daft:—The electricity is an auxiliary, but I am not at liberty to enter into details at present.

Mr. Geo. B. Prescott, Jr.:—It seems to me that in the question of the transmission of power to a great distance, the loss of energy in the conductors themselves is a very important factor, and is one that Mr. Daft has not touched upon. I think it would be interesting to know the initial potential that he used, and what he would consider the practical potential for a line the length of the elevated road, and also how far apart the generating stations would have to be.

Mr. Daft:—I did not propose to enter into that question at all to-night, as that was not a part of this paper. It was simply as I stated repeatedly, to show this concrete economy. I propose later, having an opportunity, to deal with the other part of the subject, and especially when a motor of another type, to which I have alluded, is put on the track. But I may say that I have to some extent, on this particular question, discarded the practice I had before adopted and used a very much higher potential, and that the potential which has been used here is considerably higher than I have made it a practice to use anywhere else, and for many reasons—one of them being that the conductor system is so far out of the reach of the public, on the elevated structure, that it seems hardly possible that any danger can result from it. I have used potentials which are sufficiently high to obtain the necessary economy, without using a copper mine on the track.

Mr. Curtis:—I presume you have not in mind the relation between horse power and the number of miles to be covered, that is, what amount of horse-power is distributed over a mile.

Mr. Daft:—No; I have not.

The Chairman:—I notice that Prof. Anthony is here and we would like to hear from him.

Prof. W. A. Anthony:—Mr. President: I came here to listen. I have not had much to do with electrical traction, and have not very much to say on the subject. I would rather listen to what others are saying; but I would like to ask Mr. Daft if I understood him correctly with regard to the gradient which he has overcome by means of the electro motor. I understood him to say that he has had an electro motor run up a gradient of 45 degrees. Is that so?

Mr. Daft:—Yes.

The Chairman:—We would like to hear from Mr. Mailloux.

Mr. C. O. Mailloux:—I would like to plead very nearly the same excuse that Prof. Anthony has, namely, that I came here principally to listen. My experience with electrical traction has not been in that same line, and I have maintained with regard to it what might be called an expectant attitude; for the reason that while I did not for one moment regard the project of substituting electricity for steam upon the elevated roads, or upon a scale of equal magnitude as an unfeasible or impracticable thing, yet I doubted if the art had advanced sufficiently to enable us to carry out the engineering details of it with sufficient accuracy in order to obtain industrial results that would lead to a successful competition with steam. I am very glad to see the progress in that direction which is now made public in the able paper read by Mr. Daft, this evening. I am surprised to see that more advance in that direction has been made than I had anticipated. At the same time, I am prepared to say, that I still remain somewhat conservative: that while I do not for one moment doubt that success will ultimately be ours, in that field as well as in others, yet we have a great deal of ground to cover before we can relegate the locomotive to the shelves of old and ancient models. I was reading in a Spanish paper recently, an article wherein this anticipated substitution of electricity for steam is spoken of in very rhapsodical and enthusiastic terms, and wherein it is said that we should soon see the steam engine entirely done away with, and shown to our children as a curiosity. I do not think that is likely to be the case. I think that it will be some time before we have overcome what might be called the inertia of the railroad people —before we have overcome their inborn prejudices. It is only a few days ago that I had a conversation with a person interested in the elevated railroad management in some mechanical or technical capacity, and I had occasion to notice what a strong prejudice there is against anything but steam for the purposes of traction in that service. That prejudice is the result not only of a training of perhaps thirty or forty years upon the part of the individual in the art of applying steam to that particular purpose, either upon elevated or surface roads, but also of the observation of electric railway experiments and trials which have been made in various parts of the country, and I might say of the world; and which not being unqualified successes have been very eagerly construed by railway people as being proof of unqualified failure. In order to overcome this long standing prejudice, therefore, we shall have to be sure, and make assurance doubly sure, that everything is all right before we go ahead, in order to be certain that we can command the way, and meet successfully that which now holds possession of the field—steam—and guide him out of it. I would like to ask Mr. Daft in regard to the difficulties (if I may so term them) of conveying current to the motors where the network of tracks is at all complicated by switches. There are certain parts of the system where there is a considerable number of switches and sidings, where I can conceive that there would necessarily be some difficulty in the conducting of current to the motors.

Mr. Daft:—We have not experienced any special difficulties in that direction, although, of course, we have not had entire possession of the track. It has been necessary to leave the track in such condition that the steam motors could operate. But we have, except in one or two instances, much less difficulty by using a contact brush on both sides of the motor, and where the continuity of the conductor was broken on one side we could almost invariably make it good on the other. That is true of almost every switch that we have had to encounter on the system as yet. Even at 50th St., where the switching is more than usually complicated, we have so little difficulty that only in one or two instances have we had to bridge a distance of a few feet (I think in one case five or six feet) by momentum.

Prof. Anthony:—It has always been my belief that we should at some time reach the solution of this problem of electrical traction on the elevated railroad. Ever since the elevated railroad started, or ever since we began to use electrical motors for traction at all, it has been my belief that we should at some time reach the consummation of that result. The difficulties are not electrical. It is easy enough to build an electrical motor which shall have the power necessary to pull the train. The difficulties it seems to me are mechanical and economical. When we take the problem into consideration, the question is, not, is it possible to do it, but, is it possible to construct an apparatus cheaply enough, and to make it operate cheaply enough to compete with steam locomotives. It seems to me that Mr. Daft is doing a great deal to solve that part of the problem. (Applause.) There are a great many problems that come in here to be studied in connection with it. Mr. Daft has referred to one difficulty—that of braking the trains in these early experiments. Of course, some sort of an electric brake must be devised. It is not going to be easy to put an air brake on an electrical train, although we might, perhaps, use an electric motor for compressing the air to use with ordinary brakes, but it will probably be found better to use some electrical system; and such a system must be devised. We must be able to handle the train exactly as we do the steam train. We must be able to make our stops and our starts as quickly, in order that there be no more lost time than at present, so that the difficulties, as it seems to me, are not in simply constructing the electric motor, but in overcoming the various mechanical difficulties which are to be met with in the problem, and in fact in answering all the requirements of the service. As to the economical question I may, perhaps, say a word or two. I have had nothing to do with the study of electrical traction, and have never given it very much thought except in looking over the results which have been reached by others. I have on one or two occasions computed the cost of transmitting power over considerable distances with stationary motors, and I have been somewhat surprised at the results. Assuming 2000 volts as the potential, I have found that, when we come to get up into hundreds of horse power, the cost of the plant becomes something rather enormous; and when the question has arisen of transmitting a water-power over a distance of five or ten miles it does come out every time that a man could locate a steam engine at the point where he wants the power, and if he could get coal at $4.00 per ton, run the whole thing at a less cost, taking into account interest and depreciation of plant, than he could by putting in an electric motor, dynamo and water motor, for transmitting the power.

Mr. Mailloux:—I wish to endorse Prof. Anthony's statement. It was of special interest to me, because it was a statement of my own experience. In making my observations with regard to the progress in the art of engineering, I should perhaps have said that I put the emphasis on the word *engineering*, and particularly on the mechanical part of it, and that would bring my observation at once in accord with Prof. Anthony's; because it is particularly on that side of it, and with regard to the production of the manufacturing process by which the cost of turning out machinery can be reduced, that I have particular reference to. There is no doubt that the cost which the contractor, engineer or company must provide for in order to make a profit on each electric equipment, is considerably higher than the cost of an equipment by steam or other well-known methods under the same circumstances. That is what I meant by progress in the art. I mean the general progress which is the result of evolution, and which has enabled the steam engine manufacturer and the manufacturer of lathes and other machinery to reduce the cost three or four times in a period of twenty-five or thirty years, and to simplify and perfect the construction, and to make a smaller weight of metal and a fewer number of parts perform the same functions equally well, and in most instances far better. Those are the general factors of progress in the art which we must look for in order to accomplish the results. Now, in regard to the question of momentum, I think that the point made by Mr. Daft is a very important one indeed, as is also that made by Mr. Curtis. I can scarcely conceive of any more important factor to deal with in the problem of engineering and in the system of electrical traction, whether on the surface or the elevated railroads, than that of momentum. Because it has such enormous limits, bearing on the cost of power under different conditions, the size of car, the number of miles it can be run, and a great many similar results which are so largely dependent upon it. But there is probably no more influential condition than the distances between the stops. As Mr. Daft has shown, on the elevated roads, the grade happens to be placed in some instances in such a way that the momentum cannot be utilized; but on surface roads I have seen instances where the momentum of trains could be utilized; and only those who have tried it can get an adequate idea of the amount of energy which is stored in the form of momentum in a moving body. Where the gradient is not too severe, the car is sometimes enabled to run for a long distance, and even to come to a comparative stop, and then upon being relieved from the brake pressure, go on with a perceptible rate of speed, and in that way a great deal of economy of energy can be exercised. This is particularly true with regard to suburban routes where stops are not so frequent. But in cities where the stations are located very closely, all this energy which otherwise might be utilized in propelling the car is necessarily lost; and not only that, but we must provide a means of starting quickly. That is the secret of braking a car. We extract from it the energy which exists there in the form of momentum. Of course, if we could find means of utilizing it at the same time, it would be all the better; but I doubt whether it is proper for us to consider that problem at present. It will probably come up later.

THE CHAIRMAN:—We shall be glad to hear from Mr. Martin.

MR. MARTIN:—I think that we owe a great deal to Mr. Daft for the extreme care, and the large amount of work which he has brought to bear upon the paper which we have heard this evening. My friend, Mr. Curtis, had an idea that there were certain things that Mr. Daft might have told us which he did not tell us. In listening to such a paper as this, we should bear in mind that such tests as these are very often made at great expense, and that the results obtained might well be regarded as being the exclusive property of those who make them; and we should appreciate the public spirit, and the desire to advance the profession of those who lay before us such elaborate, careful and exhaustive results and figures as we have had this evening. (Applause.) With regard to some of Mr. Daft's statements I think he will allow me to say that four or five years ago I had an opportunity of seeing him make some of the tests to which he referred; and then my own doubts as to the results which he obtained were as great as those of anybody, until I actually saw the car ascend the grade that he speaks of, with, I think, himself, on board. At that time the opportunity of operating on the New York elevated roads seemed very remote; in fact it seemed a question which it was idle to discuss—just as now it seems idle to discuss the probability of running the New York Central road by electricity. But I think that we have done something more than "mark time" in the four or five years which have elapsed since I saw Mr. Daft make those experiments; for we are now operating a great many miles of track in this country with electric motors. It is true that we are not operating on the elevated system, but I believe that the day is not very far distant when the elevated roads will certainly be placed at our disposal, to do our worst with, and I have no doubt that Mr. Daft will bear his share in the honor and glory of that time. I think to New Yorkers it will be a very pleasant and agreeable time when we shall have dispensed not only with steam, but with the horses which now afflict us in this city. (Applause.) It may be a little bit poetical, but as I sat and listened to the horses out there in the street it seemed to me they were doing their best to drown Mr. Daft's voice (Laughter), and to prevent us realizing the full scope and importance of the remarkable tribute which he was laying before us. I had the misfortune two or three years ago to hire a flat near the track of the elevated road. It was up town, in the neighborhood of Central Park, and it seemed to me very desirable. I hired it in winter time when the windows were closed, but when the summer came I realized that the situation was not as desirable as it seemed in winter; and I had opportunities of studying very closely and very exhaustively the effect of steam upon the elevated roads, and I satisfied myself, as any one who travels on the road must be satisfied, that in electricity alone are we to find the solution of our difficulties and troubles; and in electricity alone shall we find such a system as is worthy of the city of New York.

Mr. Curtis:—Before you make your motion, I beg to say a few words. I did not mean to imply that there was anything which Mr. Daft might have added to his paper, germane to the discussion, which he left out. I simply thought that, as a very interesting feature connected with his experiments, the counter-electromotive force might show something as to general efficiency.

Mr. Daft:—I am quite sure that Mr. Curtis would not say anything that was discourteous, and I did not so interpret anything that he said.

Mr. Martin:—I simply wished to express our appreciation of what we have had, in as concrete form as possible; and therefore I move that our thanks be presented to Mr. Daft for his interesting paper.

The Chairman:—It gives me great pleasure to put the motion. I think we owe our thanks to Mr. Daft and to others, who offer us the results of work done, as in this case. This represents, as you see, a work extending over months, and the expenditure of much money, and the results are offered to us in a form which will be useful; giving us many important facts, and carrying us to a higher plane of knowledge.

The motion of thanks was unanimously adopted; and the meeting then adjourned.

A paper read before the 37th meeting of the American Institute of Electrical Engineers, New York, September 10, 1889, and discussion thereon, Vice-President T. C. Martin in the Chair.

ALTERNATING CURRENT MOTORS: THE EVOLUTION OF A NEW TYPE.

BY LIEUT. F. JARVIS PATTEN.

On giving our secretary the title of this paper it was my purpose to treat the subject in a broader light, and to show the progressive steps in a series of experiments which led eventually to the type of machine that I shall bring to your notice this evening. The recent and urgent claims of other work have rendered such a treatment impossible at present, and I shall limit the paper to a description of a new alternating current motor, one form of which is shown in the accompanying drawings.

The place that the alternating current electric motor is destined to fill in the industrial arts is familiar to you all, and the various ways known to the scientific world by which such machines may be rendered operative have been ably considered and elaborately discussed in the Institute papers of the past two years by Prof. Thomson, Dr. Duncan and Mr. Tesla. We are thus all more or less acquainted with the prominent difficulties of the problem.

My experience, in common with that of my predecessors, teaches that the alternating current motor has a strong and persistent disposition to stand still, and when persuaded to motion is apt to be a sort of "go as you please" machine and asserts its inherent right to turn in either direction indifferently, direction of rotation in some cases being purely a matter of chance. I shall not have much to say about efficiency, as my experiments with large machines are not sufficiently advanced to furnish any reliable data, but I will endeavor to give a general solution of the problem designed to meet the following conditions of practice:

1st. A machine that will start itself independently of the speed of the generator or number of alternations of current per unit of time.

2d. A machine that has but one direction of rotation and cannot reverse under any conditions of current alternation.

3d. A machine that is not necessarily synchronous with the generator, revolution for revolution.

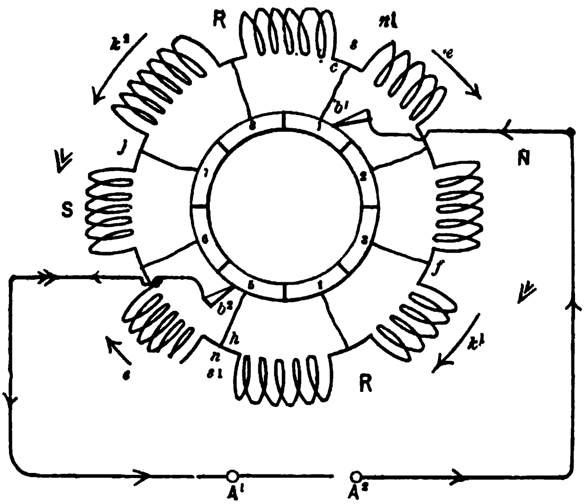


Fig. 1.

4th. A machine in which reversals of current direction do not produce corresponding reversals of magnetism in any iron part, when the machine is in motion at its normal speed and maximum efficiency.

5th. A machine of simple form having an ordinary continuous wound armature revolving in a single or two pole field.

Referring now to the figures which are simply diagrams of the circuits and operative parts we have in Fig. 1 an ordinary closed circuit armature shown as a Gramme ring merely for convenience of illustration, the points of the winding intermediate between the eight coils being connected in the usual way to the eight segments or bars of an ordinary Gramme collector, and it may be well in passing to note here the functions of this collector.

It is not a commutator in the strict sense of that term as it does not rectify or redirect reverse or opposed currents. If the brushes b^1 and b^2 were held upon the outside wire of the ring, the same results would follow, and the Gramme collector properly so called simply transfers the brush contact from point to point of the ring winding. If a source of direct current be interposed between the terminals A^1 and A^2, current will flow continuously from brush b^1 to b^2 downward through the right and left hand halves of the ring in the direction of the arrows k^1 and k^2, making say a south pole in the ring at the upper point s, and a north pole at the lower point n. With a continuous current these poles would be continuously maintained and placed in the magnetic field indicated by N and S the armature would revolve in the direction indicated by the arrows E, E. If now a source of alternating current be inserted between the terminals A^1 and A^2 the polarities of the ring would be reversed at each successive alternation of current, and if a direct impulse indicated by the single arrows in the external circuit produced the poles n and s in the ring, the reverse impulse indicated by the double arrows would produce the opposite polarities n^1 and s^1 at the same points, and the tendency to motion would be reversed if the fields remained the same, but it will be noted the motion would be in the same direction still, if the fields were also reversed by the same reversal of current. If, however, the fields were maintained constant as indicated by the large letters N and S and some device could be contrived by which at each reversal of the alternating current, the brushes b^1 and b^2 could be made to change position, either mechanically or otherwise, then with an alternating current a constant polarity s and n would be maintained at the upper and lower points of the ring—for then a direct impulse starting from A^1 to the right would enter the ring through the brush b^1 flowing down both sides in the direction of the arrows k^1 and k^2 out through brush b^2 and back to the source at A^1. The reverse impulse indicated by the double arrows would start from A^1 to the left, going to brush b^2; and if we now suppose this to have changed places with the brush b^1 the reverse current would then enter the ring at s and flowing down both sides in the direction of the arrows k^1 and k^2 would leave through n and the brush b^1, and so back to the source a^2 maintaining the polarity of the ring the same as before. If the brushes could be thus changed at each alternation, the polarity of the ring would be maintained constant with an alternating current. While, however, it is quite impracticable to thus cause the brushes to change position mechanically at each reversal of current, it is perfectly feasible to produce the same effect without the mechanical change. The means of accomplishing this result are indicated in Fig. 2, which is identical with Fig. 1 with a single exception. There are eight coils as before, and eight bars in the collector; the odd number bars 1, 3, 5 and 7 are connected to the same points of the ring as before *c*, *f*, *h* and *j*, but the even numbered bars 2, 4, 6, 8 are connected respectively to points of the ring diametrically opposite them, bar 2 to the point *d*, bar 4 to the point *g*, and so on each even numbered bar to a correspondingly opposite point

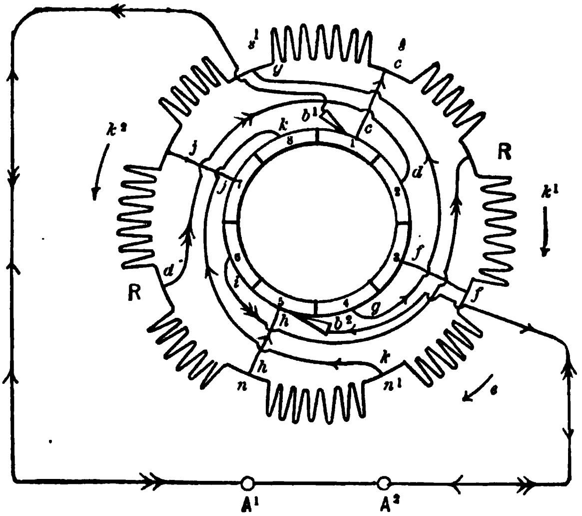


Fig. 2.

of the winding. If now a source of alternating current be interposed between the terminals A^1 and A^2 and we make the single supposition that the ring shall turn through an arc of the circumference equal to that covered by one bar of the collector during each alternation of current, we shall still maintain a constant polarity at the upper and lower points of the ring without causing the brushes to change position mechanically.

Thus a positive impulse starting from A^1 to the left and indicated by single arrows enters the ring at *b*^{1} flows down both sides to *n* producing the ring polarities *s* and *n*, out brush *b*^{2} and back to source at A^2. The reverse impulse being in the opposite direction will start from A^2 to the right, go to brush b^2, which we will now suppose bearing on segment 4 of the collector, whence it will go by the inverse connection to the opposite point B^1 of the ring, then down both sides in the same direction as before to the point n^1 thence back to the opposite segment 8 out brush b now bearing on this segment, and back to source at A^1.

The reverse currents therefore under the assumed conditions are caused to maintain a constant polarity in the ring so that in a constant field its tendency to motion would always be in the same direction with an alternating current in the armature. It will be further noted that the alternating current is not redirected or commuted in the strict sense of the word, and we may enunciate the fundamental principle which underlies the construction of this type of machine as follows:

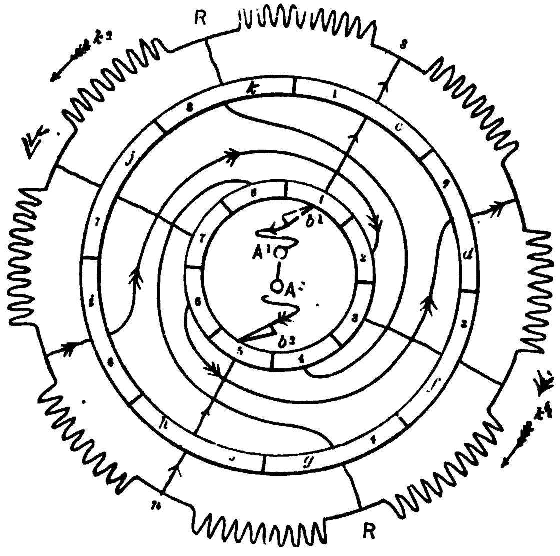


Fig. 8.

The poles of any closed circuit may be maintained constant with an alternating current by causing opposite impulses to traverse the circuit in opposite directions. The direct and inverse connections shown in Fig. 2 have precisely this effect, when, as supposed, a single bar of the collector passes under the brushes at each reversal of current. The connections *c c*, *d d*, *f f*, etc. in Fig. 2 may have any form, and other bars may be interposed between their extremities without affecting in any way their functions as connectors. This step is shown in Fig. 3

where another collector bar 1*c*, 2*d*, 3*f*, etc., is inserted in each of the connections *c c*, *d d*, etc., of Fig. 3, thus making another collector shown outside the first to avoid confusion of the drawing, while for the same reason the source of alternating current A^1 A^2 is placed inside the inner ring. The polarities *s* and *n* of the ring being maintained constant, as previously described, with an alternating current, and as current is constant in direction from *s* downward through the right and left hand halves of the ring to *n*, so must necessarily any current be constant in direction which is led from brushes through any other circuit connected to the segments 1*c* and 5*h* of the outer collector; a field circuit of constant direction may therefore be shunted from this outside collector. This is shown in Fig. 4, in which twelve coils are shown in the ring and twelve bars in each collector connected alternately direct and inverse as before. Tracing now two opposite impulses of current we have the first indicated by the single arrows from source A^1 to segment 1 of the inner collector, thence to segment 1 of outer collector, where the current divides, part going down the right and left hand halves of the ring to n^1 and part out brush b^3 through the field circuit, making the poles N and S back to brush b^4 segment 7 of outer and segment 7 of inner collector to the terminal A^2 of source. If the armature be supposed now to turn through the space covered by one col-

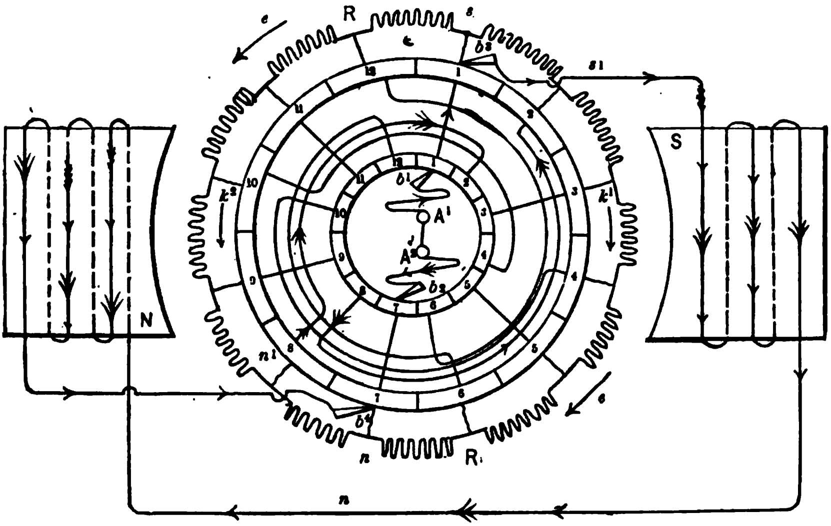


Fig. 4.

lector bar the reverse impulse can be traced as follows: Starting at A^2 in the opposite direction to brush b^2 now bearing on segment 8 of the inner collector thence through the reverse connection to segment 2 of the outer collector, now under brush b^3 where the current divides, going part as before down the right and left halves of the ring, making a south pole at s^1 and a north pole at n^1 as before, and the other part out of brush b^3 through the field circuit in the same direction as before, back to brush b^4 now on segment 8 of the outer ring, thence through the reverse connection back to segment 2 of the inner ring, now in bearing with brush b^1 and so returning to the source at A^1. Thus the two impulses of opposite direction have been made to transverse both armature and field circuits in the same direction. We have therefore with an alternating current constant armature polarity and constant field polarity, and therefore a constant tendency to motion in one direction. Not only this but the further condition is fulfilled that there are no reversals of magnetism in any iron part so long as one collector bar passes under the brushes at each alternation of current.

It remains to show how this is brought about. Referring again to Fig. 4, let it be supposed that the first impulse of current did not cause the armature to turn through the arc of the circumference subtended by one segment, but all the brushes still bore on the same segments as shown in the figure and the reversals of current continued. By tracing the circuits it will be seen that each reversal of current reverses the polarity of both field and armature and with either direction of current or rapid reversals, there will be a constant tendency to motion always exerted in the same direction. The machine under these conditions becomes therefore simply a direct current machine on an alternating current circuit with a constant tendency to start in one direction. Assuming the machine therefore self-starting, it will continually gain in speed until the condition is fulfilled of one segment passing the brushes at each alternation, for it then becomes in the broad sense a synchronous alternating motor, the current then produces no reversals of magnetism, and there is a true alternating current in the armature circuit, producing, however, no reversal of armature polarity; and a current of constant direction in the field. Under these conditions the motor is self-regulating, moving at a constant speed and with a maximum rotary effort.

It is not, however, essential that one bar should pass the brush at each alternation, as any number may be caused to do this, depending upon the speed required and the number of coils upon the armature. This is illustrated in Fig. 5, where the complete machine is shown. There are twenty-four coils in the armature, twenty-four bars in the outside collector and thirty-two bars in

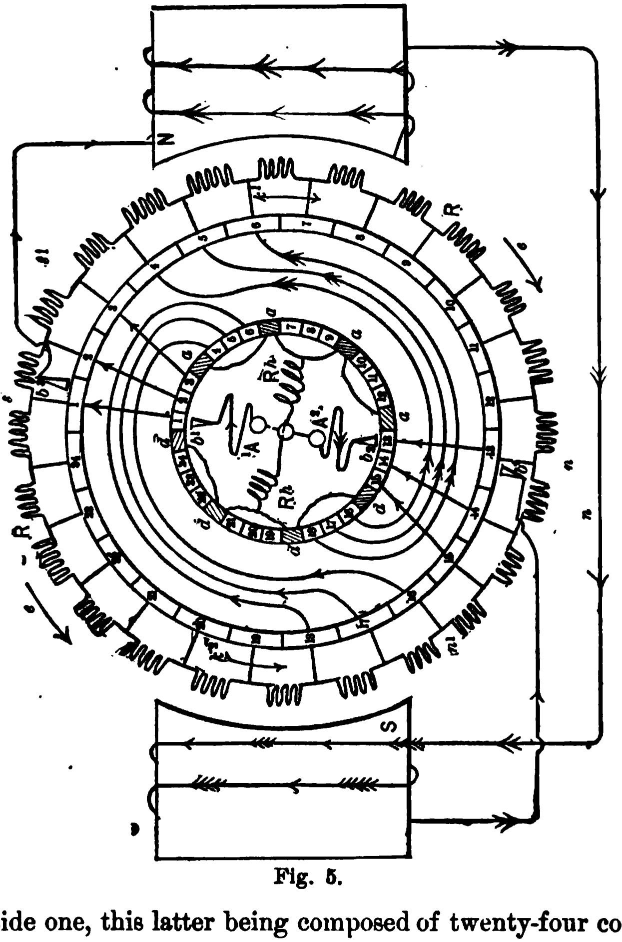


Fig. 5.

the inside one, this latter being composed of twenty-four connecting and eight insulating bars. The connecting bars of the inner ring are numbered to correspond with those of the outer ring around to the right from one to twenty-four:—the insulating bars drawn shaded, separate the others into groups of three. In this machine three segments, 1, 2, 3, in the outer ring, are connected direct to the corresponding segments, 1, 2, 3, of the inner ring, likewise the opposite three, 13, 14, 15, of one ring, are connected direct to 13, 14, 15 of the other. The next group of three is connected inversely, 4, 5, 6 of the outer ring, to the diametrically opposite bars, 16, 17 and 18 of the inner ring, and the corresponding opposite group, 16, 17, 18, of the outer ring, is likewise connected inversely to the diametrically opposite group, 4, 5 and 6 of the inner ring. The remaining segments are connected in the same manner, but the connections are omitted to avoid confusion of the drawing. The operation of the machine is evidently the same as that shown in Fig. 4, except that the required conditions are fulfilled in this instance when three bars of the collector pass under the ring at each alternation of current, and

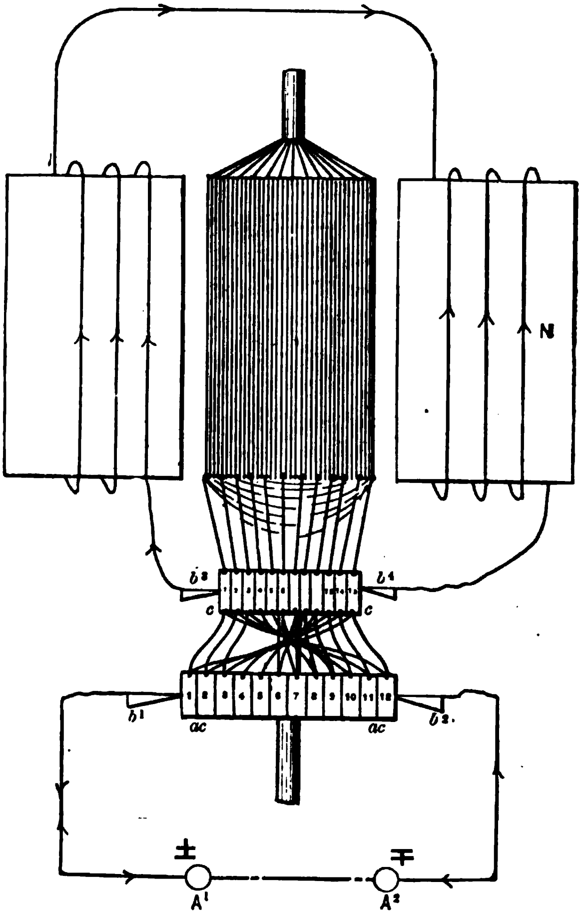


Fig. 6.

as there are twenty-four segments arranged in groups of three, the machine at its normal speed would make one revolution for every eight alternations of current, and connected in a circuit supplied with 16,000 reversals per minute, its normal speed would be $\frac{16000}{8} = 2000$ per minute, and with 48 segments arranged in groups of three its speed would be 1000 per minute. The blank segments separating the groups of the inner ring are connected to the extremities of a rheostat *Rh. Rh.* which is inclosed inside the commutator and is designed to offer a path for the alternating current such as there may be and prevent its absolute rupture at the period of change from one group of segments to the next; they also serve an important purpose in preventing a dangerous short circuit which would be occasioned by the inner brush bridging two groups of segments oppositely connected.

It follows as a matter of course that as the machine starts as a direct current motor connected in an alternating circuit, rapid reversals of magnetism will at first be produced in all the iron cores and these should be made of laminated iron to prevent undue loss by heating at the period of starting. The machine in fact starts as a direct current motor and automatically changes at a certain speed to a sort of synchronously alternating motor. It reaches its normal speed at this point, is then self-regulating, and its capacity of doing work is a maximum.

Fig. 6 shows a plan of the machine as constructed; it consists simply of an ordinary closed circuit armature in a single field; *c c* is the ordinary collector really a part of the armature circuit from which the brushes b^3 b^4 take a current of constant direction to the field shunt; *a c*, *a c* is the reversely connected commutator and the brushes b^1 b^2 bearing upon this commutator are connected to the terminals of the alternating current circuit.

DISCUSSION.

LIEUTENANT PATTEN, continuing orally:—I might add to the paper the fact that the inner commutator, the one which is the ordinary commutator of the Gramme ring winding, does not perform the ordinary functions, as the brushes bearing on this collector require no adjustment, it being merely a rubbing contact, from which the shunt field current is taken as a derivation to the main circuit of the machine. With that exception, I think the machine is fully explained in the paper.

Mr. C. O. Mailloux:—I would like to ask Lieutenant Patten whether sparking occurs to an injurious extent or not.

Lieutenant Patten:—The sparking being due to the electromotive force at the point of breaking, it will necessarily occur during the starting of such a machine and more than ordinarily when it is out of synchronism with the alternating current. At the outset, it can be easily seen that the alternating current is liable to be broken at a point where the electromotive force is high, and therefore the sparking would be very violent. I have endeavored to obviate the trouble due to that cause, by the device shown in Fig. 5. If you will notice the circuits carefully, it will be seen that the circuit of the alternating current is never broken. If the brush comes on to one of the dead segments or shaded segments connected with the rheostat before the alternating wave has come to zero, it finds a path which is at first in parallel with the machine circuits and then becomes a separate path for the alternating current during its change, whether this change begins at a high or at a low electromotive force. Of course, as the machine starts, this may happen at any period of the wave, and the brushes passing over these segments connect with the resistance at the end of the group, and before it has left the group the current is already short circuited through this rheostat. So that the alternating current always has a path and the sparking will not be excessive, judging from what little experience I have had.

Mr. R. S. Dobbie:—I would like to inquire whether the field magnets are laminated, and whether the lamination would be necessary only while the machine was starting up as an alternating motor or before the synchronism is attained. I would also like to ask if the maximum torque of a machine is at one-third speed, and how that compares with the torque of the same machine at full speed when the synchronism is attained.

Lieutenant Patten:—In answer to the first point I would say that there is no essential reason for laminating the field magnets for any other purpose, but it has been shown by quite conclusive experiments that a laminated machine has a very definite neutral line of field, which makes it better for taking the direct current to feed the field, and I avoid the necessity of having two sets of movable brushes. With respect to the other point, the torque of such a machine depends of course, entirely on its keeping in synchronism, and the result of the departure from that point is well known to all. That is the limit of the strain that can be exerted. When that is exerted the machine will stop; it will not then continue as a direct current machine.

Mr. Dobbie:—My reason for asking that question was to find out if the motor will start on a full load.

Lieutenant Patten:—No; it will not.

Mr. Lemuel Wm. Serrell:—I would like to ask how the currents are short-circuited through the rheostat when the ends of the rheostat are connected to the insulated blocks in the commutator.

Lieutenant Patten:—The question is a pertinent one inasmuch as the term "insulator" was used in the paper. They are not insulated segments in the true sense of the word. They are all connected with each other and are of conducting material. The mistake is my own entirely in using the word "insulated" in any sense. The insulating segments referred to are the same kind of segments as the others. They are simply other segments not connected to the machine circuits and a little narrower than the others.

Mr. Serrell:—The shaded portions, as I understand, are insulated blocks, but as is now explained, the shaded portion indicates a metallic connection separated from the rest of the commutator.

Lieutenant Patten:—No; they are simply segments, the same as the others, but are connected to an interior rheostat, which is not in the circuit of the machine. I think, as has just been suggested to me, that they should be called "isolating" segments instead of insulating segments. They are segments not connected with the circuit of the machine, but simply to the rheostat, which is shown in the drawing, and this rheostat is intended to take the current at the time the brush leaves the group which is connected with the machine circuits and so prevent sparking.

Mr. H. Ward Leonard:—I would like to inquire whether the interruption of the current in the field is going to occasion any heating of the field or loss of efficiency.

Lieutenant Patten:—I might say in reply to that and answer at the same time a good many questions that might be advanced of a kindred nature, that my experiments have been very limited and that it would require considerable investigation to determine that point definitely. I have no doubt that it does limit the action of the field, but to what degree I do not know. But the current is broken when the machine is in synchronism at a point near its zero value. Of course, at 16,000 alternations per minute the interruptions are so short that the field could not readily lose its magnetism, but whenever there is a break of the current there must be some slight loss due to that.

Dr. Allen V. Garratt:—Another question in regard to the rheostat. Is the resistance of those coils gotten at by experiments, and if so, what relation do they bear to the resistance of the machine itself?

Lieutenant Patten:—So far, I have attempted to make the rheostat simply equal to the resistance of the machine, so that there would be as little change in the strength of the current as possible.

Mr. Mailloux:—Is the resistance re-actionary or simply dead?

Lieutenant Patten:—So far the resistance in the commutator is a dead resistance simply. It might theoretically be made to advantage of the re-active kind.

Mr. Mailloux:—I should think that a machine of this kind, although it is of great theoretical interest and is an evidence of striking ingenuity, would be open to a defect arising from the fact that the current is a wavy one, no matter how rapid the periodicity is. There is always a very short time when there is no current. We may consider that the magnetizing current through the shunt is a succession of waves. Consequently there would be a tendency for the magnetism to rise and fall. If the magnetic core is laminated and of soft iron, I think that it will be able to rise and fall quite rapidly enough to follow the fluctuations of the current. We know that it does so in transformers Of course there is a slight reaction in the substance of the iron itself—its "viscosity," due to hysteresis; at the same time it is not sufficiently great to interfere with the rise and fall of the magnetism, and the more perfectly the core is laminated the more rapidly the rise and fall would take place. It seems to me there would be quite a material loss under certain conditions, and also a loss due to the reactions following from mutual induction in the armature itself, because even when running at synchronism the current in the armature would be a succession of rises and falls, and although the magnetism of the armature is not reversed, yet the current is reversed through the coils, and is constantly rising and falling at a very rapid rate. I should think that the loss due to mutual induction, particularly in the presence of a magnetic body, such as the armature core, would lead to a very important source of loss, and I should not expect that a motor built on that principle would show the efficiency of other forms, for instance, as those suggested by Leblanc in France, and Mr. Mordey in England.

Lieutenant Patten:—I have only to say in reply to that, that there is one evident misconception about the direction of current in the armature circuit. It is always the same. As to what has been said with reference to the induction due to the rise and fall of current, such loss is contemplated, and it is difficult to make out of an alternating current anything that does not vary. The essential fact, however, is that the current at work is due not only to the maximum and zero values of the electromotive force exerted, but to the current which is the result of the mean electric force. I do not recall any machine described by Leblanc, except those that he has referred to as Mr. Tesla's and Prof. Thomson's earlier form of motor. Beyond them I do not recall any machine in which these difficulties are not equally present.

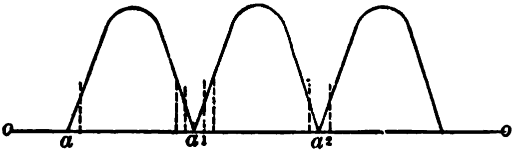


Fig. 7.

Mr. E. P. Clark:—I do not think that the rise and fall hat very much influence, because when the armature is revolving as a high rate of speed it is the same as a continuous current dynamo. As long as the speed is constant the current will be very nearly the same. There will be very little rise and fall to it, due to the rise and fall of the alternating current.

Lieutenant Patten:—The current in the field will necessarily be a re-directed current. It would be represented graphically in the form I have drawn on the board (Fig. 7). The commutated current at work, or the electromotive force, to speak more strictly, would be cut off at the point *a*, we will say, if the machine is in synchronism at points a^1 and a^2, the interval would be a period of no current. The loss spoken of by Mr. Mailloux will exist during such a slight period of no current whatever, as occurs at this point. I think that a solid field would not probably give up its magnetism during that short period of time. A closely laminated field might do so.

Mr. Leonard:—I should like to ask if, when the load is removed, there would be a tendency to race?

Lieutenant Patten:—The limit of load in such machines is that which is required to throw it out by a full quarter of a period, and this is reduced to a quarter of a group of segments. If the load is lifted off it cannot, unless the conditions of current change suddenly, go beyond its ordinary synchronous rate.

Mr. Leonard:—Can it race?

Lieutenant Patten:—I think not. The tendency of course is, if it races, to go to the double synchronism. It has to go to the double speed or the quadruple, or else at the single speed at which it is synchronous.

Mr. Mailloux:—I do not think that Lieut. Patten has made quite clear the influence at work to maintain the speed constant when the load is increased or decreased—the exact action which takes place by which the torque is changed.

Lieutenant Patten: —The action of this machine is not different from that of an ordinary alternating current motor, where its maximum electromotive force is exerted when it is in synchronism within a quarter period of that of the generator. If it is overloaded beyond that point it comes to a state of rest or short circuits the armature and it burns up.

Mr. Edward P. Thompson:—The question has occurred to me whether this new type of motor is also a dynamo, and what kind of current would be generated by it?

Lieutenant Patten:—I have never driven the machine backwards, but I discussed this question with Mr. Seely in the Patent Office, and it was first supposed by him that the machine was not a "dynamo and electric motor" as I had termed it in the specification. But we finally came to the conclusion that it was. The chief objection is that a self-exciting dynamo must have a continuous current to excite its field, and it must be an increasing current. The unfortunate thing about this machine is that it has not an increasing current. The question arises as to whether there would be a sufficient continuity of current in the machine with this short circuit to make it a dynamo. I have not driven it backwards, and the theoretical result can be arrived at as readily by my associates as by myself. I think it would be a dynamo, but of low efficiency.

Mr. Leonard:—I should like to ask Lieutenant Patten if he would kindly give us some description of the performance and sizes of the machines that he has made thus far.

Lieutenant Patten:—Those I have made are too small to give any data which would indicate any efficiency worth talking about.

Mr. Leonard:—What voltage did you work upon?

Lieutenant Patten:—100.

The Chairman:—There are several points in this discussion which should be elucidated, and if any gentlemen has questions to ask, or theories to ventilate, or ingenious motors of his own to describe, I trust he will have no reluctance in doing so. There are several gentlemen here whom we all know to be interested in the subject; I do not think that they should make it requisite for me to call upon them. We would be very glad to hear from them, and although some of them are here with us for the first time, I do not think that they need have any hesitancy in letting us hear from them.

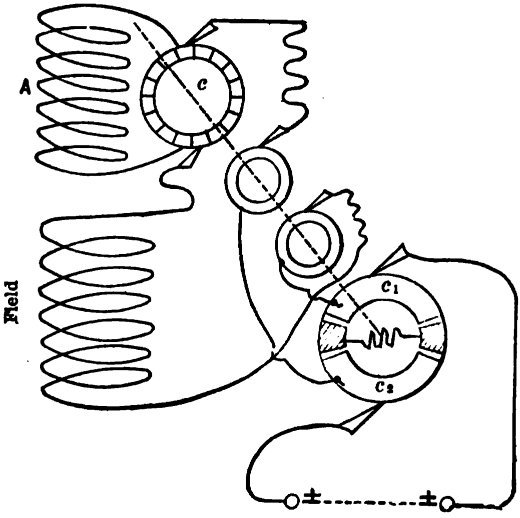


Fig. 8.

Lieutenant Patten:—In the meantime I will draw the Mordey machine on the board and show the difference. (Fig. 8 was placed on the board.) The device for rectification is fixed on the spindle of the moving machine. The dotted line represents the spindle of the machine. It is the commutator of an ordinary armature which is shown here. This being the armature circuit we will trace it simply as a closed coil from the upper point to the opposite point of the commutator and thence to the field. On the same spindle with this armature is placed another commutator, which is shown here in two parts. The upper and the lower halves are separated by isolated segments connected to a rheostat such as I have described this evening. It is shown this way, but in reality there are for each pair of segments a corresponding pair of isolating segments connected with the rheostat, so that after the brush passes one segment it connects with the rheostat, and so on. At the bottom is the source of the alternating current. These terminals go to the two brushes; bearing on the rectifying commutator which is broken up into alternating segments, part of them connected and part isolated. The live segments are connected alternately to two rings, say one half to the segments of one ring and the other half to the other ring. Bearing on these rings are two brushes used as intermediate connections. Evidently this machine requires a speed of rotation such that the alternating current coming in at these two terminals will be commuted; then a direct current will flow from the brushes, bearing on the continuous rings, and from these brushes we have a direct current through the armature and field in series, back to the outer ring and so returning to the source of the alternating current. This is spoken of by Leblanc as one solution of alternating current motors. It is not an alternating current motor in the strict sense of the word, because the alternating current is commuted before it is put into the machine. It is open to the same objection, of course, that the alternating current is broken at the point where it passes zero value, and the same remark applies to the field.

Mr. W. Forman Collins:—When you break the circuit there, the field will lose its magnetism. Will not the armature act as a short circuit across the field.

Lieutenant Patten:—If the machine is in synchronism the current at this point is near zero, and the short circuit is then of little consequence.

Mr. Collins:—It would induce very heavy sparking, would it not?

Lieutenant Patten:—It would if it were not for the isolated segments, on which the brushes then bear, and which offer a path for the current and so prevent its rupture.

Mr. Collins:—Provided the field maintains some litttle magnetism.

Lieutenant Patten:—Yes; if it gives it up entirely; of course the reactionary effect would produce sparking.

Mr. Dugald C. Jackson:—I should like to ask if all these schemes are not beating around the bush to get a continuous current motor, and if the size of the machine when put on practical circuits is not prohibitive, and that consequently we cannot with any of these devices get an alternating current motor that will do the work on practical circuits economically.

Lieutenant Patten:—If we accept the gentleman's remarks as conclusive that would settle the matter. I shall have to refer you to future generations for your answer.

Mr. Charles J. Reed:—I would like to ask one more question in regard to the action of these waves. As the potential varies from maximum to zero, will not the iron completely lose its magnetism each time as the potential falls to zero. I confess I am not posted on the rate of demagnetization of iron, but I am informed that in an ordinary converter the iron will actually become demagnetized something like twenty thousand times a minute. I would like to get some information on that. If that is so, and you use a current having a less number of reversals than that, would not the magnetism actually become zero between each of those successive impulses or waves?

Lieutenant Patten:—I would suggest that you call upon others to answer these question of general information. I would simply say in reply, that so far as my experience is concerned I have seen magnets that change their magnetism as slowly as fifteen times a second and retain enough to act as small dynamos; that is very slow as compared with 133 reversals which are used in this country, and 50 on the other side. But there undoubtedly is an approach to zero in the magnetic field, and there is at the same time a period of zero current in the armature. You can have an iron field that will not give up its magnetism in the 133d part of a second, and you can have one that will give it up in much less time, and you can have one that will retain it one-fifteenth of a second.

Mr. Reed:—Would not your laminated field be an objection in that it would tend to have the magnetism actually become zero at each reversal?

Mr. Mailloux:—I think we can throw some light on this point. I do not think that the magnetism would necessarily come to zero, for the reason that we are not dealing with a simple magnetic circuit. If we were dealing with a simple magnetic circuit in which there is no reaction against the lines of force in collapsing then you would doubtless get a very quick collapse of the magnetism, and possibly absolute zero of magnetism in the interval between the two pulsations. You might explain this by taking an ordinary magnetic circuit—a ring for instance—with a magnetizing coil, the ring consisting of very finely divided wire. Now, if there is no reaction (Foucault currents, etc.) against the flow of lines of force; if we assume the ring to be fully magnetized and the magnetism suddenly released, and if we even eliminate the self-induction of the magnetizing coil itself, then we will readily see that the magnetism will fall almost instantly. There are experiments which go to show that the fall of magnetism would take place in an inconceivably small space of time under such circumstances. But if we interpose in the path of the lines of force as they move, while rising or falling, a closed circuit or some other form that would tend to produce reaction, then we get what we call "Lenz effects," and in that case we cannot get such a rapid rise and fall. Now, in this machine the armature itself, the core and conductors are in the magnetic circuit; they form a part of it. Consequently at the time the magnetism is falling or rising there would have to be considered the reaction due to the conductors wound around the magnetic field itself, and also the conductors wound around the armature, and the currents induced in these conductors by either the rise or fall would necessarily tend to retard both the magnetizing and the demagnetizing process, and I should think that it would be possible to so regulate the self-induction of the different portions of the circuit as to control that. I at first thought that the demagnetization would take place very rapidly. I still believe that there will be a great fluctuation, but it would probably come considerably short of absolute zero. There would be a certain amount of residual magnetism during the intervals between the current waves.

Dr. Garratt:—On the general principle that sometimes "fools rush in where angels fear to tread," I would say that I confess that it does not seem to me that this question of the fall of magnetism of the field would be such a very serious one, although in this case I am speaking not at all from experience, never having wound a machine of that character. But if I understood the gentleman correctly, the only reason why he laminates his field is to prevent undue heating at the starting of the machine. We have spoken of lamination as though it were a fixed thing. It may mean breaking up the core into two or three pieces or into a million. I suppose that it would be simply necessary to so proportion the laminations of the field that its magnetization would not fall unduly and so that in starting the machine the field magnets would not get unduly heated. It seems to me that it could be got at empirically in the natural process of building these machines of various sizes, and that it cannot be got at by pure inductive reasoning.

MR. E. T. BIRDSALL :—I think one point has been overlooked and that is, the action of different sizes of the machine. In small machines, we know that in breaking the magnetic circuit, the magnet loses its power very quickly. In large machines of twenty or thirty horse-power, it is known that magnets will not lose their magnetism for from two to three seconds, where the cores have a large amount of iron in them. In that case we might arrive at a mean degree of lamination, which would not produce too much heating. We might arrive at a size which would be a mean size of machine and which would give the best effect. It also struck me in regard to the magnetism falling, that possibly, although I have not thought it out, it does not matter so much whether the magnetism does fall, because everything else is zero at the same time the magnetism is zero.

THE CHAIRMAN :—There is one gentleman with us this evening who has had considerable experience in the manufacture of converters and also in their use, who, I think, could enlighten us on two or three of the points which have just been raised. I refer to Mr. Gutmann, I think he might favor us with one or two practical remarks on the subject.

MR. LUDWIG GUTMANN :—I think the motor just described is a very ingenious one, but we cannot discuss the motor itself as it stands alone. We have, of course, to consider it in connection with the circuit, because we want to transmit power. Synchronous motors generally can not be thoroughly successful because they will not answer all purposes. The same speed is not required in all cases. This motor, of course, is a step forward, because it can be so arranged as to permit the introduction of motors of different speeds on the same circuit. Nevertheless the motor is of the constant speed type, while in practice a variable speed is often desirable. I see one serious objection to the motor, and that is, that it has the high potential current in the motor armature itself, and in case of any trouble arising, the consumer who has to handle it is immediately exposed to the danger of the medium or high-tension current in the main wires. That is the most serious trouble that I find with it. Otherwise I believe it would answer many purposes, because I think the motor will do its work well. It appears the motor would not start from a state of rest with any load on it and it would need a loose pulley so as to get up to synchronism before the load could be thrown on. This of course, would be a weakness, but not fatal; but I do think it serious that the machine requires a high potential. I do not think that this motor can be worked with low tension currents, because, especially to produce three or four horse-power, or more, the self-induction in those coils in connection with the mass of iron is great. If such a motor should be placed on an ordinary alternating current having about 1,000 or 1,500 volts, and we should always have to send practical and experienced men to repair the brushes, etc., because they are exposed to the 1,000 volt currents, and on this ground I do not think that it would be a solution of the problem, although a decided step in that direction.

Lieutenant Patten:—With reference to the high potential of the machine, of course in the general description I have given I have represented any source of alternating current of high or low potential, and so no special reference is made to its use as a high potential machine, on the direct mains from the station. The source of alternating current is presumably one suited to the conditions. With reference to the further difficulty mentioned, accidents will happen with any kind of machine, either direct or alternating, while the other forms of motor have some advantage in having ring contacts and sliding brushes that require no attention. The disadvantages that are introduced by bringing in the use of brushes are incident as well to the ordinary forms of motors. We are not so familiar with the kind that have no commutators that we have got used to their ease and simplicity of working. The second set of brushes are not, as I said before, brushes in the ordinary sense, and do not require adjustment and care. If a high potential machine is used, of course the ordinary precautions will have to be taken. It is not quite within the province of the alternating current motor to make the alternating current safe for everybody who uses it.

Mr. Gutmann:—I wish to point out that alternating current motors have this drawback, that the coils surrounding the iron core need a certain pressure to make any current flow through those coils—to get any power out of the motor we have to force the current through. Now, to produce five or ten horse power we need a certain number of ampere turns in the field. To have this certain number of ampere turns we must have a considerably higher pressure than might be supposed. We must have an armature to perform the work and to obtain the torque. Consequently I think it is impossible to realize a large amount of power without using medium or high tension currents for alternating current motors. Another point is, that we have to consider that alternating current motors will not always be placed in the hands of scientists, but will have to be used by inexperienced men, consequently I think this is a weakness in the motor if we have loose or sliding connections in a high potential circuit. I should prefer to have a motor, for instance, like Prof. Thomson's, in which the high tension current is fixed—no rubbing contact, and Mr. Tesla has shown us a motor without any commutator or brushes, and I believe there are many electricians hard at work to get something similar without a commutator, which is far preferable, but we will have to wait some time before we shall get that; and Thomson, for instance, has given us a motor in which the field alone is in the high tension circuit. This is the main drawback that I find to this motor. If we had no rubbing contact in these motors it would be a great advantage. As it stands, the armature is in series, or is in connection with the field, and consequently we have an increased self-induction, and we need a greater power and greater tension for this class of motor than we would if we had only to energize one of the two parts, either the armature or the field with the current, and that is the reason I said that there is a serious difficulty in introducing it into ordinary use.

The Chairman:—If there is any gentleman here who is not a member, but who wishes to speak, we should be glad to hear from him. I noticed in the course of discussion that the criticisms so far on this motor have come from those who are partial to the alternating current motor, and may be said to be working in that field. But it seems to me that there are one or two criticisms or objections to be advanced from the continuous current side of the house, and those objections have been hinted at by one gentleman on my left. With regard to the shape and size and weight of the alternating current motor as compared with others, it seems to me that there are one or two material and pertinent points to be dealt with. Dr. Wheeler perhaps might touch on that. We should also be very glad to hear from Mr. Blake.

Dr. Wheeler:—You are very kind, but I have not heard all of the paper and I am not prepared to speak on it.

The Chairman:—Haven't you read it?

Dr. Wheeler:—I read it hastily. (Laughter.)

The Chairman:—There being no other speaker, we should be glad to hear from Lieutenant Patten in reply and in conclusion, though I think his replies have already been numerous and in most cases almost conclusive.

Lieutenant Patten:—I think the alternating current motor might be left for the present. As to making a rival to the direct current motor, no attempt is made in that direction. The object is not to make a machine which has less weight and more efficiency at the outset than one that has been improved by the ablest talent of the world for the last five or six years. The question is, whether or not we can make an alternating current motor at all. If we could, the question might be put, what are you going to do with your current ten miles from here if you cannot get an alternating current motor? I think if the efficiency of such a machine drops to sixty per cent. it would not be a vital question in the problem with reference to the great importance of the transmission of power to great distances.

The Chairman:—I am sure it has been a great pleasure to all of us this evening to learn from Lieutenant Patten how great and how rapid has been the improvement in the direction of giving us a practical and useful alternating current motor.

I now have much pleasure in announcing that for our October meeting we shall have a paper by Mr. Thomas D. Lockwood, a member of our Board of Managers, who has recently returned from a prolonged trip through Europe, and has made a great many observations on electrical development there, and who proposes to lay before us the pith of his notes on the subject. The paper will be read on October the 15th.

Adjourned.

A paper read before the 39th meeting of the American Institute of Electrical Engineers, New York, October 15th, 1889, and discussion thereon. Past-President Franklin L. Pope in the chair.

ELECTRICAL NOTES OF A TRANSATLANTIC TRIP.

BY THOMAS D. LOCKWOOD.

Although the English *Electrical Review* has heretofore been so good as to intimate in speaking of some of my pen productions, that they are characterized by such a mixture of practical information and humor as only a true born American could produce, yet it is a fact, as some among you have long been aware, that I am of English parentage and birth, and lived in England for the first sixteen years of my life.

It thus came about that I was more easily enabled to assimilate in habit and thought with the English electricians whom I had the good fortune to meet, than is usual, and my visit, so far as the British Isles were concerned,—though the first one in twenty-four years—was much like the return of a wanderer to his home.

The most difficult part of this paper was the choice of a suitable designation therefor. I cogitated over many; among others, I thought for a moment of denominating it "A Visit to Europe and the Bull;" the Bull, of course, being playfully indicative of of the domain of John of that ilk.

Calm contemplation, however, showed me that "Europa and the Bull" were out of question, and that such a title would have appeared in the highest degree anomalous to those who consider the United Kingdom as constituting a part of Europe. Moreover, it might be thought by others that as I spent so little time on the Continent of Europe and saw so little of it, I was not authorized to speak so grandiloquently of my humble few days in France and Belgium; and inasmuch as many of my observations,—for example those on outdoor electrical construction—are equally applicable to continental and insular Europe, there might be some doubt in the minds of my auditors whether there was any clean cut line of differentiation between the two elements of such a combination title.

Indeed, while myself considering this composite caption, I was forcibly reminded of a remarkable passage in the life of the late Alfred Smee (he who was principally known as the inventor of the Smee battery, in which polarization was prevented or materially retarded, by making the negative plate of silver, coated with finely divided platinum), but who was really one of the most able of the early electricians and electro-metallurgists, and a general philosopher of high merit.

Mr. Smee, during the years 1845 to 1847, took a great interest in the potato disease, and his view that the disease was produced by the ravages of a variety of aphides excited considerable rancorous animosity and ridicule.

In a pantomime exhibited at Drury Lane, appeared the following:

Scene—A village with shows, etc.—little boy looking at a peep show.

Showman loquiter—"This is the aphis vastator as you may see, very much magnified by Mr. Smee." Boy—"Please, sir, which is the aphis and which is the tater?" Showman—Whichever you like, my young investigator"

And in the same manner had I chosen the enterprising title already referred to, my hearers might have felt disposed to enquire: "Which is Europa and which is the Bull?

My excursion, from the time I left home until the day of my return, extended over a period of twelve weeks, minus two days. I sailed from Montreal in the steamship "Vancouver" of the Dominion Line, and had a short passage and over 48 hours of river and gulf sailing.

Passengers taking this route have a very short sea space and a correspondingly long and delightful smooth water and landlocked experience; they have a greater chance of seeing icebergs, whales and porpoises, with a certainty of cooler weather and a reasonable possibility of rougher seas. I returned in the "City of Rome," the only Anchor Line steamer plying between Liverpool and New York.

I staid on my arrival in England a few days in Liverpool, passed on to the Derbyshire peak district, then to Birmingham. From thence I went to London, making my headquarters not in the city, but in a most beautiful town, Tunbridge Wells, which is an inland watering place some thirty miles distant, in Kent. Later I passed over by way of Dieppe to Paris, and after a few days there, returned to England through Brussels and Antwerp.

After staying a few days more in London, I went to Scotland, calling at the Cathedral cities of Lincoln and York, and at the manufacturing city of Newcastle-on-Tyne. I halted at Edinburgh, passing on later to Inverness, via Perth; and then down the Caledonian canal to Glasgow, and back to London, stopping en route once more at Edinburgh. Then I took a side trip from London to Salisbury, Bath, Gloucester and Worcester, and again returned to London via Oxford. Finally I started from London to Chester, a most charmingly antiquated walled city, visited some of the North Wales watering places, inspected the mediæval castles of Conway and Carnavon, climbed Snowdon, sailed from Holyhead to Dublin, took railway to Cork, passing the celebrated Blarney castle and stone, spent a day at Killarney, and took ship at Queenstown.

My visit was not a business trip, and I made no attempt to combine pleasure and business, but it is hardly necessary to say that I did not travel with my eyes shut; and that when anything electrical came before me, I had no hesitation in making observations on it. However, my observations were somewhat desultory, and my account of them will necessarily also partake of that character.

TELEGRAPHY.

I saw little of interest in telegraphy until my arrival in London, where I lost no time in looking up William H. Preece, the well-known electrician of the British telegraphs, whose urbanity and good nature were as usual far above par. By his favor I was able to inspect the central telegraph station, which is located in an immense building in St. Martins Le Grand, just opposite the general post-office building proper.

I was placed in the care of Mr. Cooper, a genial gentleman, charged to a high potential with useful information, and in his company paid my respects to H. C. Fischer, the controller of the central station.

Mr. Fischer, with whom I had had some previous acquaintance during his visit to America in 1877, made strenuous efforts to recollect me, and I think succeeded, although I am not sure. But if his recollection was not perfect, it was at least a good imitation, and did not in the least interfere with the cordial reception which I received at his hands.

The London operating room, or rooms, is an immense affair. Heretofore I had a most exalted opinion of the size of the Western Union room in New York, but the London room was a decided revelation. I noticed a very peculiar effect there. When first introduced, a good sized room filled with operators, or more properly telegraphists, and apparatus dawned upon my vision, and together with Mr. Cooper and the superintendent in charge, I wandered through it; but near the end we suddenly turn a corner, and another vista as wide and long as the first, appears, which again we traverse only to find once more as we approach its farthest limit, that a third vast expanse lies before us, and so on apparently for half a dozen successive times. Then I was led upstairs where a similar succession of rooms was exhibited to my bewildered gaze.

Morse instruments, both sounder and ink writer, were there to be found. The single needle also appeared to hold its own, and I saw something new to me, viz., telegraphists reading by sound from the click of the single needle as it vibrated from one side to the other, striking its limiting stop.

With respect to the Morse, I do not think the average transmission is as fast as is the American average, nor do I think it possible to achieve extremely rapid sending with the heavy keys generally in use.

I was greatly interested with the way in which the Delany multiplex has been taken up by the British telegraphic authorities. It may have been in some degree improved and modified, and I understood that the number of circuits simultaneously worked successfully, were not as great as had been hoped for, but still it was in use; its use was increasing, it was doing good work on lines of considerable length, and the operators liked it.

Upon making inquiries about the Wheatstone automatic, it was to be noted that while authorities united in enthusiastic praise of its qualifications as a fast telegraph in all cases where the matter transmitted was to be received in duplicate or in multiple, such as news, the praise was by no means so emphatic with respect to its ordinary use as a means of transmission for business messages, as so much time was required to be taken up in the preliminary measure of punching.

The Pneumatic Dispatch branch of the London station is on an equal scale of immensity, and in fact is one of the great features of London telegraphy. Throughout the length and breadth of the land I found civility and prompt attention in the matter of receiving and delivering telegrams. Early and late I tested the telegraphic service, and found it up to the mark.

It is, I believe, a regulation that all telegrams are to be prepaid by stamp ; the said stamps in all cases to be affixed by the sender. The former of these provisions is no doubt invariably carried out, but as the latter is not always, I cannot speak positively as to either.

As a matter of fact I was rarely required to affix my own stamps in telegraph offices in large cities, whereas the letter of the law was fully carried out in small places, the principle being, I suppose, at these places, to make up in formality for what was lacking in amount of business.

The facilities for telegraphy in the United Kingdom are wonderfully complete, and I found no place too small for a telegraph office ; moreover in my opinion, telegraphy is there cheap, in spite of the fact that the address and signature has to be counted in and paid for.

The dynamo does not appear to be employed as a source of telegraphic currents, and here, I think the British telegraph system is a little behind the times. Yet, as far back as 1873, I understand that Mr. Preece experimented with the Gramme machine for this purpose, but found it too unsteady for the Wheatstone automatic instruments.

There are 2,300 cells of battery in use at the central station in London, and 220 circuits are supplied by accumulators.

Do I approve of the telegraphs being owned by the government? In a monarchy, whether limited or absolute, yes. Because the government is permanent and usually stable. It answers well in England; it is cheap and good. But does it pay? Well, if you consider that the agencies of a government should be administered for the people's benefit, I think it does. But for a republic, at least one like ours, in which radical and sweeping changes of administration occur at frequent intervals, I am clearly of the impression that a government ownership would be an unqualified evil. No doubt a government control leads to a superabundance of red tape; to the ridiculously excessive formalities of the "Circumlocution Office;" and to extreme conservatism. It also acts as a damp sheet upon invention. Yet in spite of these tendencies, the telegraphs of the British Isles are, I think, on the whole, better managed and better manned, than they would be by one or more corporations; and the question of remunerativeness seems to me quite of a secondary character, since even though the telegraph department be not profitable, it seems at least evident that the people are enjoying a lower rate than they would under other circumstances be entitled to or receive.

The rigid use of discipline was, however, well illustrated in an attempt which I made to see the telegraph office at Birmingham, but where I was told that they would have to write to London for permission to allow my visit. I could not wait the exchange of correspondence and thus have not as yet seen the telegraph office there. It seems the telegraph department does not use its wires at all on office business, but employs the mail altogether, the wires being reserved exclusively for paid business.

ELECTRIC LIGHTING.

From my point of sight as an electrical engineer I was disappointed to find that electric street lighting was, comparatively speaking, still a thing of the future nearly all over the Kingdom. Not that electric lighting is not being done, but that so little of what can be done is seen. Instead of having electric illumination all around you as we have it, we have, so to speak, to hunt up installations.

There are a number of central station plants in London, but London is an immensity and might contain many more, and yet electric lighting would be conspicuous by its paucity. You may, for example, walk through Holborn and Oxford streets and see lines of glorious shops lighted electrically; you may go to Grosvenor gallery and watch the historical illumination there; and you may at various factories see that the electric light is for this class of work being extensively employed.

The Great Western Railway station at Paddington is lighted up by 100 arc lamps and an infinity of incandescent hairpins; and many seaside resorts know of the electric light; while at Glasgow and Liverpool are central lighting stations of high capacity, but in spite of all this, you cannot help forming and bringing home the impression that there has been but little done in comparison with the immense amount which remains undone in this branch of electrical application. The truth is that the British public seems to have had a surfeit of electric lighting companies at an early date, with all that an electric lighting company implied in the years 1878 to 1882, and has of late years had no wish to take a second dose.

As a telephone electrician, I felt disposed to congratulate the telephone and telegraph people that they had escaped the horrors of burned out central stations and disturbed circuits.

Such lighting companies as may hereafter undertake to occupy this field are also to be congratulated that they will have the several years' experience of others, and all of the many improvements which have been produced in apparatus and insulation to profit by. I think that municipalities in England will themselves shortly take up this matter. In no case, I am sure, will "undertakers' wire" or the reckless class of construction which we have too often seen in this land of liberty be tolerated.

Isolated house lighting, is however, well advanced. Inquiries which I made proved that this branch of incandescent electric lighting which has overflowed upon the steamships and boats is very popular. In the several examples of it which came under my personal notice, the electricity is drawn from secondary batteries, which are during the day charged by a dynamo.

I had the happiness of dining with W. H. Preece, at his house at Wimbledon, and of examining his lighting plant. He uses the secondary cells of the Electrical Power Storage Company. The plates are formed of a leaden grid, the perforations of the positive plate being filled originally with minium, while those of the negative plate are filled with litharge. The charging dynamo was run by a gas engine, and Mr. Preece informed me that it cost him exactly as much for gas as it did when he used gas for illuminating purposes, while he had a much greater amount of light. I am bound to state, that in my opinion, the light was superb, being apparently softer and more equable than when obtained directly from the dynamo.

While upon this subject I may state that I also had the pleasure of inspecting the beautiful laboratory and workshop of Sir David Salomons, at Broomhill, Tunbridge Wells. Sir David is a wealthy amateur who has made the realm of secondary batteries peculiarly his own, and who has written a textbook on their construction and care, which, if not absolutely faultless, is certainly the most useful and practical work on the subject in existence. He has a pair of fine compound steam engines and several dynamos, these being connected with the prime motors by leather link belts of American manufacture.

A great number of storage cells, several hundred I should judge, and in prime condition, are located in a side room, floored with glazed tile, the entire floor sloping to one side for purposes of drainage. In the machine shop I saw some of the finest machine tools that my eyes have ever fallen upon, lathes, drills, planers, shapers and so forth, and it was particularly noticeable that each machine was provided with its own electromotor. All of the motors were of the Elwell-Parker make. Upon a shelf in the laboratory office were a few well-chosen books, among which I noticed the last bound volume of the proceedings of this Institute.

Sir David uses these motors for every conceivable purpose, furnishing electric light and power for his entire establishment, and while I was there, electricity was employed in making butter. If an electrician, whose heart is in his work, is ever justified in cherishing envy in his bosom, it is when visiting such a place as that of Sir David Salomons, and he has my heartiest acknowledgments for the kind way in which he allowed me to inspect the same, though himself absent.

I saw throughout my travels little in the motor line, except those to which I have referred, but I am informed that the use of electromotors in Britain is already great, and is rapidly increasing.

TELEPHONY.

The chief centers of good work in telephony are Dundee, Glasgow, Liverpool, London, Edinburgh, Manchester and Birmingham. The fact that I mention Birmingham last, shows that no attempt at classification has been made. I could not inspect the Liverpool central station as I happened to be there only during holidays, when the authorities were all absent, and for lack of time I was unable to visit Manchester at all, but I saw a good deal of the Liverpool outside construction, and I heard enough about Manchester, to assure myself that the installation was par excellence.

Many of you will be aware that three of the principal telephone companies in England, viz., the United, the National and the Lancashire and Cheshire, have recently joined their forces under the name of the National Telephone Company, Limited.

This company has now taken charge of the London Exchange, where I was rather surprised to find that there were already more than 7,000 subscribers.

There, nearly all lines were built over the housetops, with tubular iron fixtures of a construction, beautiful and light and yet strong. A very peculiar feature of this London construction was that the wires of a route were, as a rule, all strung in a vertical plane, there being very few cross-arms. This made a very symmetrical appearance, but it struck me that it was hardly a sufficient utilization of a route, or, as the Britons say, of the "way leave." This, however, was the work of the United Company, and I think was not in accordance with the judgment of the engineer of that company, who remains in the same capacity with its successor.

The post office department charges the telephone companies a regular royalty, it having been legally decided that the telephone is a "telegraph" within the meaning of the statute. The postmaster-general apparently does not like the action of the companies in uniting, although none of them were competitive, but all working in distinct territories. There is no doubt, however, that he fears that the capitalization will unnecessarily enlarge and will be a big bite for the government mouth whenever, and if ever, purchase may be decided upon.

It does not seem to be likely that the postmaster-general will take any steps to harass the companies, nor do I believe that the post office telegraph department will care for a long time to come to trouble itself with telephony, and it is my opinion that the remaining telephone companies will soon in turn be absorbed or otherwise united with the National Company.

Long line telephone work in Great Britain is, of course, limited by the size of the island and by the peculiar relative position of the large towns. I found very good talking between Birmingham and the several towns of Sheffield, Derby and Nottingham; between Glasgow and Edinburgh and Dundee; and especially fine from Sunderland to points upward of a hundred miles distant. For these latter distances circuits had to be artificially made up. In London there are necessarily many central telephone stations. No one who has not seen and studied London can form an idea of its vastness, and by reason of this vastness there will always be (notwithstanding the manifest excellencies of centralization) a plurality of central stations connected by trunk lines, mainly converging to a principal trunk station, but supplemented by a sufficient number of trunks between such central stations as are adjacent to each other. Yet I should say that there are at present too many central stations and that they are too close together, and I am under the impression that the present management thinks so, too, and will condense closely grouped central offices considerably.

In Scotland I found a regular multiple switch-board in the Glasgow central station, which was the most American looking central station which I have seen anywhere. In Glasgow, also, a "nickel in the slot" toll box has been devised, and is largely used at public stations, a penny or a certain number of pennies being the local charge, and a sixpence, or more, the trunk line or out of town charge. The operator can tell by peculiar sound signals automatically given by the passage of a coin or coins whether the proper amount is dropped in, and if not, the machine simply absorbs what has been put in and does not grant the desired accommodation. Regular subscribers have a check key which operates the apparatus without requiring the electro-deposition of a coin. This machine is well spoken of, and is said to give satisfaction. In some other parts of England these boxes are also used, and non-subscribers to the exchange may purchase a local check key for the purpose of using at any time the public station without having to pay every time. It may be of interest to state that at every railway station in England the "nickel in the slot," or rather the "copper in the slot" machine, applied to a variety of uses, is to be found. The weighing machine is the most popular, but the electrical shocking machine is a close second.

The best working long lines which I found were without doubt those belonging to the Northern District Telephone Company. This company is in charge of C. B. Clay, of Sunderland: the lines are new and are built of the latest model.

In Newcastle, all lines, both telegraph and telephone, which are owned by the government, are underground. They work very well, and the government telephone system there is ably managed by A. W. Heaviside, who based his telephone exchange upon a pro-telephone exchange, actually existing in Newcastle, and operating dial telegraphs long before the telephone exchange was thought of, or the telephone itself invented.

The National Telephone Company is fortunate in having such men as Messrs. Bennett, Coleman, Jackson, Phillips, Sinclair, and as far as possible employs men trained by previous education and experience to be equally efficient in a business and technical capacity.

As to electrical construction outside, I may say without hesitation, that the average is far above ours, whether telegraphic, telephonic or otherwise. The poles are no longer than are necessary, are uniformly well trimmed and painted, and above all are well stayed. Mile after mile I have ridden on a railway both in England and on the Continent and found every pole "stayed," or as we would say "guyed." The cross-arms are usually short, and project alternately a greater distance to one side or the other—that is, the upper cross arm may carry two insulators on the right side of the pole and but one on the left, while the next will reverse that, two being on the left and one on the right, and so on. The telephone companies had much housetop construction, but the same system of staying prevailed, so that the construction was superb, even though the roofs were all of the pitched variety. In Sunderland only, and the other towns where the Northern District Telephone Company holds sway, are to be found long cross-arms a la American, the idea having being imported by Mr. Clay, who visited the United States a year or two ago, and who is a manager of the most progressive type.

In Edinburgh, though nearly all wires were overhead, they were so symmetrically arranged as to be hardly noticed. A. R. Bennett, who is general manager of the Edinburgh district, looks also after Dundee, Galashiels, Dumfries and many other towns. The above remarks apply alike to the government construction in Great Britain, France and Belgium, and to the construction of telephone companies.

The only underground telephone line work which I found was the well known Paris sewer work and the government telephone exchange wires at Newcastle-upon-Tyne; but the National Telephone Company is beginning to lay some underground wires in Birmingham.

At Birmingham I also noticed some little pole work, and it was both amusing and instructive to see that, wherever possible, the poles were modestly planted close to the gable end of a block of buildings, or inside of a high fence, or in the corner of a vacant lot, so as to attract as little notice as possible.

I found that silicon bronze wire was well thought of throughout the several territories which I visited, and in general use. It can be made of a conductivity nearly as high as copper, while it is tougher; and it is much better for telephone work than iron, because not only is the conductivity higher but it possesses a much lower co-efficient of self-induction.

One telephone span I saw in the south of England crossing the opening of the Dartmouth harbor, which was certainly not much less than half a mile in length. I venture the prophecy that in the near future we shall use silicon bronze here in the United States much more than heretofore we have done.

In France I visited of course the Exposition, and met Messrs. Abdank-Abakanowicz and Berthon, both old friends. The finest electrical affair about the Exposition, to my mind, was the way in which the great fountain was illuminated in kaleidoscopic colors every night. This has, of course, often been done before, but never, I think, so perfectly or on such a scale. The success of the installation is due to Mr. Aylmer, who has had it in charge.

I stood in the tower and admired, and then I went down through the tunnels and stood beneath the fountain, where the arc lights were reflected through colored screens and through the plate glass floor, and marveled and admired still more, especially when I saw how the levers arranged in the tower like those of the Pennsylvania railway signal tower, worked pneumatically the screen mechanism below, and kept an ever-changing succession of beautiful colors upon the different fountain jets; and all this apparently, so far as the crowd could see, working itself; for there was no manifest link between the operating tower and the fountain.

The Exposition telephone station, as arranged by L. Berthon, was a sight in itself, with its multiple switch-board, its closets for private communication, its samples of fine electrical apparatus made by the Societe Generale des Telephones, and its apparatus and circuit arrangements for receiving operatic performances and re-issuing them for the delectation of numerous auditors.

Long telephone lines stretch from Paris to Havre, Lille, Bruxelles, Lyons and Marseilles, and work admirably. These are chiefly of copper and are arranged in metallic circuits.

Mr. Berthon, the managing engineer of the Societe Generale des Telephones, told me that his company while sparing no effort to keep up its efficiency, had for some little time been resting on its oars in view of the possible purchase of its telephone property by the government. And, as you are aware the government has since decided to acquire this property, and has in fact taken possession of it, which action, I believe, to be an unfortunate one, alike for the government, the society and the people.

Of course the great Edison exhibit with its 9,000 feet of floor space was a tremendous affair; and, although it would naturally impress the observer without any extraneous aid, it is fair to say that its merits were literally hammered into the visitor's mind by the able enthusiast who had it in charge.

This, together with the surprisingly fine exhibit made by the Thomson-Houston company in association with the Thomson Electric Welding company, and also the unique historical exhibit, made by the American Bell Telephone company, fully redeemed the electrical section from any participancy in the otherwise too true charge that the share of America in the great exposition was comparatively insignificant.

The compound engines, built by Messrs. Davey, Paxman & Co., of Colchester, England, to run the dynamo for lighting the fountain tower, and exhibition, were an interesting sight to me. I was delighted to watch their fine and speedy regulation as the requirements varied. The young man who was running them, was a rising electrician, H. D. Wilkinson, the same who succeeded Mr. Kennelly in the authorship of the series of electrical measurement articles lately published by the London *Electrician.*

Mr. Wilkinson, believing it right to give himself a practical intimacy with all branches of electrical engineering, was taking this method of doing so, and illustrating the English tendency to carry out to the fullest extent the scriptural maxim "Whatsoever thy hand findeth to do, do it with thy might."

I was favored by Messrs. Siemens Bros. & Co., by permission to visit their works at Charlton, Woolwich. There I found active work going on in all branches of electrical manufacture, but the most new and interesting feature to me was the submarine cable work which I witnessed in various stages. This I could not avoid reflecting, is the one branch of electrical work in which Britain is practically without competition. Here I met Frank Jacob, the electrician of the Siemens company; a remarkably able and intelligent man, and the inventor of one of the earliest systems of multiple telephony and combined telephony and telegraphy. A most interesting feature of the Siemens works is the testing room, where every foot of cable is tested during the process of manufacture. Immediately across the river and thus an opposition physically as well as commercially, is the rival cable establishment of Silvertown.

Speaking of manufactories, brings to mind that the tendency of the British business public to consolidations (or amalgamation as it is mercurially termed), after having swept over all the breweries, distilleries and gas companies within reach, and after having touched the telephone companies, is now exerting itself upon the electrical manufacturing interests, as evidenced by the fact that all of the storage battery companies and electric railway companies have amalgamated to form "The Electric Construction Corporation, Limited," and that the various Woodhouse and Rawson manufacturing companies have also consolidated into a large joint stock corporation.

While in Birmingham, and engaged in visiting the local lions, I went to see Aston Hall, the free museum of the town. In one of the many interesting rooms there, I observed on exhibition, an old fashioned large magnetic machine designed, as an inscription thereon informed me, for use in electroplating. Upon close inspection, I found it to be the first magneto machine ever designed for that purpose; namely, that made and patented in 1842 by Stephen J. Woolrich. This machine was presented to the museum, by Thomas Prime & Son, electro-metallurgists of Birmingham.

Passing through the cathedrals and abbeys of Great Britain, my bosom, like that of Sir Joseph Porter, K. C. B, swelled with pride, to note how these buildings and their precincts were made into a species of mundane Valhalla, where the worthies of the nation, distinguished in war, science or literature were immortalized, and their memories treasured, for the example and encouragement of their countrymen, and as a mark of appreciative acknowledgment of their services rendered to their country.

In Westminister Abbey I remarked the tombstone of William Spottiswoode, electrician and philosopher; while in the abbey and churchyard of Melrose, I noticed respectively the graves of Michael Scott, and Sir David Brewster, one, the friend of True Thomas the Rhymer, of Ercildoun, and the scientist par excellence of the fourteenth century; the other, one of the most fortunate wizards of the nineteenth century; Michael in his day was called a "wizard," but would probably now be called an electrician.

In conclusion, I desire to say that the cordial and fraternal manner in which everywhere I was received by electricians, and by those interested in electrical development, and the hearty hospitality which I received, merit and receive on my part the profoundest appreciation and the warmest reciprocal feelings of kindliness. Never from the moment that I set my feet on European shores, until the moment of my departure, had I any need to feel that I was a stranger in a strange land.

DISCUSSION.

THE CHAIRMAN (Mr. Franklin L. Pope): We have listened with much pleasure, not unmixed I hope with profit, to Mr. Lockwood's interesting paper. In no way is there more to be learned than by comparing with our own work, that which has been done in other countries, by men who are working out these problems on their own lines and under different conditions. By what Mr. Lockwood has said about the English telegraph service, I am reminded of one or two features of it which struck me as being exceedingly convenient. One of them is the method of prepaying the telegrams by stamps. The rate is sixpence for twelve words, counting the address and signature. Each additional word is a halfpenny and the ordinary postage stamps are used indiscriminately for letters and telegrams. The convenience of this appears, as it did to me two or three times, when traveling a long distance on an express train which sometimes does not stop for two or three hours. Having occasion to send a telegram for some one to meet me at a point ahead I have written my message while on the train, put stamps on it, wrapped up three or four coppers with it for "backsheesh," and thrown it off at a way station; and I have always found that it was dispatched and received in due time at the point of destination. That is a very great convenience. There is another feature that will strike our people as a little curious, and that is that in English telegraphing there are no dead-heads. If Mr. Preece sends a telegram he has to put stamps on it, and if Queen Victoria sends a message, she also has to put stamps on. Office messages are sent by mail and paid by postage stamps.

Mr. Lockwood has referred to the construction in England, and the careful manner in which the poles are stayed. That is very largely due to the costliness of timber, rendering it necessary to use light sticks of timber where we would use heavy ones, and stiffening them with stays, so that the general appearance of their lines is a very large proportion of wires and insulators to a very small allowance of pole. It gives them a rather neater appearance than ours in some respects, but I doubt whether it is much more substantial, although there is no question that the fastening of the wires and of the insulators is much more substantial than ours, and that is particularly the case on the continent.

DR. ALLAN V. GARRATT:—Mr. Lockwood has spoken of the excellence of construction in England. We have heard through the English journals from time to time in regard to the rules and requirements enforced there. If Mr. Lockwood would kindly tell us about the system of inspection for inside electric light wires in England, and the conflicts, if any, between the insurance and the electric light people, it would be interesting and instructive.

MR. T. C. MARTIN:—With regard to that very interesting topic suggested by Dr. Garratt, I think we might have some light thrown on the subject by a gentleman here this evening, and known to a great many of us by his extremely able and lucid writings. I refer to Mr. G. L. Addenbrooke, of England. That gentleman is now on his way home from Australia, where he has been engaged in putting in several plants on steamers. He has been associated with Mr. Ferranti in London in a large portion of his work there. I know of no more interesting work at the present time than that which Mr. Ferranti is doing. It transcends in many respects anything we have done or contemplate doing, and the experience derived from it, I think, would be extremely valuable to us. I think Mr. Addenbrooke's experience in operating the station at the Grosvenor Gallery, and also from running the overhead circuits in the streets, might be profited by in this city, and regarding which various statements have recently been made with more or less truth. But before Mr. Addenbrooke favors us in that way I would like to call attention to one extraordinary omission from the paper. It is probably due to the fact that there was no information obtainable. I refer to the fact that there is no mention in the paper of electric railroading. At the present time in this country we have in something over 100 cities, counting roads, branches and extensions, 179 electric railways. By the end of the year they will operate some 1,200 miles of track. Over that track will run something like 1,800 to 2,000 cars. In view of the fact that we have such an extension here, and that it meets with such universal approval; and that Mr.

Lockwood with all his keen observation—and we know him to be a most observing man—should have seen nothing of the kind whatever to arrest his notice or attention in England, is something phenomenal, and it would be interesting to know what causes that. Does the difference lie in conservatism? Does it lie in local conditions altogether apart from national temperament? Does it lie in the obstructiveness of the local authorities? Does it lie in the caution of capital, or what? What is the reason? There must be some reason and even if it be a bad one, there probably is an argument to offer for it.

Another very interesting point in the paper, was the use of stationary motors. Mr. Lockwood said that he understood the use of such motors to be on the increase in England. I have been at some pains to ascertain whether that is a fact, and I cannot find that the use of such motors is very largely increasing, and the reason seems to be there are no stations to supply the current, and naturally there are no motors. But an explanation has recently been offered by Mr. Van Rysselberghe of Belgium. It is proposed at the present time to introduce electric lighting pretty extensively in Brussels, but Mr. Van Rysselberghe seems to think there is no use for a central station. In his opinion everybody who wants the electric light should generate the current himself and supply himself, and for that reason in place of a central station, as we know it in America, he proposes to put in hydraulic motors to drive independent plants, these hydraulic motors to be worked by the high-pressure hydraulic service of the city. Mr. Van Rysselberghe, in explaining his system recently, said that he did not think there was any reason at all for the introduction of the electric motor on the Continent; that the European consumer wanted something simpler than the electric motor. How he would get it, I find it hard to conjecture, but such he said to be the case. I have listened with a great deal of interest to Mr. Lockwood's paper, and would be very glad indeed if he could give us light upon one or two of these points which I have suggested. I would suggest that Mr. Addenbrooke's experience with regard to the work in connection with the Ferranti installation in London is of the most interesting and valuable nature; and now that the Deptford station is going into operation, and is a practical development of the results obtained at the Grosvenor Gallery, I think it would interest us to know what he has done there himself.

Mr. G. L. Addenbrooke:—I should be only too glad to say anything that I can, premising my remarks with the statement that I have been away from England very nearly a year, chiefly in Australia, and that I came here to find out what they had been doing in England. I rather came here to hear the latest news than to be able to say anything myself. It is a great pleasure to have heard Mr. Lockwood speak so appreciatively of the overhead work in England. We have felt satisfied ourselves that our overhead work had a good many defects in it, but in England we look a great deal to people in America, and Mr. Lockwood having such thorough experience, and speaking so well of what we have accomplished, it gives us a great deal of gratification, especially as I happen to have been connected with the work in various capacities myself, both telephonic and electric lighting. In referring to overhead electric light work I am speaking rather of the past than of the future, because I think your are aware the commission appointed by the Board of Trade, which has lately gone into the whole matter, has formed a set of regulations under which the whole of the electric lighting in London is to be placed underground as soon as possible. No additional overhead wires are allowed to be placed, except such as may be agreed upon between the inspectors and the various companies, and the central station companies in London are now very hard at work laying down mains. However, there is no doubt a good deal of overhead work will be required for various purposes, and as you have so much overhead work here perhaps it might be worth while to explain our practice. First of all, we have not been allowed to plant posts in the streets at all, except in country roads. Within the boundaries of towns it is an absolute rule that no posts can be fixed in the streets. If we want posts we must put them in people's back gardens and in vacant lots, etc. Of course this has required that the majority of posts be placed on the tops of the houses. In foreign towns they have also had to go on the tops of the houses very largely. They have adopted one construction; we have adopted another. On the Continent most of the roofs have a very steep slope, and are tiled so that it is quite impossible to walk on them, and, of course, people do not put up stronger roofs than they can help—only strong enough to support the tiles and stand the wind, but not strong enough to support telephone posts. On such buildings they use two long pieces of timber, and form what they call a horse. That consists of a number of uprights and a lot of cross frames. I think you have some of them here. Nearly the whole of the work is done in that way. In England we went to work in quite a different way. As Mr. Lockwood said, it is not unusual in London, at any rate, in overhead work on houses, to have wires arranged horizontally As a rule there is but a single span. We have first of all what we call a chair. That is an iron casting weighing from fifty to sixty pounds that goes on the top of the roof. From this rises a wrought-iron tubular pole. These tubes are carefully made to specification; they are about three inches internal diameter. It is usual to put up a pole of not less than 18 feet, but they range as high as thirty. In such cases as the latter, it is usual to have two or three poles, one inside the other, each pole having a collar and a bolt going through. These poles are simply stayed to the brickwork. Heavy spikes are driven into the brickwork, and it is rather an agonizing process for the house as you may imagine, but somehow men have a way of doing it, and they drive spikes eight inches long into the brickwork. It is wonderful how the masonry stands it. I may say I have seen some very large cracks occasionally. The wall itself will sometimes split out in the plaster joint. Each pole will have at least four of those stays under ordinary circumstances, and then of course it has to be stayed in accordance with the rules.

When I came to America and saw the mass of wires running in all directions, it reminded me of what we call the early days of telephones in London, and we had so many faults with wires then that I could not understand how things worked. So I looked about me and I find apparently the difference is that we think nothing of a span of 120 yards. Sometimes in telephone work we go 200 yards, and 150 yards is considered very little for a span of wires. Of course in running wires that distance there is a very considerable amount of sagging, and in a wind they are liable to come in contact with each other, and they are also apt to get out of regulation; that is, wires put in when the weather is hot, and wires put in when it is cold, and brought into unison, will get out of unison occasionally. So that, perhaps, accounts for the reason why you are able to run such a mass of wires about the streets and yet get along at all.

Then we come to another point, what we call "leading in"— that is, taking the wire from the pole to the office of the subscriber. In England that is always done with gutta-percha covered, wire, as the people who began telephone work in England were those who had had experience with telegraphs. This wire costs about three or four pence a yard. It has a thick covering of first-class gutta-percha. Of course this insures immunity from faults and leakage, and it is in some cases taped and in some cases braided. But if it is taped, it is very strongly taped, and the whole thing is what you call a very excellent job.

One very great reason why tramways perhaps have not made more advance in England is that posts are not allowed in the streets. Perhaps in the country districts they might be allowed, but without a lot of formalities, etc., before town councils, no leave to put up posts will be given. I think it is highly probable it will come, but still it will take time. On the other hand, notwithstanding what has been said about electric railways here, I know perfectly well you have nearly a thousand miles in operation. I have just come from San Francisco, and stopped there some days. I stopped at Denver and Salt Lake City, and at Chicago, and at Pittsburgh, and at Philadelphia, and I have been here some days. At each of these places I have done all I could to see electric tramway work. I happened to see a car in the distance at Salt Lake City. They were putting up the wires in Pittsburgh. I was kindly shown a car running with accumulators here yesterday. I called on the Sprague Company, and they said they had nothing which they could show within a hundred miles of New York, so that although you have a great deal, yet your country is so large, and the work is so scattered around, that it does not show much, and I think, perhaps, the same applies to England. We certainly haven't got anything like that length of track, but there are a lot of little tramways running about there which I think may be called to some extent the pioneers of tramway work. For instance, there is the Blackpool tramway, which has been running several years. There is the Portrush tramway, in the north of Ireland, which I think was the first electrical tramway ever erected. There is a small tramway running along the Brighton Beach which has been very popular and has been running six years. There are two or three companies in England operating experimental lines now in a small way, and certainly without going out of your way to see them you probably would not succeed in doing so.

Passing on to electric lighting, beginning at the station, at the

Grosvenor, we were working with 2,500 volts, and the dynamos were so perfectly self-regulating, at the period I was there, and with the load they had on them, that all regulating appliances were practically unnecessary beyond a sensitive governor on the engine and perhaps an electric governor on the engine and something to keep the exciter in order. All you have to do is to keep your dynamo turning round at the same rate. The circuits were so large that the putting on of lights or taking them off practically made no difference, and we always arranged things in such a way, that we never allowed, if we could possibly prevent it, any large circuits to be run into public buildings or theatres, or anything like that. That is to say, we always contrived that not more than 50 or 60 lamps should be turned off at the same time. Of course a man going around a building could turn off four or five switches, but while he was doing that the engine driver would have a chance to look around. The circuits were so large that 50 or 60 lights on or off practically made no difference, and most of the regulation was simply done by the engine driver controlling his engine. Of course that simplified matters very much.

The overhead work is done on very much the same line as telephone work, only the poles are shorter; sometimes they are not more than twelve feet, but as a rule they run to eighteen feet. Near the top of the pole is a clip, and an insulator is secured to this by a strap and nut, as was formerly done in telephone work From here to the next pole, which might be as much as 100 yards away, but preferably not more than 60 or 70, a steel suspender of seven No. 14 gauge wires is run, and that is bolted up pretty tight. I might say it is a very difficult task, making up these steel suspenders—turning the things around to make a good tight way-leave job. Below this there was another similar insulator and to this the cable was made fast. The cable ran below the suspender. Most of the main circuit cables were nineteen No. 15 gauge, the branches being about seven No. 16's, and so leading off to seven No. 20's, or something like that for a few lights. You will see that, first of all, this steel suspender is absolutely insulated. That suspender will practically stand almost any voltage you could put on it. The cable itself was insulated in the same way. Supposing it was a bare wire there was always three inches of china between it and the nearest earth. These cables were first of all insulated with little ebonite rings, by which they were suspended, having a little split ring which allowed them to run along the suspender. Afterward Mr. Ferranti adopted raw hide leather thongs, which were slipped over the cable. The cable was all wound up on a barrel or any convenient means. This cable was taken up to the pole, the first ring put on and the cart taken to the next pole. If you were going to run half a mile of cable you would have a man to almost every pole. They would haul the cable, the men putting on the rings as it went by. Of course it is a slow process, but it is fairly satisfactory when it is done. First of all, you have the insulation of the cable; secondly, you have the insulation due to the raw hide thong; thirdly, you have the insulation due to the suspender; so that altogether you are fairly safe. Supposing that suspender goes; the cable at first is bound round the insulator quite firmly enough. It is no use spending a lot of money on insulation and getting tests in a test room if you spoil it before it is up. But in this case these insulations were due to the cable that was wound around them and lapped across so as to avoid as far as possible cracking the insulation in any way. Therefore, unless the pole came down bodily, there was not much chance of an accident happening, and if the pole came down bodily, this insulation, as has proved to be the case in more than one instance, would be quite sufficient to keep everything right until the fault was remedied. There is another advantage in having this suspender. Our poles were usually lower than telephone poles. The steel suspender the telephone people were at liberty to draw their wires across as much as they liked. Occasionally they would get a wire underneath, and when it was sagged, haul it a few minutes, but they soon learned to leave off doing that. Of course, working with the alternating current and on a parallel system we have no confusion of wires such as there is here. A single circuit was generally sufficient for most of the requirements. In a few instances, where lines were going into different districts, owing to difficulties in getting our way-leaves, there might be four wires, but no more. I put it to American engineers whether this is not really the better way of doing things. The number of circuits you may get by running arc lamps, or anything else, in series becomes in time stupendous; and I know in working in telephone work underground what a bother it is to be mixed up with conduits and the remedying of faults. In adopting a parallel system you get rid of that, and you get simplicity right away. When we wanted to take the current off we put another of these brasses on and another set of insulators and we took what we called "T's" out of the cable at a few feet from the post. These, of course, were small wires; they were carried down the side of the house, the insulators standing well out from the wall, and as a rule they went through the walls in china pipes, especially made for the purpose; that is to say, we do not simply take a chisel and ram a hole through a wall anyhow and just put the wires through, as I saw them doing in Pittsburgh. It was rather a costly job, I must say. We finished up with Portland cement afterward, the wire coming to a cut-out, which by the regulations of the fire insurance companies was either put on slate or on a brick wall. These wires were not allowed to be put on wood or near wood. Such things were not allowed as bringing a wire through the framework of a window. That would not be tolerated. When they come inside they come almost immediately to the cut-out, and even if there happened to be a ceiling three feet above, the fire insurance companies insisted on a large sheet of asbestos being fixed against that. From there we box our converters inside of iron bricks which were fixed in the wall, and on the way to the converter there was the usual switch for cutting the whole installation out. Of course the switch was in many cases not used at all by the householder. In fact, it was more for the convenience of the central station people than for the householder. They turn off their lamps on the local circuits.

Now, before going further, I would say something of the class of wire that was used. I think the reason that English work has taken the particular line it has, is almost entirely due to the fact mentioned by Mr. Lockwood, that England has done all the cable work so far. We were accustomed to expensive wire for cable work, and consequently we did not object to paying £250 a mile for such a cable as nineteen 15's, which I dare say you would think a good deal here. The class of conductor we used was made of nineteen No. 15's, tinned on the outside. This was then wrapped round with three wrappings of fine rubber, such as you use in making joints. Then on the top of that comes what is called a white rubber, that is wrapped on two or three times spirally; outside of that there is another of what is called black rubber. These two outside rubbers have vulcanized material mixed with them. When that cable is made to the thickness of insulation which we used on these cables, and do use now—one-eighth of an inch of rubber—the cable is then put in the tank and vulcanized. Now, there is one point that cannot be too strongly insisted on, and which is this, that it is no use having an expensive cable like that if you are not going to protect it thoroughly. India rubber will stand fairly well under water. It will stand fairly well in moist weather, but will not stand in dry air; and it will not stand in alternations of wet and dry. If you will expose the rubber to the air it will in time perish. It is also liable, if it rests on anything sharp or sharply rounded, to get very thin at that point.

The way we went to work was this: On the outside of that cable is a fine waterproof tape, which is vulcanized on to the material and absolutely forms part of it. How much further you go depends on circumstances, but a general rule was to take a compounded tape, that is a strong linen tape twilled and dipped in some compound of ozokerite, and this was wrapped around in one or two coatings. Outside of that we carefully put on a strong braid of yarn, and the whole was thoroughly saturated with the compound. You have then done about all you can do, and I may say that in London I believe that there are three or four miles of cable of that class which has now been lying in iron pipes carrying a tension of 2,500 volts without any accidents. The cables are simply drawn into the iron pipes and lie side by side. I know that the Silvertown people four or five years ago laid down such cables as that for arc lighting throughout their works. The grounds of their works are fairly saturated with various mixtures, yet it has given them no trouble at all since. I cannot help thinking that if your cable is properly protected that it will not give trouble. That cable, of course, before it leaves the manufacturer, is carefully tested. I have spent months in testing telephone and electric light cables of that character. This class of cable manufactured by the Silvertown Company would test from 2,000 to 8,000 megohms to the mile at a temperature of 75°. If you put the testing current on to a cable like that it flows into the cable with a great rush, and then the rush diminishes, and you gradually see the spot of light coming back. One should always watch that spot of light for ten minutes. Sometimes it will give a little kick, and that will denote that something is not quite right. But the Silvertown people have told me, and I have known, that they have manufactured in submarine cables as many as seventy miles of cable without locating any fault that they were obliged to cut out.

As to installation work, that is also done with first class rubber covering thoroughly vulcanized. It usually depends a good deal on the fire insurance inspector. One man will pass one thing and another will not pass it. You have got to know your man. Mr. Musgrave Heaphy first succeeded in imposing his rules more or less generally, and work is now, I believe, fairly up to his standard. His standard means that no wires are to be within an inch of each other in wood casing; that wood casing is used everywhere; that there is no exposed wire; that there are cut-outs to every two or three lamps; that the cut-outs have all slate or porcelain bases; that all switches have porcelain solid bases, and that no screws communicating with the current are allowed to come in contact with woodwork at all.

No doubt we have sacrificed rapidity of progress to some extent, and we have had a lot of trouble; still there has been immunity from accidents, which is very gratifying, and the work will stand. There is no doubt that the people, when they put electric lights in their houses, have an idea that it is going in just the same as gas fittings, but a good deal of the electric work put in in past years I do not think would last as long as fair average plumbing. I am very much obliged to you for listening so appreciatively.

THE CHAIRMAN:—I would like to ask Mr. Addenbrooke his opinion on the proposition which we have heard considerably of within a few weeks, that while there is no particular difficulty in insulating a direct current of 3,000 or 4,000 volts, it is absolutely and entirely impossible by any known means of insulation to insulate an alternating current of a thousand volts. What is your experience as to that?

MR. ADDENBROOKE:—It is not a subject on which one likes to make a random shot—and my experience was pretty fully stated in the London *Electrical Review* three or four years ago. I do not think there is anything whatever to show that the alternating current will break down insulation quicker than a continuous current. I know a great many statements are made on the subject. I do not know of any one in England or America who has made a careful series of experiments on that point. We all know that the alternating current will not produce anything like the arc that the continuous current will.

A continuous current of a thousand volts will produce an arc—I would not like to say how much longer than an alternating current will produce. We know that a cable does not receive the full charge of electricity at once, while you can see the movement of the galvanometer an hour after the cable has had the current on, but of course the great rush occurs immediately. Still it takes a very appreciable time, and in the case of an alternating current, we will say alternating 200 times a second, it is quite conceivable—I do not say that it is a fact, but it is worth putting forward—that the insulating substance around that cable has not time to get sufficiently polarized to allow the discharge to pass through quickly before the current has turned in another direction and has depolarized it. I do not say that this is a correct view. It is a thing that requires to be gone through very carefully. From all the observations I have made and what I have heard on the question of insulation, I think that an alternating current will not go through more quickly than a continuous current.

THE CHAIRMAN:—The English telegraphic engineers have always taken the ground that a submarine cable was less injured by an alternating than by a continuous current. They have used the double current system on defective cables, in preference to the single current system, which will tend to prove, I suppose, that they apprehended more trouble from the breaking down of the insulation by the continuous current than by the alternating.

MR. LOCKWOOD:—The hour is late, so I shall make my remarks in reply to questions very brief. The Secretary has mentioned the subject of pneumatic tubes, and that very little or nothing had been said in my paper regarding them. I think, perhaps, I may be excused for the reason that there was very little or nothing to say. It is true that pneumatic tubes are much more entensively used by the British telegraph authorities than they are used here, because it is apparently one of the maxims of the British telegraph authorities that it takes a longer time to deliver a message than it does to send it. I am sure that the same view is borne out by the experience of every one of us who has ever had occasion to send or receive a despatch, and so instead of filling up the large cities such as London, Liverpool, Manchester and Birmingham with telegraph sub-offices, at about every hotel and every ten blocks, they have organized a system of pneumatic tubes which is very like that of the Western Union in New

York, but on a much larger scale, and as a general thing, they send their messages in packages in a pneumatic tube from one point to another.

In respect to the inquiry made by Mr. Garratt concerning the relative methods of inside construction in electric lighting, and how they get along there with the inspectors, I think that has been sufficiently answered by Mr. Addenbrooke.

Mr. Martin thought something should have been said regarding the electric motor railway service. There are not anything like as many street car roads as there are on this side. But it is only within the last twenty-five years that street car railways were introduced into England by Citizen George Francis Train, and they called them tram roads, not from any connection with Mr. Train, but because there were tram roads in collieries, before Mr. Train was heard of. During the twenty-four years I was absent from England, I found there had been a great advance in street railroad business. But they are not a fast nation. In England they would rather do a thing well once than do it fast a dozen times. So they found the horse car a pretty good thing to stick to, and they have made more of it than we have done, because they make all the cars double deckers; not only that, but they use steam dummies to a very large entent, and between Birmingham and Soho and other districts I find they use the traction cables, and it gives good service. One of the electric roads I had the pleasure of traveling on was about a couple of miles long, and situated on Brighton Beach. It is a very poor kind of a thing to look at, but it is in a safe location—on the sands of the sea shore, and it does not trouble a single telephone wire. The current passes through the two rails. The Port Rush road is still there, and, as I understand, is doing good service. I had not time to see it. After all is said and done, the main reason that electric motor roads, operated as we operate them with a single wire overhead are not more common, is the reason mentioned by Mr. Addenbrooke, reinforced by the very potent hand of the central government which knows that the operation of such an electric railway in such a way would be a great deal of a nuisance to its own telegraph wires, and would mean death and destruction to all telephone wires in the kingdom, which will possibly be all government property some day. I asked Mr. Preece about that, and he reminded me that he had presented to the Institution of Electrical Engineers a paper showing very clearly the evil results to all electric systems operating sensitive instruments, of railway or electric light systems using the earth for a return. I also inquired of a gentleman well informed in electrical matters, why the electrical railways were not more used with the overhead wire, and he said because they would not be allowed. I thought that was a very good, solid reason, and when I told them how they were being sown broadcast through the land here, and how, when harmless telephone campanies remonstrated, they were universally met with the cry that the telephone companies wanted the earth, he said, "That is very true; they want the earth here also, but they want it to work on like any other peaceable mortals, and they do not propose to allow that a tiger has the same right to walk the earth that a man has." And that seems to me the principal reason why the electric railway has not made more progress than it has in England.

I do not know that I have anything more to add except to express my gratification at the very lucid demonstration made by Mr. Addenbrooke. He said one or two things that I should like to have said myself, but I am going to stay in the country and he is not. I thank you, gentlemen, for your very kind attention.

THE CHAIRMAN:—In closing this very interesting disussion, I will take the liberty of expresing the thanks of the meeting to Mr. Addenbrooke for the information he has given us, and will announce that the next paper before the Institute will be read on the evening of October 29th, by our fellow member, George B. Prescott, Jr. His subject is "Some Methods of Regulating Accumulators in Electric Lighting."

A paper read before the 39th meeting of the American Institute of Electrical Engineers, New York, October. 29, 1889, and discussion thereon.

SOME METHODS OF REGULATING ACCUMULATORS IN ELECTRIC LIGHTING.

BY GEORGE B. PRESCOTT, JR.

In preparing the following paper, the limited scope of which is indicated by its title, I have assumed that it is generally conceded that the modern electric accumulator is an apparatus capable of performing a peculiar but useful function in the art of electric lighting as now commercially practiced, and that it is free from inherent defects which would render it unsuitable for such application. Indeed, I am sure that every careful observer has noted in the somewhat slow evolution of the accumulator, that structural improvement and adaptation to practical requirements, which unquestionably show that it has come to stay; and will acknowledge that even as it exists to-day, the accumulator is a factor in electrical industry too important to be ignored by those who take a broad view of the situation.

It is not my intention to refer to those numerous successful applications of accumulators to the propulsion of small marine vessels and land vehicles, to car lighting and other minor work where the continuous generation of power from the combustion of fuel would be impracticable; neither can I fully discuss their characteristic actions under working conditions, within the limits of this paper. Nevertheless, before proceeding to the subject proper, I deem it incumbent to point out that the electrical accumulator, even in its more improved forms, is an apparatus having well defined limits of capacity, and that these limits cannot be habitually exceeded without endangering its effectiveness and impairing its durability. Generally speaking, the limits referred to. which vary with every type and size of cell, relate to the rate and quantity of charge and discharge, these factors being measured respectively in amperes and ampere hours.

In considering this admission, however, it should not be forgotten that every dynamo-electric machine is, to a certain extent, subject to similar limitations; for, like the accumulator. if it is demanded of them, they are capable of generating a current far exceeding their safe carrying capacity. Here the likeness ceases, however, for while the dynamo will deliver its normal current as long as adequate power is applied to its shaft; by continuing to draw current from an accumulator for too long a time, as likewise from an excessive rate and quantity of charge, serious damage may result. In fact, in some respects an accumulator may be likened to a draught animal, which can be made to perform abnormally large amounts of work for short periods of time, but only at the expense of its vitality if the practice is frequent.

While much might be said regarding the maintenance of accumulators, and moreover, the apparatus to be presently described is employed in a large measure for the purpose of effecting proper maintenance as well as regulation, it must suffice to say here that the normal working rate and capacity of an accumulator battery having been stated by the makers, all necessary data relating to its use may be ascertained from the indications of ordinary hydrometers, ammeters and voltmeters. That is to say, during the charge and discharge of such a battery the rise and fall of its potential and of the specific gravity of the electrolyte in its cells, are both quantitatively indicative of its condition and capacity for doing work. It is evident from what has already been said regarding the importance of working accumulators within certain prescribed limits, that in order to effect this result two classes of apparatus must be provided with every accumulator installation, viz.:—indicating or measuring instruments, and regulating appliances to be operated either automatically or by hand. I am constrained to add here, that careful and extended observation leads me to believe that a lack of appreciation of those now obvious facts will largely account for the ill success attending many of the earlier accumulator installations.

Although there is reason for believing that some of the early promoters of business enterprises based on the manufacture of electrical accumulators, anticipated that accumulator systems of lighting were destined to compete with and perhaps supersede many direct systems, I believe that the more mature modern idea is that they are, for the present at least, mainly subsidiary to other electric lighting systems. As auxiliaries to many direct systems, accumulators undoubtedly effect a reduction in running expenses and add an element of reliability, besides accomplishing certain results that are not otherwise obtainable. In order to fully appreciate the truth of these statements, it will be necessary to glance at the conditions commonly existing in ordinary isolated and central station plants as they are operated at the present time. Doubtless every one who is familiar with the electric lighting business, is acquainted with the vagaries of the load curves during each 24 hours of a central station's run. Generally speaking, about sunset the load begins to increase gradually, finally reaches a maximum which is maintained for a greater or less period of time, and then falls to a minimum about which it fluctuates for the remainder of the 24 hours. Almost every isolated plant, whether it be in office building, hotel or apartment house, has a curve of the same general character; and probably most manufacturing establishments would prefer to have a few lights burning through the night, if only for the watchman's convenience, were it not for the expense entailed by the continuous operation of the plant. It is a truism to say that the cost of fuel per horse-power increases inversely as the power plant operates below its normal capacity, and that other operating expenses are, to a considerable extent, fixed independently of the load. In view of these facts it is universally acknowledged that the operation of large power plants of any character during periods of light loads, is enormously wasteful. Many have hoped that the supplying of current to small electric motors during the day would remedy the evil in lighting stations, and to some extent the expectation has been realized in manufacturing centres, but even here the unfortunate overlapping of the motor and lamp service still leaves an undesirable margin.

Here then is a field in which accumulators may perform their special function; not in competition with direct systems, but as supplying the missing feature in the commercially successful operation of most 24 hour plants of moderate size. There is no obstacle in the way of applying a sufficient number of accumulators to a plant of the character mentioned, to maintain the light load during the time it would not pay to operate the power plant, and to provide for the charging of the accumulators during the regular running time of the dynamos. Assuming, in illustration of this statement, that 10 hours is the ordinary working day, and that the engines of a central lighting station are started at 2 P. M. and are operated until midnight, it is probable, if not certain, that there will be sufficient surplus current for an ample length of time, to charge enough accumulators for the service required after the engines cease to revolve; excluding, of course, the period of maximum load, when the dynamos will be taxed to their full capacity in their regular service. Under such con-

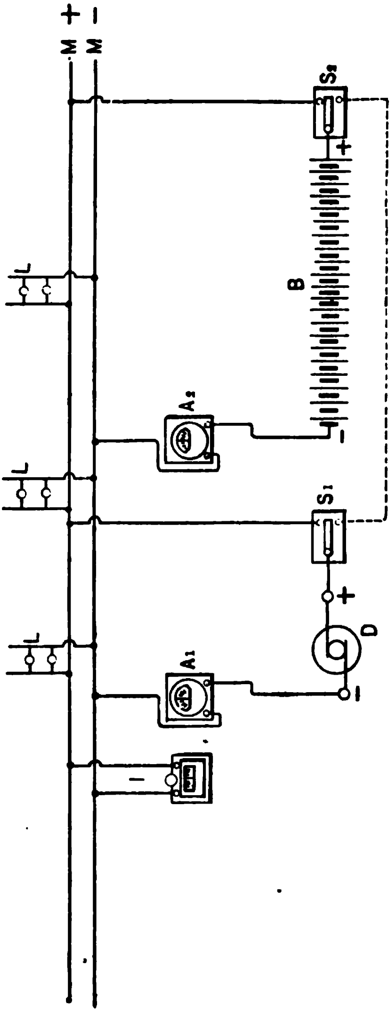


Fig. 1

ditions the engines could be operated at somewhere near their normal capacity during the whole period of their running time, and the cost of unit power would be a minimum; while only one staff of men will be required. It may be advisable to explain here that although the current available for charging the batteries during the regular running time of the dynamos would vary with the lamp load, this is of no importance as it is not essential that the batteries should be charged at a uniform rate. In the case of such stations as also supply arc lights, the engines could be started later and operated through the night. Besides reducing the operating expenses of a station during its periods of light-load to a greater extent than they increase them at other times, in the event of an accident to the power plant or incandescent dynamos, the accumulators can always be called upon for extra service; while whenever there is an abnormally heavy demand for current they may also be operated in conjunction with the dynamos.

But there is still another field in which accumulators may be employed to equal advantage, viz.:—that of long-distance lighting now so successfully occupied by the alternator-converter system. An accumulator is indeed merely a chemical converter, which is unequaled as a pressure-reducer, and any one who gives careful thought to the subject will perceive what an excellent substitute it is for the alternator-converter in those situations where the character of the lighting makes it necessary to run "light" for many hours each day. Indeed I can only account for the fact that accumulators were not long since more extensively used for this class of lighting, except by the existence of a feeling of skepticism as to their durability, together with their somewhat formidable first cost.

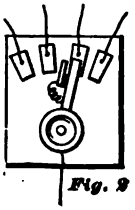


Fig. 9

As manufacturers now guarantee the durability of accumulators, under certain conditions of use, for an annual percentage on their first cost; and as methods of manufacture must improve with experience and moderate that cost, both of these objections are gradually being modified.

But even in the present state of the art, the question of first cost, depreciation and efficiency may be duly allowed for, and a considerable reduction in operating expenses still shown to result from the use of accumulators under many circumstances. To quote from an article by the writer "On the Place of Accumulators in Electric Lighting," which was published in the March issue of *The Electrical Engineer*:—

"Whenever one kind of energy is transformed into another kind, more or less loss can be shown to occur, and the action of the accumulator is not an exception to this rule. The proportion of loss generally varies with the conditions under which the transformation is effected, and in the case of the accumulator depends mainly upon the rate of charge and discharge. If these double transformations are conducted slowly the loss may be nearly inappreciable, but at the higher rates which are frequently demanded in practice the efficiency is proportionately reduced. The fact of the existence of this loss clearly indicates that the direct production and consumption of electricity must always be less expensive, both in original outlay and cost of production, than its subsequent use through the medium of accumulators; unless, indeed, there are other modifying conditions which over balance the additional cost of accumulators and offset their maintenance. That such conditions do exist in many branches of the electric lighting industry can be readily demonstrated, and it may likewise be shown that the use of accumulators as auxiliaries to many existing lighting plants would insure a marked reduction in their running expenses.

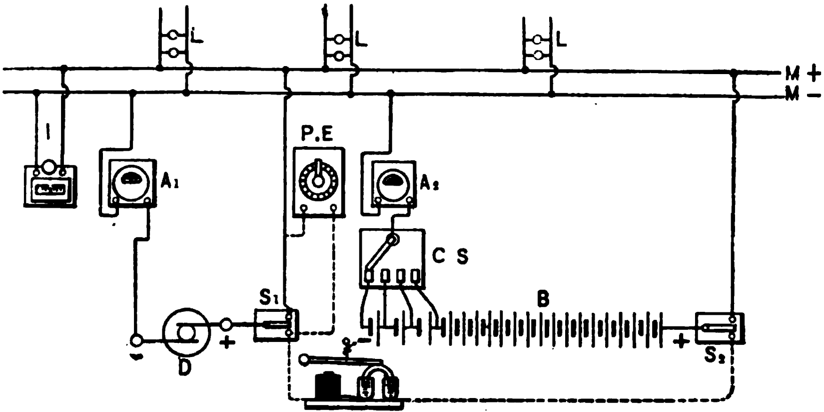


Fig. 3

*   *   *   *   *   *   *

"From what has already been said regarding the infallible certainty of loss resulting from any form of conversion of energy, it is evident that if the load were equal to the capacity of a plant for 24 hours each day no system of conversion could compete in point of economy with one which effected the direct production and use of the current. There is an exception even to this general statement, however, when one or more groups of lamps are located at such a great distance from the source of power that the interest on the cost of conductors, together with the cost of the energy wasted in them, becomes of serious importance: not to mention the increased difficulties of regulation. In cases of this character, the value of the accumulator as an auxiliary to a direct incandescent system becomes obvious. The cells may be located directly at the centres of consumption and charged by a moderate current at high pressure over a small conductor. They may be charged during the hours of light load and discharged as required, the whole charging current, in addition to the current of the batteries, being available on the lamp circuits, during the period of maximum load, if it should be so desired. Often the interest on the cost of such an accumulator installation, plus the cost of the energy lost in conversion of current, is much less than the loss by the direct system as previously specified. Another and vastly important consideration in favor of the accumulators is that during the 10 or 12 hours of each day when the consumption of current is so small that a station can only be operated at

Fig. 4.

a loss, the power could be shut off entirely and the accumulators resorted to for the current required during that period."

As a matter of fact, the ideal condition of a continuous maximum load, as assumed above, nowhere exists in practice. The actual load curves for stations of similar capacity throughout the country differ not only in respect to the duration of the maximum period, but likewise in the hours during which the light and heavy loads occur, according to the character of the lighting. It is nevertheless true that they all show a remarkable similarity in the variation of current consumption, and all the 24 hour stations, particularly the smaller ones, require but a very small portion of the capacity of the plant for the greater part of the day. For this reason many of the smaller stations cannot be profitably operated for more than 12 hours a day, and customers are therefore unable to obtain the light for the remainder of the 24 hours, the system being thereby deprived of much of its value to many consumers.

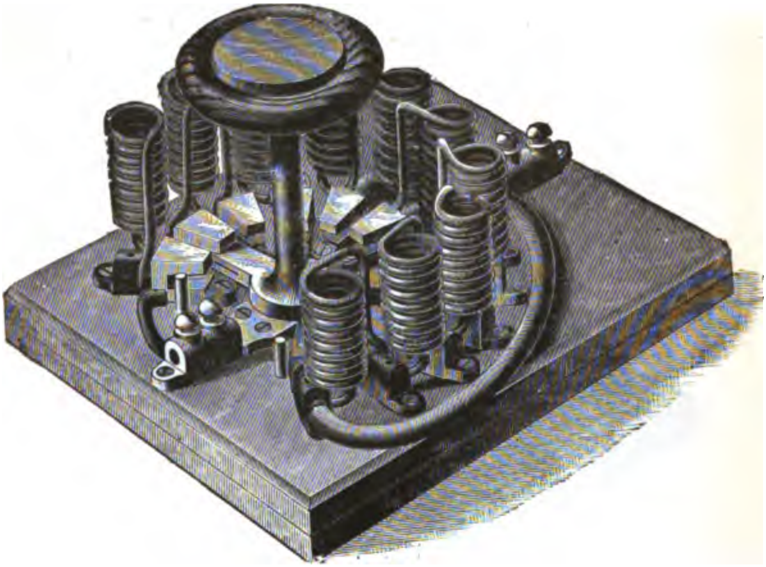


Fig. 5.

Here again the utility of the accumulator for tiding over these costly periods of light loads becomes apparent. It is evident that any ordinary station running for 12 hours a day, more or less, is not likely to have an output approaching its capacity for more than two hours at the longest, while for the greater part of the time it will be much below it. Why should not such a station be operated during that period of 12 hours at somewhere near its full capacity, and, therefore, under the most economical

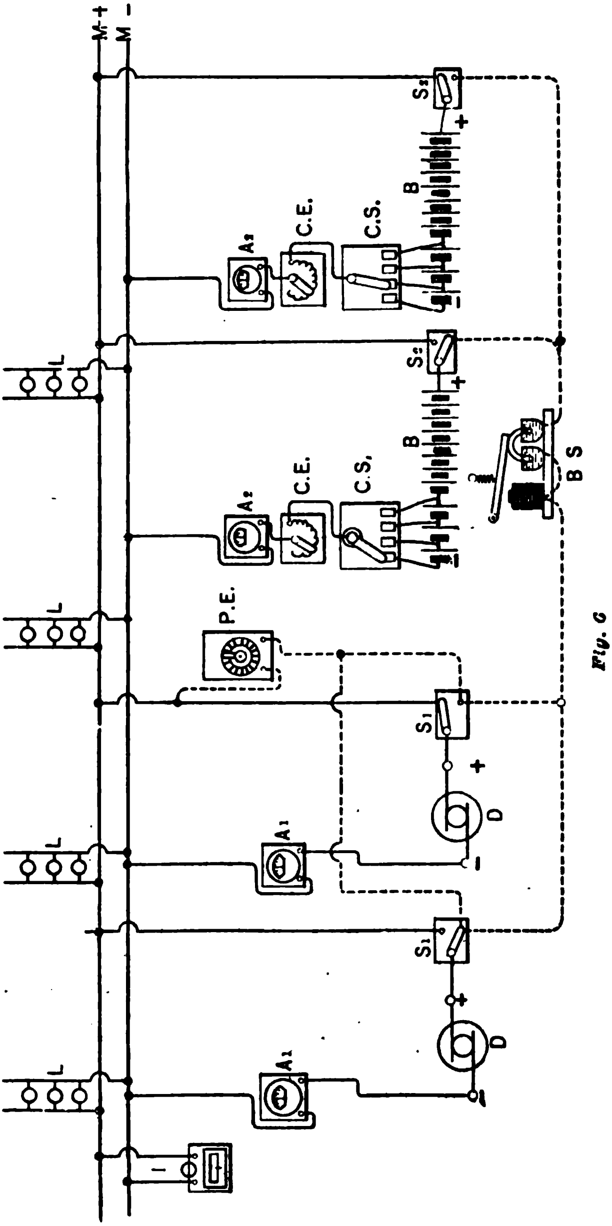


Fig. 6

conditions for the power produced, and utilize the excess of current to charge a sufficient number of accumulators to maintain the lighter load during the remainder of the day?

This is the legitimate work of accumulators, and when they are properly installed and maintained to meet such conditions, the class of small stations referred to will be enabled to supply satisfactory light for 24 hours a day at moderate cost, while the larger stations may greatly reduce their running expenses and at the same time maintain the efficiency of their service."

Before proceeding to consider the methods of regulating accumulator currents, it will be advisable to examine some of the peculiarities of the element with which we have to deal. Generally speaking, the total current capacity, expressed in ampere-hours, of a single cell of accumulator of the lead lead-oxide type, is proportional to the number and size of its plates; its rate of discharge depending upon the number of plates and the effective surface of each, while the time of such discharge varies with their thickness. Although there are no obvious theoretical reasons why a single cell of accumulator should not be made sufficiently large to possess any desired capacity, there are mechanical considerations which make it advisable to limit the dimensions of a cell to the extent that it may be conveniently portable. Therefore when higher rates or longer discharges than an ordinary cell will give are demanded, two or more cells must be connected in parallel. On the other hand the electromotive force of all sizes of accumulators composed of the same elements is of course identical, but as the internal resistance of a cell varies with the number and surface of its plates, its effective working potential must be a function of the strength of the discharge current. As, however, the internal resistance of all sizes of accumulators is, in virtue of the large surface and compactness of their electrodes, exceedingly small in comparison with their of discharge, their working potential nearly equals their omotive force on open circuit. While the normal effective

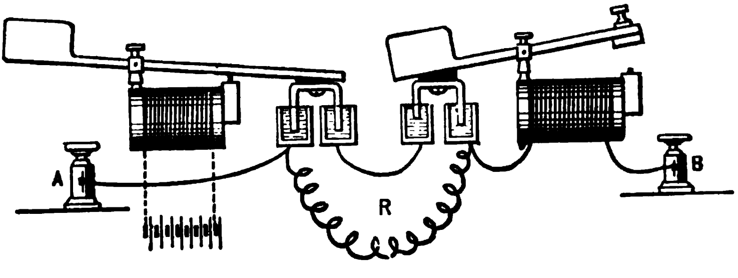


Fig. 7

working potential of a fully charged accumulator in good condition is usually stated as about two volts, as a matter of fact it is somewhat higher than this after being charged and rather lower after normal discharge, the average effective potential being about 1.95 volts. During the operation of charging an accumulator, its potential, or what is then usually called its counter electromotive force, rises gradually until the cell is nearly charged, and then more suddenly as gas is evolved, sometimes requiring an effective charging pressure of as high as 2.5 volts per cell if the current is continued after the cell is charged. In actual practice the accumulator is usually considered to be fully charged when the potential of the normal charging current reaches 2.3 volts per cell, or at 2.2 to 2.25 volts per cell when the charging rate is reduced as gas begins to be freely evolved towards the end. Similarly when a cell is discharged at the normal rate its effective potential falls during the progress of the discharge from 2 volts to 1.8 volts, at which latter point it is considered to be discharged to its normal limit.

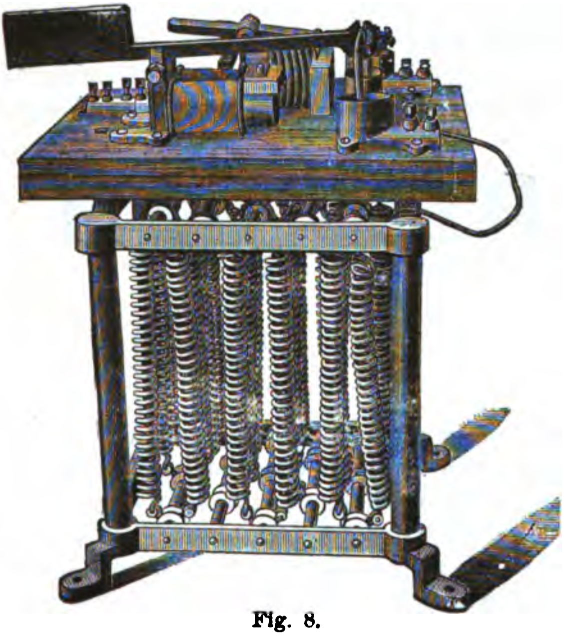


Fig. 8.

The facts above cited to the effect that the electromotive force of accumulators rises during charge, and falls during discharge and that their capacity for charge and discharge is limited, are the key notes to the regulative processes; and only one other point need be here considered. When two or more series of cells connected in parallel are to be charged at the same potential, it is evident that, unless each series is in precisely the same state in respect to residual charge, there will be a difference in their electromotive forces; and, in consequence, less current will flow in those series having higher potentials than others. While the larger current flowing into the less charged cells will have a tendency to bring up their potentials to the average, it is found in practice that some series will become fully charged sooner than others. The simple means provided for compensating for these variations will be duly described.

One of the commonest, and perhaps the simplest applications of accumulators to lighting work is found in their employment in connection with direct isolated plants in factories, office buildings, etc. In illustration of the method of applying accumulators in such cases, we may select as a type of this class of lighting one of those office buildings common in New York and other large cities. This building, we may assume, has already been, or is about to be, wired for 500 16-candle, 100-volt, ½-ampere lamps on the multiple-arc plan, and is to be provided with a 125-volt, 200-ampere dynamo; it being calculated that more than 400 lamps will rarely be lighted simultaneously. We are not specially concerned with the power plant, and will simply assume that it is of ample capacity, it being remembered that such buildings are usually steam heated, and therefore, offer favorable conditions for the operation of a plant of the character under consideration. Now it can be deduced from experience that, during the day, from say nine o'clock in the morning until about sunset, only a limited number of lights will be burned in certain dark corners of the building, and that as twilight and darkness come on the load will gradually increase, reaching a maximum at a certain hour depending upon the season; subsequently the load

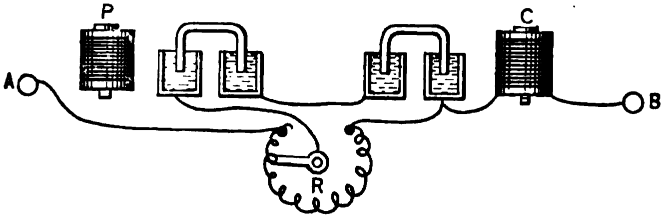


Fig. 9

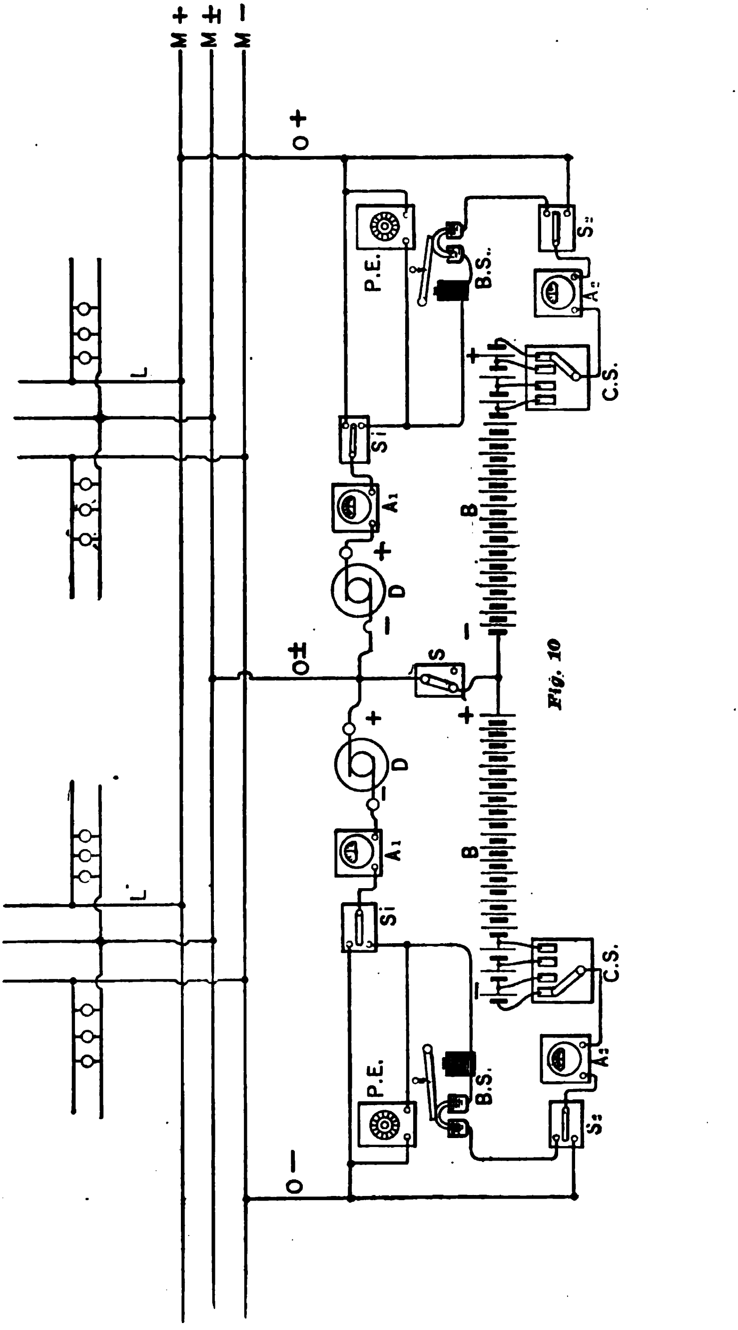


Fig. 10

will decrease, finally reaching a minimum after the janitor and his assistants have finished their cleaning operations, which load will probably be maintained for the remainder of the 24 hours. It is evident that a direct plant operating under such conditions would necessitate the employment of two forces of men, either

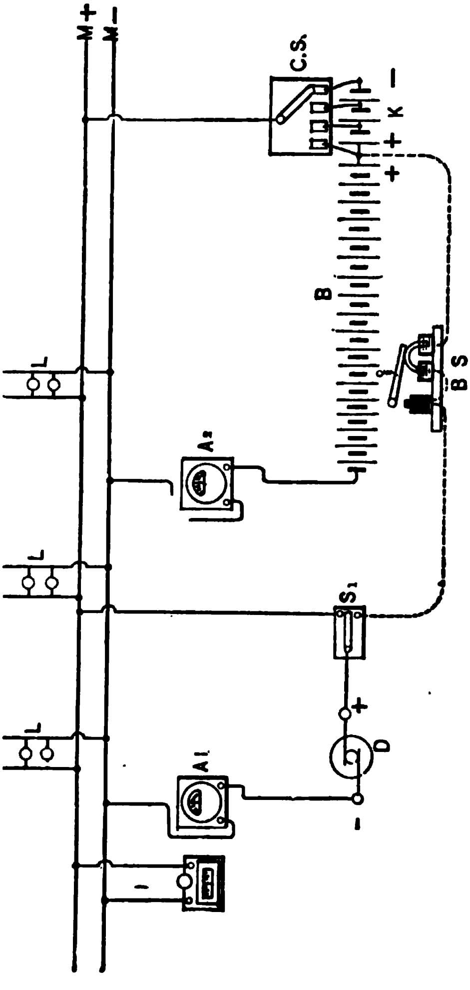


Fig. 11

one or two in each, and would, moreover, be running under exceedingly uneconomical conditions for a large part of the time. Let us now consider in what manner accumulators may be added to this plant in order that it may supply the variable load for 24 hours daily, while at the same time dispensing with the services of one staff of men and reducing the running time of the engine to eight hours.

It has been shown in a general way how the load varies during 24 hours; but in order to ascertain the capacity of the accumulators required, it will be necessary to assume, though only approximately, somewhat more precise figures. Say that the load is as follows:—

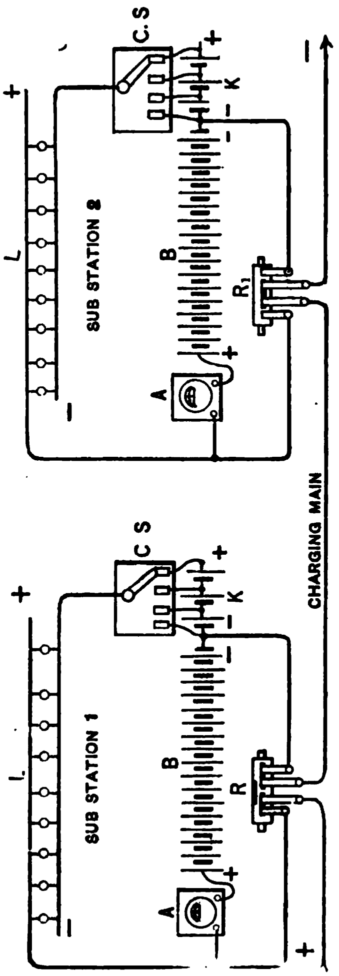


Fig. 12

From 9 P. M. to 9 A. M., 20 lamps — 120 ampere-hours.
" 9 A. M. to 4 P. M., 200 " — 700 " "
" 4 P. M. to 6 P. M., 400 " — 400 " "
" 6 P. M. to 9 P. M., 50 " — 75 " "

An inspection of the above schedule shows that for 12 hours out of the 24, only 20 lamps, or 10 amperes, are used, while for three hours more only 50 lamps, or 25 amperes, are required; and a simple calculation proves that a set of accumulators having a capacity of 200 ampere-hours will be amply sufficient to maintain the light lamp load for 14 hours out of the 24. If the dynamo is started at 8 A. M. and operated until 6 P. M., while maintaining the required number of lamps during that period, it will still have surplus current for charging the battery as follows:—

From 8 A. M. to 9 A. M., 190 amperes for 1 hour,
" 9 A. M. to 4 P. M., 100 " " 7 hours,

or 890 ampere-hours, an available capacity vastly in excess of the requirements. Now, if during the eight hours in which the dynamo is operating, the accumulators are charged at the rate of 30 amperes, in that period they will receive a total charge of 240 ampere-hours. According to the schedule, the maximum output required from the battery will be 185 ampere-hours, whence it follows that the charge received by it is more than ample, even after making the customary allowance of 20 per cent. for loss by conversion; and the desired result has been accomplished.

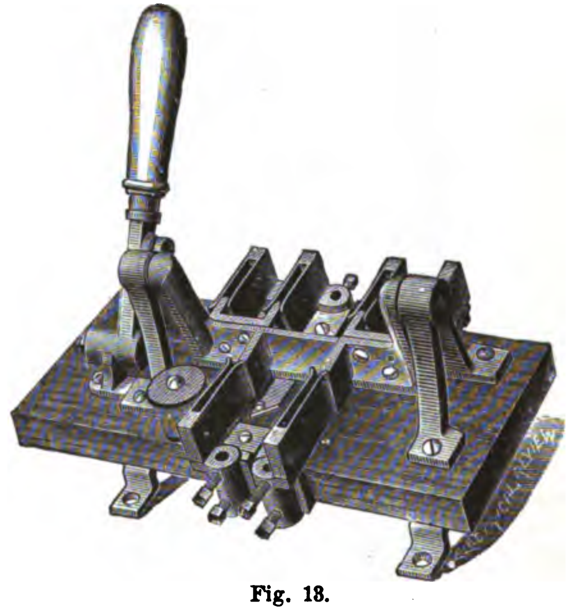


Fig. 18.

We have now to consider the details pertaining to the practical arrangements of such an installation. According to the stipulated schedule, after the dynamo ceases to run at 6 P. M., the battery alone must supply 25 amperes for three hours, and thereafter 10 amperes for 11 hours, or a total output of 185 ampere-hours. One series of 50 cells, having say a normal capacity of 30 amperes for eight hours, will satisfy these requirements, and will be well within the nominal rating of ordinary commercial cells. Assuming that these 50 cells, connected in series, have been suitably placed on insulated shelving at any convenient distance from the dynamo, they may be electrically connected with the latter and with the lamp circuit, as shown diagrammatically in Fig. 1.

In this diagram the two parallel wires M (+), and M (—), represent the common dynamo and lamp mains to which the pressure indicator, I, is permanently connected in the usual manner;

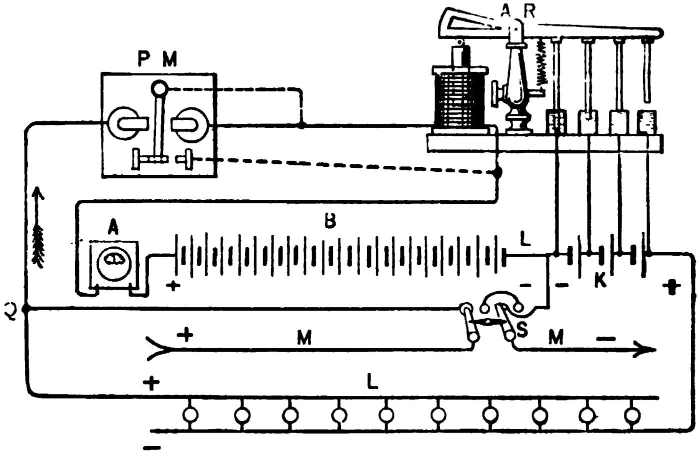


Fig. 14

and L L L represent the lamp circuits. The dynamo, D, is connected to the mains through the ammeter, A_1, on one side, and through the upper contact of the two-way switch, S_1, on the other. These are the ordinary connections of a multiple arc plant; and the (+) and (—) terminals of the accumulator battery, B, are also connected with the mains through the ammeter, A_2, and two-way switch, S_2, in precisely the same way. It is evident that if the levers of both switches, S_1, S_2, are against their upper contacts, the dynamo and battery will supply current to the lamp circuits in exactly the same manner as would two dynamos connected in parallel, provided, of course, that they are both at the same potential. On the other hand, if the potential of the dynamo slightly exceeds that of the battery, the current from the former will divide between the battery and the lamp circuit in a certain proportion determined by the ratio of the resistance of the lamp circuit to the internal resistance and counter electromotive force of the battery. If the levers of the two switches, S_1, S_2, are now moved to their lower contacts, the dynamo and battery will be connected in series with their like poles opposed, the main, M (—), acting as part of the circuit; and supposing that the pressure of the dynamo still exceeds that of the battery, the latter will receive a charge. Thus, by this very simple arrangement, the dynamo and the battery, either separately or together, may be connected with the lamp circuit, or with each other, or be entirely disconnected.

While this method satisfies all but one requirement in certain classes of plants where it is convenient to employ the dynamo solely for charging the battery during a part of the day, and to use either the dynamo or battery, or both, on the lamp circuits at other times, it does not, in fact, satisfactorily permit of that splitting of the dynamo current between the lamps and battery which was incidentally referred to. More than this: it fails to provide a means of compensating for that rise and fall of the potential of the battery during the charge and discharge which has been mentioned; and this is the unsatisfied requirement referred to above. The reason why the dynamo cannot satisfactorily divide its current between the battery and lamps when connected in the manner illustrated in Fig. 1 has already been pointed out. During the charging of the battery its potential will rise to 2.2 volts per cell, so that the pressure of the dynamo must be raised to 110 volts in the case now being considered, and this excessive pressure would endanger the life of the lamps. How, then, will it be possible to employ the higher pressure demanded by the battery and at the same time supply current to a number of lower voltage lamps without raising their candle-power above normal? The answer is easily given, for it is only necessary to insert a suitable resistance in the main between the dynamo and lamp circuits in order to accomplish the desired result. Such a resistance, usually called a "pressure equalizer," should be made of wire sufficiently large to safely carry the current for the greatest number of lamps likely to be required at the time when the dynamo which supplies them is to be simultaneously called upon to charge the battery, and should be made conveniently adjustable, as the fall of potential through it varies with the current. The method of regulating the working potential of the battery is equally simple, for it merely consists in adding to, or substracting from, the number of active cells in circuit. This is accomplished by means of a multiple-point switch, called a cell-regulating switch, shown diagrammatically in Fig. 2, which is so constructed that, in the act of shifting the cells in and out of circuit, it neither interrupts the circuit nor short-circuits the cells. It consists essentially of a single pivoted lever, which carries on its outer end a short metallic arm. This arm is attached to the lever by means of a block of insulating material, but is electrically connected with it by a short spiral of german-silver wire. The lever and arm may be made to pass over a number of contact strips, which are so arranged that before the lever breaks contact at one strip, the arm comes in contact with the next strip, and the reverse action takes place when the lever is moved in the opposite direction. During the brief interval while both lever and bar are in contact with adjacent strips the cell connected to those strips discharges a feeble current through the spiral of wire.

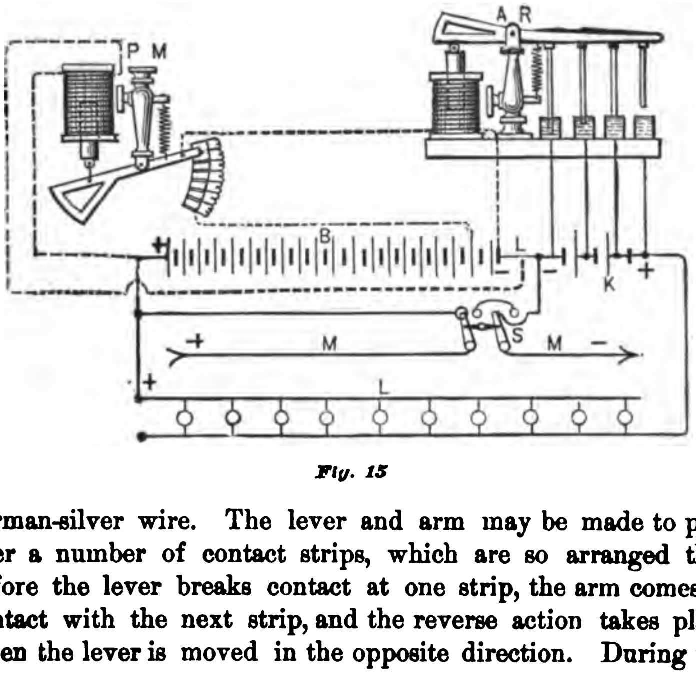


Fig. 15

It is necessary to mention here, that in practice it is customary to provide for the installation of 10 per cent. more cells in each series, than a simple calculation on the basis of two volts per cell would show to be necessary. Thus a plant using 100 volt lamps would require 100 ÷ 2 = 50 cells + 10 per cent. of 50 = 5, or 55 cells in each series. This allowance is usually sufficient to compensate for the fall of potential during discharge, as well as to provide for the ordinary loss of potential in the lamp mains.

The method of employing the pressure equalizer and cell-regulating switch is shown in Fig. 3, this diagram being otherwise similar to Fig. 1, with the further exception that an automatic break switch, B S, is inserted in the branch wire connecting the dynamo and battery through the lower contacts of the switches S_1, S_2. The function of the automatic break switch is merely to interrupt the charging current, in the event of the potential of the dynamo becoming so much reduced from any cause as to allow the battery to overcome it, and perhaps reverse its polarity. This switch, a practical form of which is shown in Fig. 4, consists of a simple electro-magnet with a weighted armature lever carrying at one end an ∩-shaped bent wire dipping into mercury cups. The charging current passes through this magnet and from one mercury cup to the other through the bent wire. When the current becomes greatly reduced to any predetermined extent, gravity overpowers the attractive force of the magnet, its armature is released and the bent wire being drawn out of the mercury cups the circuit is broken. By referring to the diagram in Fig. 3, it will be seen that the pressure equalizer, P E, has been so placed that when the switches S_1 S_2 are on their lower contacts, the current from the (+) pole of the dynamo will divide at switch S_1, part going through the battery and part through the pressure equalizer to the lamp circuits, both currents again uniting at the main, M (—), to return to the (—) pole of the dynamo. By this arrangement the potential of the dynamo may be adjusted to the requirements of the battery, and at the same time the pressure at the lamps suitably reduced by adjusting the resistance of the pressure equalizer.

When the proper balance is once obtained. and by means of this apparatus it may be quickly and easily accomplished by one man, no further attention than would ordinarily be given to the dynamo would be necessary, for although the counter potential of the battery would gradually rise toward the end of the charge and the current passing into the battery become correspondingly reduced, this result is even desirable, as it is somewhat advantageous to reduce the strength of the charging current as the battery becomes filled. In some installations, however, the extra or regulating cells are used to a more limited extent than the others, and thus having had less current taken from them they become sufficiently charged in a shorter time. A simple movement of the cell-regulating switch will then serve to cut out of circuit one or more of the charged cells as may be required, while the others receive current for a longer time. In some cases a further adjustment of the field rheostat of the dynamo and of the pressure equalizer may afterwards be required, and if so are quickly effected.

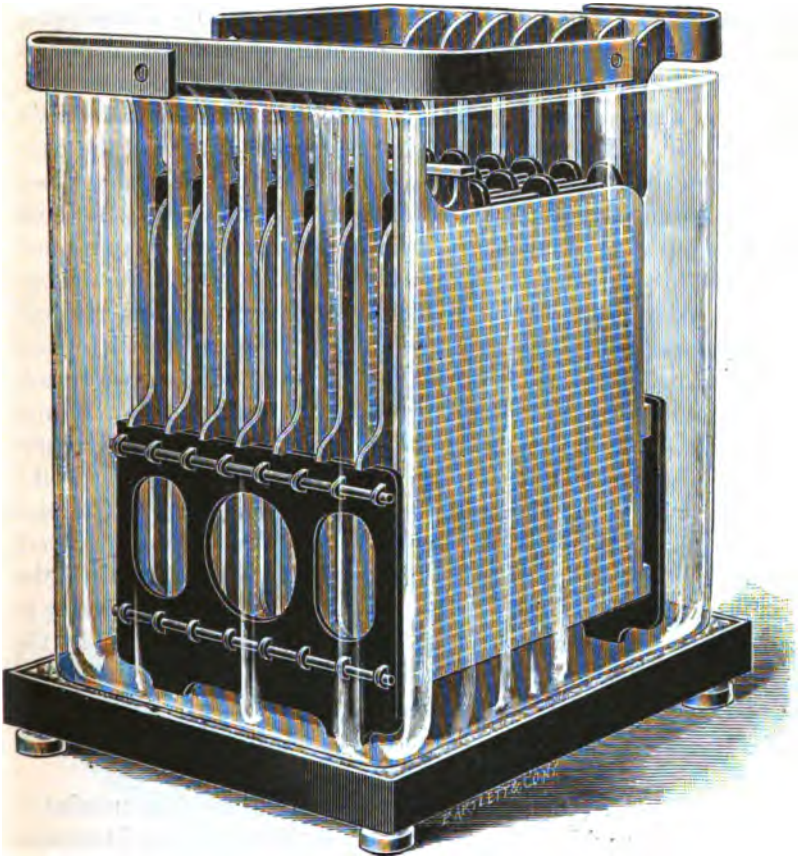


Fig. 16.

The first indication that the cells have received nearly enough charge will be given by the gentle evolution of gas which gradually increases thereafter, and which should never be allowed to become violent. At the same time the potential of the battery will approach 2.2 volts per cell, and even reach above 2.3 volts per cell when the gassing becomes marked. In order to take advantage of this indication of charge it will be sufficient to have a voltmeter conveniently located, and connected with the terminals of the battery, by means of which instrument, the number of cells in the battery being known, the potential per cell is readily ascertained. While this increase in potential is taking place, a similar variation in the density or specific gravity of the electrolyte in the cells also occurs, but this change, unlike that of the potential, remains fixed, even after the charging current is discontinued. That is to say, if the specific gravity of the liquid was 1.160 when the cells were normally discharged, it would rise to 1.190 when they were fully charged, and would remain at that for a considerable length of time if no current was drawn from them. This rise of 0.030 in the specific gravity of the electrolyte is about the average increase in density which takes place in the common types and sizes of accumulators when 1.160 acid is used in the original charge. and when the range is from normal discharge to full charge. It often happens, however, that charging is commenced when the cells have more or less residual charge, and in this case the rise in specific gravity will be correspondingly less; while on the other hand it will be correspondingly greater if the cells were previously over-discharged. Again, if unusually large containing cells enclose the piles (as the aggregation of electrodes is commonly designated) the rise of specific gravity will be smaller, and on the other hand, larger if unusually small containing cells are employed. As heretofore stated, a similar drop in specific gravity and potential occurs when the cells are discharged, which in degree has about the same value as the rise. This variation in specific gravity is explained by the formation of lead sulphate during discharge which implies the absorption of acid from the electrolyte, and by the reduction of this sulphate during the charge when the electrolyte is strengthened.

While the method of installing a single series of accumulators in connection with an isolated plant, as described and illustrated in Fig. 3, fulfils most of the conditions required in small plants operating a single dynamo, in larger plants of a similar type using two or more dynamos, several series of the largest cells may be required. Although such an installation would consist mainly of an amplification of the system already described, still there exists a new difficulty not met with in that system. This arises from the fact already mentioned, that when several series of cells are charged in parallel, any essential difference in the amount of the residual charge in the several series, results in their allowing different amounts of current to flow through them, although all of the series are charged at the same potential. The remedy for this undesirable result is of the same character as that employed to prevent excessive pressure on the lamp circuits, when the dynamo which supplies them with current is at the same time used to charge accumulators. In circuit with each series of cells is placed a small adjustable resistance coil, called a current equalizer, a cheap practical form of which is shown in Fig. 5. It is composed of wire large enough to carry the maximum current of a single series of cells, and usually has a resistance of from $\frac{3}{10}$ to $\frac{6}{10}$ of an ohm. Each series of cells is also provided with an ammeter, and upon the commencement of a charge the current equalizers are so adjusted that each ammeter shows the same

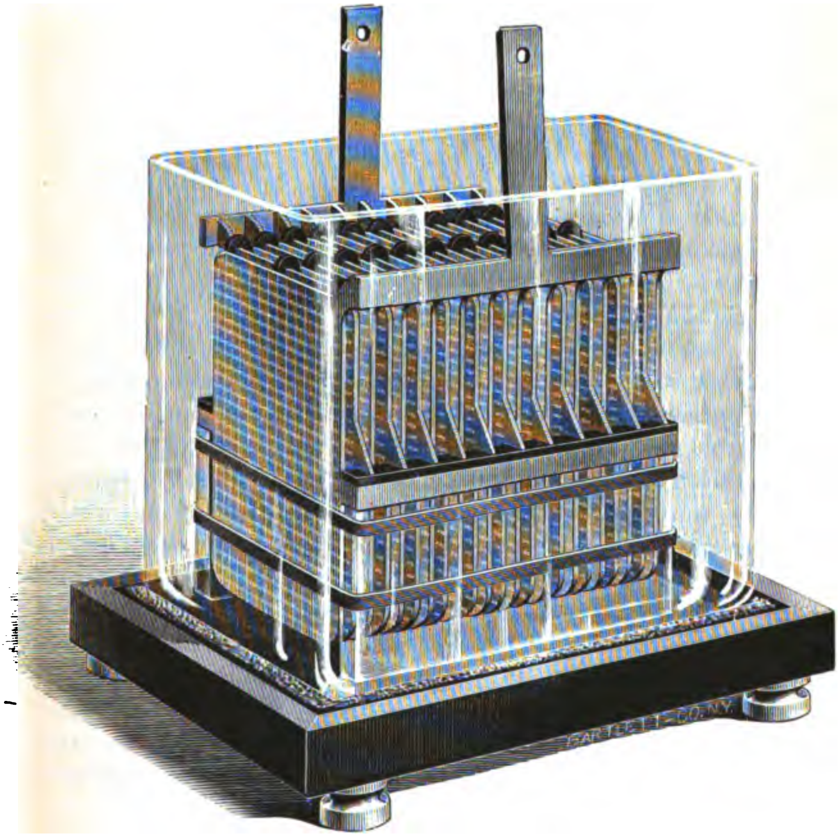


Fig. 17.

amount of current to be passing through each series. In order to avoid as much as possible any loss of energy in the current equalizers, they are all turned to the non-resistance point at the start, and resistance is then only inserted in such series as may be taking more current than others.

The general arrangement of an accumulator plant consisting of two dynamos and two sets of batteries is illustrated in Fig. 6, and a simple extension of this plan only is necessary to adapt any number of series of cells to a plant having any number of dynamos. It will be seen that one equalizer circuit and one charging circuit with its automatic cut out, are common to all the dynamos that may be used. While this arrangement possesses all the flexibility of the method of installing a single series of cells as shown in Fig. 3, it has the additional advantage that at such times as the load is below normal, a spare dynamo can be exclusively employed for charging the cells, and thus avoid loss of energy in the pressure equalizer. As a general thing when the current equalizers, C E, have once been adjusted at the beginning of a charge they need but little if any further attention, still if from any cause one series of cells should happen to be overdischarged, or discharged more than the remaining series a suitable readjustment of the current equalizers will permit the undercharged series to receive current at a higher rate than the others, and thus equalize itself with the others. It may also be mentioned that both the pressure and current equalizers may be made to operate automatically by the application of a potential magnet to the former, and of a current magnet to the latter, together with a simple but somewhat expensive train of gears to move their levers. This is found to be wholly unnecessary in practice, however, as an engineer is usually in constant charge of such a plant while the dynamos are running, and he can easily devote the little time necessary to the infrequent regulation required.

It was mentioned in the early part of this paper that it was possible to injure accumulators either by overdischarging them or working them at too high a rate. It is assumed that, in the larger plants, there will be such supervision available under ordinary working conditions that occurrences of this kind will be avoided; but a simple device for preventing such abuse of the cells in smaller plants, where such supervision is not always convenient, has been devised. It is called an overload and over discharge switch, and its operation is based on the fact that, when a series of cells is overdischarged, their potential falls below a minimum value, and that when they are worked above their normal rate the current exceeds a maximum value. The apparatus consists essentially of a pressure magnet connected to the terminals of the battery, and of a current magnet in series with the battery; when the potential of the battery falls below a minimum value, the armature of the potential magnet is released and throws an artificial resistance into the battery circuit, and

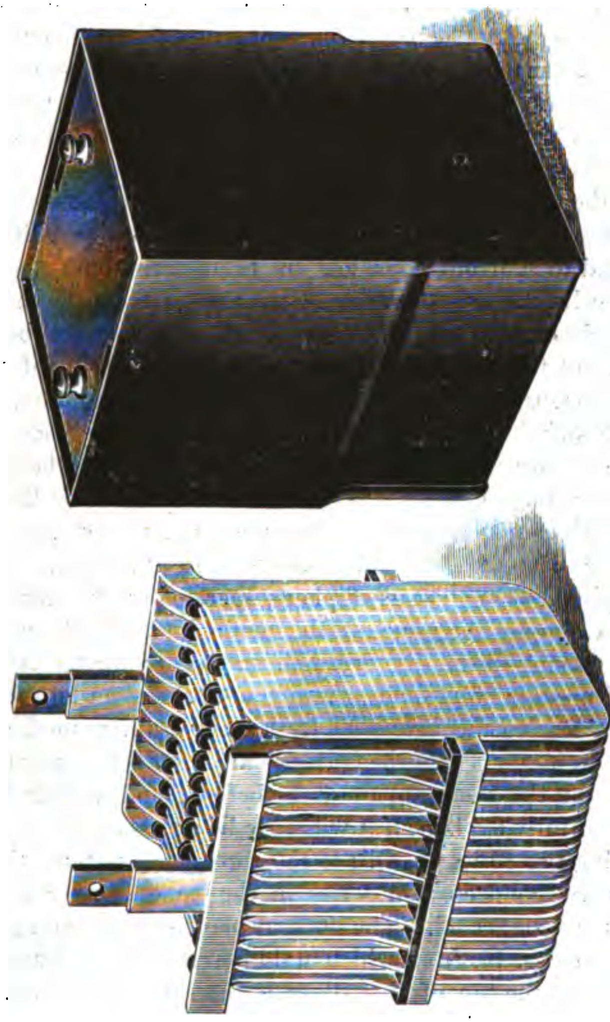


Fig 18

thus reduces the current; and similarly, when the current exceeds a maximum value, the armature of the current magnet is attracted and performs a similar function. The connections of this overload and overdischarge switch, C and P, are shown diagrammatically in Fig. 7, in which a common resistance, R, serves for either magnet. The battery current passes from A to B through the overload magnet, while the terminals of the overdischarge magnet are, as before stated, in shunt to the battery terminals. In Fig. 8 is illustrated a practical form of a combined overload and overdischarge switch.

With the object of reducing the cost of the regulating apparatus whenever both the combined overload and overdischarge switch and the current equalizer are to be used with the same series of cells, the switch portion of the equalizer may be mounted on the resistance frame; and thus a single resistance coil be made to do service for both instruments. This combination, of which the connections are shown in Fig. 9, while allowing the current equalizer switch to control the full range of resistance, at the same time causes the whole coil to be thrown into the battery circuit upon the operation of either the overload or over discharge switch. This, of course, diminishes the brilliancy of the lamps, and thus indicates the state of affairs to those interested.

In applying accumulators to plants operated on the three-wire system, a simple extension of the method already described is adopted, except in the case of small stations requiring only one series of accumulators, and where the working circuits can be multiplied during the light load, so that the ordinary method will suffice. In larger plants the batteries are installed in pairs, each series having the same apparatus as before; and as this has already been fully explained, it will only be necessary to add that, in adapting two series of cells to the three-wire system, the two batteries are connected in series in the same manner that the dynamos are connected. The general plan of this arrangement is shown in Fig. 10, from which unimportant details have been omitted. At the top of this diagram the three horizontal lines M (+), M (±), and M (—) represent the omnibus wires in the dynamo room, from which the feeders, L, are led. The three vertical lines, O (+), O (±), and O (—), are extensions to the omnibus wires to which the dynamos are directly connected in the usual manner when the levers of the switches, S_1, are on their upper contacts. The batteries are also connected with the omnibus wires in a similar way, so that when the switches, s_2, are closed on their lower contacts the batteries will also discharge into the lamp circuits in parallel with the dynamos. Now, if the switches, s_1, are turned to their lower contacts, and the switches, s_2, to their upper contacts, the current from the dynamos will divide at the switches, s_1, part going into the lamp circuits, and part through the batteries, which thus receive a charge. As in the multiple-arc system, the potential of the dynamos being assumed to be raised above the normal to meet the requirements of the batteries, in order to suitably reduce the pressure on the lamp circuits, it is only necessary to adjust the resistance of the pressure equalizer, P E. In central stations where the feeders are already provided with pressure equalizers the connections are still more simple, for the batteries are merely connected to the omnibus wires, while all other connections of the ordinary three wire station are unchanged. In such cases, the increased pressure at which the dynamos are operated during the time the batteries are charging, is reduced at the lamps to the proper point by the usual adjustment of the feeder or pressure equalizers. During the charging period all regulation of the accumulators may be dispensed with by opening the switch s_1, when both batteries will be connected in a single series to the full pressure of both dynamos.

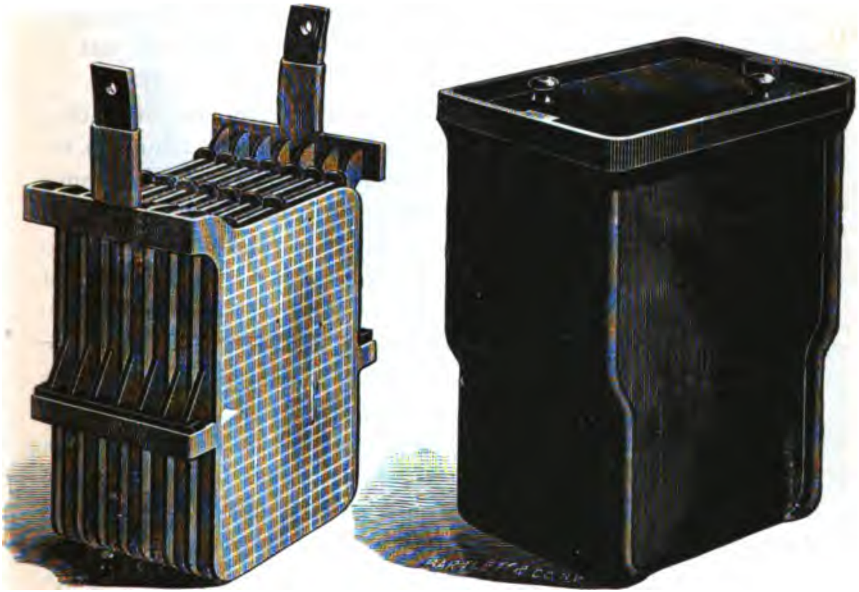


Fig. 19.

In addition to the apparatus shown in the diagram, each series of cells is provided with a double plug switch and sockets suitably disposed, by means of which the relative positions of the batteries on the two sides of the system may be changed by simply transposing the plugs from one socket to the other. The object of this arrangement is to provide means for compensating for the unequal discharge of the batteries when the two sides of the lamp system are unbalanced; and the transposition is never made oftener than once each day. In all other respects the manipulation of accumulators when applied to three-wire systems is practically the same as when they are operated on the multiple-arc plan. To increase the accumulator capacity of three-wire stations, double batteries are added in parallel to the first set, just as additional dynamos would be.

It was explained further back that the extra, or regulating cells, in each series often became charged sooner than the remainder of the cells, and required to be removed from the circuit before the others. This operation, of course, calls for some labor; and, little as it is, it would still be desirable to have the number of cells in a series remain fixed both during charge and discharge. By the use of what are called counter-electromotive-force cells, this result may be effected, and at the same time the pressure equalizer be dispensed with. These very simple cells are made like an ordinary Planté accumulator of plain sheets of an inoxidizible lead alloy and without active material. When a current is passed through them they act as gas voltameters, and while they instantly oppose a counter-electromotive-force of about two volts, they are incapable of producing a current of any appreciable amount or duration on account of their inoxidizible property. The use of counter-electromotive-force cells in place of equalizers is advantageous in several ways, for not only may their internal resistance be made so small as to be practically negligible, but their counter-electromotive-force is as effectual in reducing excessive pressure as a dead wire resistance, while possessing the unequaled advantage that the fall in potential of the current passing through them is unaffected by any variation in the strength of such current.

The method of using these counter-electromotive-force cells is shown in Fig. 11, which represents a single series of accumulators installed in connection with a direct lighting plant operating one dynamo, as first described, and for simplicity and effectiveness this method cannot be well exceeded. The dynamo is connected to the lamp mains, as before, through an ammeter on one side, and through the upper contact of a two-way switch, S, on the other; while the battery, B, is similarly connected to the lamp mains on one side, but on the other the circuit is completed through several counter-electromotive-force cells, K, the number opposed being governed by the position of the cell regulating switch, C S. When the lever of switch S is on its lower contact, the charging circuit is completed in the now familiar manner. The action of a plant arranged in the above way is as follows:—Assuming the lever of the cell-regulating switch to be turned off, or open, as the expression goes, and that the dynamo switch is on its upper contact, the dynamo alone supplies current to the lamp circuits. If, now, the lever of the cell-regulating switch is turned to its left hand contact plate, the battery will be similarly supplying current to the lamp circuits in conjunction with the dynamo, or alone if the dynamo switch is opened. If the dynamo switch is now turned to its lower contact, the dynamo current will divide at the point where it connects between the battery and counter-electromotive-force cells, part going through the battery and back to the dynamo through the (—) lamp main, while the remainder will pass through the counter-electromotive-force cells to the

Fig. 20.

lamps. Thus, while the full pressure of the dynamo current will be effective at the battery terminals, its pressure at the lamps will be less by 2, 4, 6, etc., volts, according as 1, 2, 3, etc., counter-electromotive-force cells are opposed to the passage of the current into the lamps by the position of the lever of the cell-regulating switch.

When the battery has been fully charged and it is desired to stop the dynamo, the latter may be disconnected by opening the switch s_1, when the battery alone will maintain the lamps. As previously explained, the potential of the battery will be a maximum immediately after it has received a charge, so that in this case if the battery has the usual allowance of extra cells its potential will be higher than the lamps require. More or less of the counter E. M. F. cells may now be inserted in the lamp circuit, however, until the potential is suitably adjusted. Although during the greater part of the discharge the E. M. F. of the battery will remain fairly constant, if discharged to its limit the potential will slowly fall towards the end, and this fall must be compensated for by removing one or more of the counter E. M. F. cells from the lamp circuit.

It is to be noted that by means of the above method, the number of cells in the battery proper is fixed and unalterable during both the charge and discharge, and that whenever the dynamo is supplying current to the lamps, its surplus current is always available for charging the battery; moreover, only one adjustment of the counter E. M. F. regulating cells opposed to the lamp current will be required. It is obvious that counter E. M. F. cells may be substituted with equal effectiveness for the equalizers and regulating cells employed in all the previously described systems, and it seems unnecessary to go into any further particulars in order to illustrate the practicability of such systems when properly installed and operated.

We have now to consider another branch of the electric lighting business, in which accumulators also claim a share of attention, viz.:—the supplying of current to lamps located at a considerable distance from the source of power.

The first method to be considered, is known as the half direct plan, in which one or more series of cells are installed in any number of sub-stations conveniently located near centres of lamp consumption. The charging station may be situated wherever economy dictates, without any special reference to the location of the batteries, for although the expense of the charging wire and the cost of the energy wasted in it, cannot be neglected, both these factors must be offset by the saving effected from locating the power station in the most desirable situation. This half direct system is so called because during the period of maximum load half the current is supplied by the charging dynamo, and half by the batteries. At other times the dynamo may be simultaneously charging the batteries and supplying lamps, or the battery alone may be in operation. Each sub-battery station is, in fact, a small central station by itself, and it may contain one or several series of cells, the number of course, depending upon the amount of lighting to be done in its vicinity. A common charging main, like an arc light circuit, passes through each sub-station, and each battery may be inserted in the main or withdrawn from it at will, exactly as arc lamps are cut in and out of circuit. The charging dynamo is usually of high voltage and of moderate current capacity; machines of 500 volts and 15 amperes to 1,200 volts and 40 amperes may be employed in small stations, while in larger ones the latter machines and their circuits may be multiplied. For charging purposes shunt wound dynamos are preferable on account of their non-reversibility, but even series and compound wound machines may be used if suitable precautions are taken. The arrangement of the accumulators and regulating apparatus in each sub-station on the half direct system is essentially the same as the method adopted in the last described multiple-arc system. The connections of two sub-stations installed on this plan are shown in Fig. 12, in which station 1 is shown with its switches in position for both its battery and lamp circuits to re-

Fig. 21.

ceive current from the charging circuit; while station 2 is disconnected from the charging line, which is closed outside of it, and the batteries alone supply the lamp circuits. The switch which is employed to throw the batteries into and out of the charging circuit is of the snap action type, and is sometimes called a consumer's switch. It maintains the integrity of the charging circuit when the batteries are removed from it, and is provided with a spark coil which prevents the opening of the charging line and the consequent formation of the injurious arc which results on the interruption of high potential circuits. A modified practical form of this switch is illustrated in Fig. 13. As shown diagrammatically in the sketch, it consists in the main of four terminal contact springs which bear against an insulated cylinder in which are embedded two rows of metallic contact strips. In one position the two left hand and the two right hand springs are brought into electrical contact, but if the cylinder is rotated slightly the two middle springs will be brought into electrical contact, while the two outer ones are left free, as shown respectively at R and R_1. As before stated, this change is effected rapidly and without breaking the circuit. These switches are usually operated by hand at the beginning and end of a charge, at which time whatever inspection of the cells is necessary is also generally made. By applying the polarized magnet principle to this switch it can be made to operate automatically from the central station through a momentary reversal of the charging current, a principle which has already been applied.

While the method just described of locating the batteries in sub-stations near the lamps to be supplied, reduces the resistance of the supply wires to such an extent that the variation of potential with changes in current strength is unimportant compared with what it would be were all the lamps operated from one central point, under ordinary conditions, still a similar variation does occur from a different cause. We have already seen that the potential of a battery is higher while it is being charged than at other times, and that the greater the charging current the higher the potential of the battery becomes. It is evident that if charging commences when only a very few lamps are burning on a given battery only a small part of the charging current will be required by the lamps, while the greater part will pass through the battery. This will raise the potential of the battery considerably, and as the lamp mains are permanently connected to its terminals, the lamps will receive an excessive pressure. As the number of lamps increases, however, more of the charging current will pass to them and less through the battery, the potential of which thus becomes gradually reduced until it reaches a minimum, when the number of lamps burning becomes so great that the whole output from both the dynamo and battery is required to maintain them. The variation is in the opposite direction, of course, when the lights are diminishing in number. It is evident from the preceding facts that during the period when the charging dynamo is running and the lamp loads are varying, some regulation of the pressure at the lamp mains at each sub-station is required. This regulation is automatically effected by means of the apparatus shown in Fig. 14, in which a solenoid magnet, A R, and a polarized magnet P M, are inserted in the battery circuit. In all other respects the relative positions of the battery, lamps and charging circuits remain unaltered from the arrangement shown in the last diagram. The operation of the apparatus in this sub-station will be as follows:—Assuming the charging dynamo to be delivering a current through the charging main, M, and that the consumer's switch is in the position shown in the diagram, the charging current will pass from M (+) to the point Q, where it will divide, part going through the lamps and the opposed counter-electromotive-force cells, K, to the point I, and thence to the line again, the remaining part of the current passing from point Q through the magnets P M and A R, and out through the battery to point L and line. When the current is flowing in the direction of the arrow, as in the case just cited, the armature of the polarized magnet is moved to its left hand contact as shown, and the plunger of the solenoid magnet is more or less drawn down, according to the strength of the current. This action of the solenoid magnet causes one or more counter-electromotive-force cells to be inserted in the lamp circuit in such a way as to oppose their electromotive force to that of the charging current, while the full pressure of this current is available at the battery terminals. If now the number of lamps is increased to such an extent that not only all of the charging current passes through them, but also that more or less current from the battery joins in parallel with it; then the current from the battery will traverse the polarized magnet in the opposite direction from that previously taken by the charging current, and its armature will be moved against its right hand contact, thus short circuiting the solenoid magnet and the counter-electromotive-force cells, as indicated by the broken line. For, as the sketch shows, the lever of the solenoid magnet is so balanced that when no current is traversing the magnet, its metallic contact rods all dip into their respective mercury cups, and so shunt around the counter-electromotive-force cells with which the latter are in electrical connection. When the lamp load becomes reduced and the charging current is discontinued either by the stoppage of the dynamo, or by its being shunted past that particular battery by a movement of the switch S to the right, the battery alone works into the lamp circuit, and the counter-electromotive-force cells are short circuited.

While the automatic regulator just described, when applied to the half direct system, maintains the pressure at the lamps sufficiently uniform for practical purposes, whether the charging dynamo is operating or not, and although the proximity of the battery to the lamps it supplies, prevents undue variation of potential when the load changes; yet a perfectly automatic method of maintaining a constant potential at the lamp circuits, even where the resistance of the leads is great, would be generally useful. Such a method is illustrated in Fig. 15, in which the battery, counter-electromotive-force cells, lamps and charging circuits occupy the same relative positions as before. The solenoid magnet of the regulator is no longer in series with the battery, however, and is, in fact, entirely disconnected from it, while the polarized magnet is replaced by a second solenoid magnet, P M, wound to high resistance. The armature lever of this second solenoid is so mounted that when its core is attracted or released this lever moves over a series of contact strips which are insulated from each other. Connected with these contact strips are coils of wire of suitable resistance, the whole constituting a simple rheostat, more or less of the wire of which is included in a local circuit, according to the position of the lever which acts as a movable contact. The solenoid magnet of the regulator A R and one or two cells of accumulators are also included in the local circuit referred to, the strength of the current flowing in this circuit depending upon the amount of resistance inserted by the movement of the lever of the pressure magnet P M. The latter magnet is connected directly with the battery terminals, and as the potential of the battery rises and falls the current flowing through this magnet varies correspondingly, causing a similar variation in the current in the local circuit. Thus an increase of pressure at the lamps, which are connected to the battery terminals, indirectly causes the armature of the regulating magnet A B to be attracted, and this in turn opposes one or more counter-electromotive-force cells in the lamp circuit until the pressure again becomes normal. If the pressure at the lamps falls below normal the regulator acts in the reverse way, cutting out the counter-electromotive-force cells until the normal pressure is again restored. If in connection with this method the polarized consumer's switch, previously mentioned, be substituted at s, the operation of such a sub-station will be entirely automatic.

It is proper to mention that the pressure magnet must be very sensitive and requires a delicate adjustment, besides being somewhat costly on account of the excellence of the workmanship required. Although I have only seen the instrument used in an experimental way, its practical application to similar purposes has given satisfactory results.

Under certain circumstances, as, for example, when a lighting station is worked to its full capacity at night but during the day time has ample surplus power, the all-accumulator system may often be applied to increase the capacity of such a station with satisfactory results. In such cases the sub-stations of accumulators are located at distant points in the usual way, the cells being charged during the day and discharged on the lamp circuits at night while the power plant is doing its regular work. The arrangement of the accumulators and regulating apparatus in the sub-stations of the all-accumulator systems is practically the same as when the half direct system is employed, except that a simple transfer switch which transfers the battery from the lamp circuit to the charging circuit, and *vice versa*, is used instead of the consumer's switch.

It is needless to say that all of the methods which have been described of employing accumulators in long-distance lighting may be adapted to existing lighting plants, and that when so adapted the earning capacity of such stations may be considerably increased without extending the capacity of the power plant. For this purpose there will usually be required a special dynamo at the central station, a charging circuit taking in the territory outside of the regular lighting limits, and one or more sub-stations with accumulators.

There are, of course, numerous other ways of utilizing accumulators in central station supply systems, such, for example, as the double battery method now operated in England by the Electrical Power Storage Co., in which the dynamos are kept running for 24 hours daily, duplicate sets of accumulators being alternately inserted in the charging and supply circuits at uniform intervals of time, by an automatic mechanical device. But I have limited this paper to a description of some of the simpler methods of accumulator regulation which may be readily adapted to ordinary lighting plants, and, in concluding, can only hope that I have succeeded to some extent, at least, in showing that when so applied in an intelligent manner accumulators do occupy a useful place in the industry of electric lighting.

Through the courtesy of the Electrical Accumulator Co., I am enabled to present to the Institute this evening the first proofs of some cuts of a new line of standard accumulators recently brought out by that company. The cells referred to are shown in Figs. 16 to 21 inclusive, and their dimensions and capacities are described in the following table:—

LIST OF ELECTRICAL ACCUMULATOR CO'S STANDARD CELLS.

(Approximate Data.)

Figure.	Type of cells.	Charging current.	Working rate and capacity, Amperes.				External Dimensions of jars. Inches.						Weight of acid. Lbs.		Weight complete cell. Lbs.		Height over all including connections.
		Normal.	Normal.		Maximum.		Glass.			Rubber.							
		Amperes.	Rate.	Ampere hours.	Rate.	Ampere hours.	Length.	Width.	Height.	Length.	Width.	Height.	Glass.	Rubber.	Glass.	Rubber.	In.
16	15 L	15 to 30	30	300	40	250	$10\frac{3}{4}$	$12\frac{1}{4}$	$13\frac{3}{4}$	—	—	—	50	—	130	—	$15\frac{1}{2}$
17 & 18	23 M	10 to 25	25	150	30	110	$10\frac{1}{2}$	$8\frac{1}{2}$	$9\frac{7}{8}$	$6\frac{3}{4}$	$7\frac{1}{8}$	$9\frac{7}{16}$	16	11	50	40	$11\frac{1}{2}$
19	15 M	5 to 15	15	100	20	70	$7\frac{3}{4}$	$8\frac{1}{2}$	$9\frac{7}{8}$	$4\frac{5}{8}$	$7\frac{1}{8}$	$9\frac{7}{16}$	11	7	35	25	$11\frac{1}{2}$
20	7 M	2 to 6	6	40	8	25	$4\frac{3}{4}$	$8\frac{1}{2}$	$9\frac{7}{8}$	$2\frac{5}{8}$	$7\frac{1}{8}$	$9\frac{7}{16}$	6	$3\frac{1}{2}$	20	13	$11\frac{1}{2}$
21	5 S	1 to 2	2	8	3	6	—	—	—	$1\frac{7}{8}$	$3\frac{3}{8}$	$6\frac{9}{16}$	—	$\frac{1}{2}$	—	$2\frac{7}{8}$	7

Electromotive force of each cell is about 2 volts.

DISCUSSION.

Mr. Prescott :—(continuing orally). There is another method of automatically maintaining a constant potential at the lamp mains, which for special reasons, I had not intended to allude to. It is so interesting, however, that I will make a sketch of it on

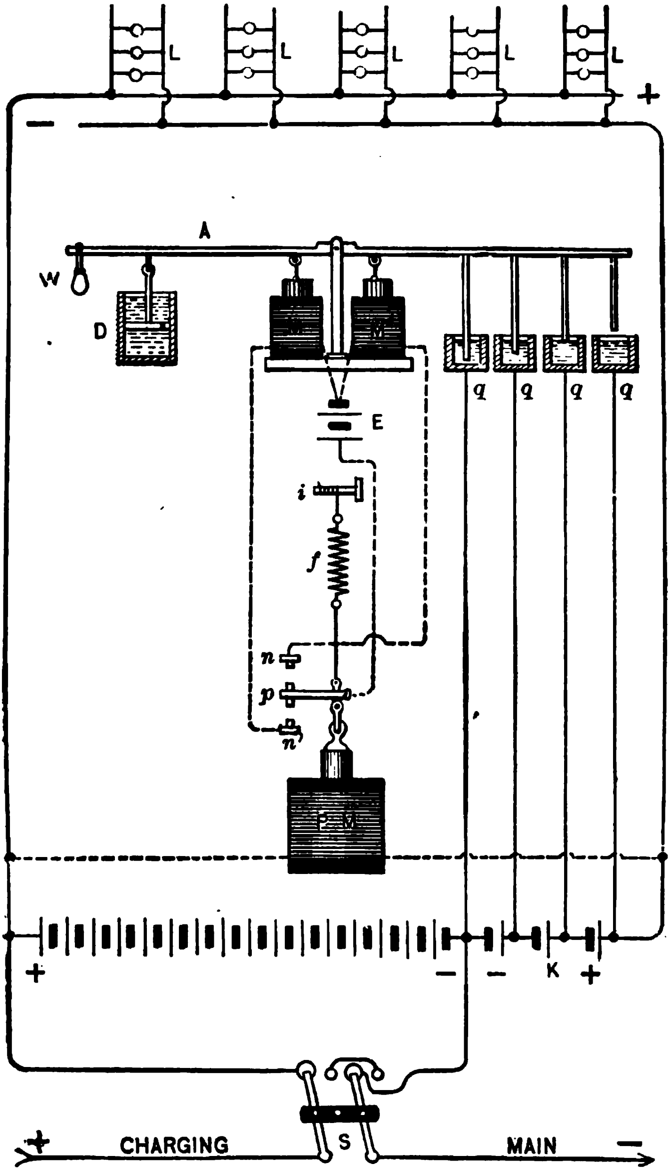


Fig. 22.

the blackboard. In this diagram (Fig. 22) the lamp circuits L, are shown at the top, leading from the mains (+) and (—). Connected across the lamp mains, preferably at a point of average potential, is the solenoid magnet P M, which is wound with fine wire. The position of the plunger or armature of this magnet is so adjusted by means of the retractile spring *f*, that when the pressure is normal, the contact lever *p*, carried by the plunger, is midway between the local contact points *n*, n^1. Probably most of my hearers are familiar with the Kohlrausch voltmeter, which is constructed on this principle of the solenoid magnet, and are aware that while it is not an entirely reliable instrument on account of variable friction and hysteresis, it still has a wide range and is very sensitive when the plunger occupies a given position. Now this magnet P M depends on this fact, and only needs to be accurate for one position of its core. If, for example, the magnet is wound for 100 volts, then when it is connected with two points in the lamp circuit, having a difference of potential of 100 volts, the core will occupy a certain position. If the pressure exceeds 100 volts the core will be drawn into the magnet. If the pressure falls below 100 volts the core will be withdrawn by the retractile spring. This core of the pressure magnet is suspended from the adjustable shaft *i* on the spring *f*, and carries the local contact arm *p*, on its upper end. When the core is drawn into the magnet this is brought into contact with the screw n^1, and when it is drawn out by the spring, contact is made with the screw *n*. So long as the pressure remains normal, the contact arm remains midway between the screws *n*, n^1, for which position the primary adjustment must be made. If now the pressure becomes either excessive or below normal the circuit of the local battery E will be closed respectively through one or the other of the local solenoid magnets M, M^1. The cores of these local magnets are attached to a balanced lever A at equal distances from and on opposite sides of its point of suspension, and the whole constitutes a simple modification of the regulator shown in Figs. 14 and 15 of the paper. The necessary weight of the lever on the side carrying the contact rods which dip into the mercury cups *q*. *q*., etc., is compensated for by extending the lever for an equal distance on the opposite side of its fulcrum, and by adding a suitable counter weight W.

The operation of this apparatus requires that the lever should remain quiescent in whatever position it may be drawn by the action of either magnet M or magnet M^1; and that, moreover any movement of the lever at all, tending to make or break contact with the mercury cups, should be very gradual. Both of these results are accomplished by introducing a slight friction at the point of suspension of the lever and by adding a carefully adjusted dash-pot D, in the manner shown in the sketch. Thus while the slight friction alluded to causes the lever to remain at whatever angle it may be placed, the dash-pot prevents the lever from moving except very slowly to whatever position the action of either magnet may tend to set it.

The function for which this regulator is employed is precisely the same as that performed by the somewhat similar regulator shown in Fig. 14 and 15, viz.: to insert in and withdraw from the main lamp circuit more or less counter-electromotive-force cells K, according as the pressure at the lamp terminals exceeds or falls short of the normal. The regulator acts in the following manner: Assuming the potential to be normal, the local contact lever *p* will stand midway between the contact points n, n^1, and the regulator lever A will be horizontal as shown in the diagram. Now if the potential at the lamps increases, the core of the pressure magnet P M will be drawn down until it closes the local circuit through n^1 and magnet M and this latter magnet will slowly tilt the lever *p* towards its side, and thus cause first one and then another of the counter-electromotive-force cells to be inserted in the lamp main, thereby reducing its potential. When a sufficient number of cells have been inserted in the lamp circuit in this manner, to reduce the pressure again to normal, the core of the pressure magnet again resumes its normal position, breaking the local contact at n^1, while the lever *p* continues to remain in its new position. If, from any cause, the pressure at the lamps now falls below normal, the attractive power of the solenoid P M will become correspondingly reduced, and the retractile spring *f*, will draw up the core until the contact lever *p* closes through the point n, the local battery circuit through the magnet M^1. This causes the lever to be gradually tilted in the opposite direction from before, and therefore to cut out of circuit one or more counter-electromotive-force cells until the normal pressure at the lamps is again restored.

It will be seen that this regulator thus automatically maintains a constant pressure at the lamp terminals, irrespective of the fact whether variations of this pressure are caused by the charging current, or by the resistance of the leads when the lamp load is altered.

Mr. Townsend Wolcott:—Mr. Prescott seems to place a great deal of reliance upon the voltage of the battery as an indication of the amount of charge left in it. If a battery is discharged in just a given manner, as far as my experience goes, the voltage will be determinate, but the higher the rate of discharge the sooner a low voltage is reached, and it is possible, just for experimental purposes, to discharge a battery at so low a rate as never to reach 1 8-10 volts, or even to go below say 1 9-10. In fact, I think by very slow discharge you could keep above 1 9-10 and still discharge so far as to spoil the battery. I suppose in practice the rate of discharge averages, in the class of work which is described, somewhere nearly constant, and the voltage may be a more valuable indication than it would be in traction work, or work of that character with which I am more familiar. I know in the Julien Company's work at Eighty-sixth street the voltage was no indication whatever of the charge left in the cells. There was a method which Mr. Julien introduced himself. The cells were divided into four groups, used sometimes in parallel and sometimes in series, and some of them would naturally be used more than others. So, in order to equalize them, the four groups were connected in parallel with each other, without any motor connection, the idea being that the cells at the higher potential would charge those at a lower potential; but as a matter of fact nothing of the kind occurs. After a few moments and a very slight discharge from the higher ones, they all come to the same potential, and where the cars were stuck on a grade, by waiting a few moments we would be able to go up the grade, or any tight place. This is another analogy to the animal that Mr. Prescott spoke of. By letting the battery rest, it gains sufficient energy to go up the grade.

Mr. Prescott:—The point Mr. Wolcott makes is perfectly correct, and unless the batteries are used under normal or somewhere near normal conditions, the potential alone is no indication of their charge. In the case where they are working at an unusually high rate, or discharging at a low rate, both the specific gravity and the potential would have to be taken into account. There the specific gravity would be the surest indication.

In regard to connecting several cells in parallel, I pointed out in the paper that where you are charging batteries in parallel and one set is more charged than another, the lower set does take more current for a while, but eventually its potential comes up to the others. Therefore we are obliged to have the regulating device I spoke of, otherwise the potential and electromotive force must both be taken into account to determine the condition of the batteries.

THE CHAIRMAN (Vice-President T. C. Martin):—In case there are gentlemen here not members of the Institute desirous of asking questions, we should be glad to have them do so. We have with us here one or two gentlemen who have been abroad recently, and have seen some of those storage battery stations in London working on analogous principles to those described here. I think it would be interesting if we could be furnished with information as to what has been practically achieved at those stations. Reference has also been made to storage batteries as replacements for some of the apparatus in use. We have with us gentlemen who are connected with stations using such systems. I think it would be useful to know their opinion.

MR. JOHN W. HOWELL:—The first thing I would like to say, is, to congratulate Mr. Prescott on the work he has done. I am sure what he has shown us here has been the one thing, the absence of which has stood in the way of storage battery success in the past. I have had something to do with storage battery plants installed by the Electrical Accumulator Company and I knew that their lack of success at that time was due to the absence of what Mr. Prescott has shown here. At a previous meeting of the Institute, in the discussion of a paper, I said a storage battery was capable of being regulated beautifully, but that it was not done at that time. I am glad to see that it is going to be regulated, because it is a most valuable adjunct of the systems now in use and properly regulated, its value cannot be overestimated.

MR. PRESCOTT:—I might say incidentally that the use of these counter-electromotive-force cells would prove by themselves a great convenience and would be very economical in all central stations in which equalizers are now employed. Of course, there is a little loss of energy in them, but the great advantage is that variations in the load would not require variations in the adjustment of a number of counter-electromotive-force cells in circuit. So this form of regulator is applicable to any lighting system whether storage battery or direct.

MR. HOWELL:—The only objection to the substitution of a counter-electromotive-force cell for an equalizer, would, I think, be the fact that it is wet.

THE SECRETARY:—I would like to inquire of Mr. Prescott if the automatic device shown in Fig. 15 is considered a perfectly practical arrangement, or whether the delicacy he spoke of would not tend to operate against its general introduction. That is the case very often in devices of that kind.

MR. PRESCOTT:—In regard to that I would say that while I think it could be made to operate practically it would be expensive and rather complicated and this device that I have shown in Fig. 22 accomplishes the same purpose in a simpler, and I think, more efficient manner. The other, as a laboratory apparatus, I have seen work, but as a commercial appliance I doubt whether it will work with complete success.

MR. JOSEPH WETZLER:—Some reference having been made to work going on in London in the distribution of current by storage batteries, I would like to state that there are two stations there now, which have been under way for several months; one in fact has been running for a year or two. This station, which has been running the longer time—the Knightsbridge station—is under the control of Mr. Crompton. They have been running very successfully, so much so that they are now enlarging their plant very considerably. Where they formerly used but one wire, they are now running a three-wire system underground in a shallow conduit with bare conductors. The appliances in this station are very simple indeed, and are all regulated by hand, both at the main station and the sub-stations. The station which seemed to be more modern in the respect of automatic regulation is at Chelsea, where Mr. Frank King, well-known in connection with storage batteries, is engineer in charge. In that station the whole operation of charging and discharging, both at the main and the sub-stations, is done automatically, and I must say that the equipment of the stations is very elaborate indeed in the matter of switches and connections. Indeed it would require an elaborate explanation to show the various functions of the apparatus. Among others, it embodied, as a method of regulation, the discharge and accumulation of gas evolved from a cell in a bell jar which gave indication of the time for the complete discharge of the batteries by raising the jar and cutting it out from the charging circuit. That was only an incident, however. The station has been in operation for several months, and I was shown some of the sub-stations also, which were in charge of boys, one of whom told me that he was getting a pound a week. He was the only attendant at the sub-station. His functions were merely nominal, as at the sub-station, also all the regulations were performed automatically. I must also add my quota to what Mr Howell has remarked regarding this paper, which I consider one of the best I have ever had the pleasure of listening to or reading.

THE CHAIRMAN:—My own opinion with regard to so valuable a paper is that it is somewhat of an injustice both to the paper and its author to discuss it off-hand. Such a paper as this could hardly be dealt with in a hurry, and therefore I think it would be well, if in view of the lateness of the hour, any gentleman who wishes further to discuss it, were to commit his views on the subject to paper, and we should be very glad to give him precedence and a hearing at the next meeting. Unless there is anything pressing on the mind of any gentleman at this moment of which he must deliver himself, I think that would be the better course.

Before a motion to adjourn is received I would like to state that at our next meeting, on November 19th, we shall have a paper from Mr. C. J. Reed, on a very interesting subject, "The Form and Efficiency of Incandescent Lamp Filaments." Mr. P. B. Delany will also bring before us some telegraphic improvements, including his own work in the devising of the Delany line-adjusting system, and one or two other matters. This paper of Mr. Delany's will be illustrated with practical demonstrations.

THE SECRETARY:—I have a matter that I think we ought to bring before the meeting before adjournment. You will all remember that the various engineering societies were represented in England during the past season and visited the Paris Exposition. The tour was begun too early to permit of the orginization of a party from the electrical engineers, the other engineering societies leaving about the first of June. But several of our members were in that body, and as the Institute had been officially invited, President Elihu Thomson of the Institute was there as our representative. At a banquet given by the Lord Mayor of London, at the Guild Hall, on the 13th of June, where the various engineering societies were represented, President Thomson responded to the toast of the American Electrical Engineers, and the Secretary of the Institution of Civil Engineers has been so kind as to forward a transcript of Professor Thomson's remarks on that occasion, and I would be glad to read them to you, for I am sure you will all be pleased to hear what he said:—

Professor Elihu Thomson:—My lords and gentlemen: It is my privilege, as the chief officer of a young, vigorous and growing organization, the American Institute of Electrical Engineers, to express its thanks for the opportunities and hospitalities which have been so freely and graciously extended to it by our English brethren. The electrical engineering profession is in fact a very recent extension, a division of engineering. I need not remind the eminent gentlemen here assembled, many of whom have been far better witnesses than myself of the fact, that scarcely more than ten years ago the electrical engineer had no part or place in the activities of engineering. It is true that the telegraphic expert was in a somewhat restricted sense an electrical engineer. It is more true that we have with us one whose scientific and mathematical genius made the difficult problem of ocean telegraphing an engineering success, and who, therefore I am quite sure you will agree may be called the father of electrical engineers. The name of Sir William Thomson, engineers in general will be prepared to honor as the great master who, in addition to his numerous labors, has given and continues to give the electrical engineer instruments which are as his rule and square and compasses. But in him also we find the type of the true scientist to whom all truth is of equal merit I can say for the electrical engineers of America, as representing their Institute, that the present opportunity of uniting with the civil, mechanical and mining engineering professions in celebrating the present memorable occasion, and particularly in extending thanks for the most hospitable and gracious privileges so freely accorded to them in England, will be long remembered and cherished. We are glad of the harmonious relations thus so auspiciously established with those on this side of the Atlantic who are carrying on work similar to our own, the growth and extension of which relations cannot but have the most beneficial effects on the work itself, but must even become a factor in uniting our national interests for the general advancement of the human race. The work of the electrical engineer is in a field of constant expansion; much work has been done, far more remains to be done. It is difficult for those unacquainted with the nature of the problems presented in a field so new to realize to the full, the hard-fought battle with difficulties and against obstacles resulting in the victories thus far achieved. It has been required of the electrical engineer that he unite the qualities of the mechanician, the chemist, the physicist and general technologist, with those of patience and tenacity of purpose. He has been, as it were, a pioneer in a new country, with rocks and fallen trees lying in his path. The materials of construction alone, with whose properties he must be acquainted, cover a vast range of available substances and manufactures.

For confirmation of this, all that is required is to inspect the stock rooms of our largest electrical manufactories. Many materials before unknown to the trade have been demanded in the work. The field of operation is wide. Electric metallurgy, telegraphy, telephony, arc lighting, incandescent lighting, motive power transmission and electric railways, electric welding, soldering and metal working, electric tanning, besides a host of other operations in which electricity plays a chief part, now afford scope for the activities of the electric engineer. Who is to say where the growth and development will stop? Will we not light our houses by electricity, giving light without heat? Shall not the time come when we may burn our coal, produce electricity in efficient amount therefrom, and thus supplant steam engines for motive power, and dynamos for electrical work? Shall not the great water powers be turned to account to propel our railway trains, to do our metal work, to light our streets and buildings, to run our factories, and to effect our chemical operations such as bleaching, tanning and others? Shall not we even store up power in substances possessing high chemical affinities, which power may be let out or recovered at any time hereafter for the uses to which electricity is now applied? Shall we even dare to hope that electrical communication on the Atlantic may be maintained with our friends and dear ones ashore? I believe that these things and more lie in the path which the electrical engineer of to-day has but begun to tread. As one of the oldest and greatest of philosophers has said of man, "For who shall bring him to see what shall be after him," we can certainly set no limit of achievement in this new field; we have at last laid hold of the properties of the universal ether to do our work, and its universal character will no doubt be exemplified in its applications to such work. As a delicate controller, as a transmitter of ideas written or spoken, as a giant mover of masses, as a lantern bearer, as a storer and carrier of power, as the most powerful and yet most delicate chemical reagent, as a heating agent under perfect control, and in many other ways, the ether has been called to do man's bidding, that wondrous agent which has place in nature, co-extensive with nature itself. In these allusions to electrical work I cannot forget, that to theoretical science in England, and to its great leaders, we owe much of the growth and development which have led to our beginning, feeble as it yet is, of an understanding of the nature of electric and magnetic phenomena. In conclusion, I join in the heartiest manner in the response to the toast in expressing for myself and the society which I represent a most thorough appreciation of the friendly spirit and kind wishes shown.

The Chairman:—I am sure it is gratifying to us to know that our society received this honor, as no greater honor can be bestowed in London, at least, than a banquet at the Guild Hall.

There is one other honor of the kind, and that is the freedom of the city, and it may be interesting to you to know that that honor would have been bestowed upon another of our members had he remained long enough in the country to receive it. I refer to Mr. Edison. I think as members of the Institute we have great reason for pride and pleasure in knowing that our President made so perfect a response in our behalf.

Adjourned.

A paper read before the 40th Meeting of the American Institute of Electrical Engineers, New York, November 19, 1889, and discussion thereon: Vice-President Martin in the chair.

FORM AND EFFICIENCY OF INCANDESCENT FILAMENTS.

BY CHARLES J. REED.

If we pass an electric current through any conductor, as a cylindrical wire, its temperature tends to increase by the transformation of electrical energy into heat. If no heat is allowed to escape, the temperature will increase indefinitely, or until the conductor is melted or otherwise destroyed. If the heat is allowed to escape by radiation alone from the surface of the conductor, its temperature will increase only until the rate of loss by radiation is exactly equal to the rate of transformation. By heat we here include all radiant energy, whether of high or low degree.

As the temperature of an incandescent body increases, not only does the actual quantity of radiant energy increase, but its wave lengths diminish. Hence, as experience has shown, after incandescence is reached, increasing the temperature in a given ratio, increases the light emitted in a much greater ratio.

The exact relation between temperature and luminosity is not known, and it is probably not very simple, if such a relation exists at all. The phenomenon of luminosity is really a physiological one and depends partly upon individual optical capacity; some persons being able to see above, and some below the visible spectrum of the average human eye. The radiant energy we call light is one thing. The sensation of luminosity by which we always estimate light is an entirely different thing. A constant source of light may vary greatly in luminosity, according to the condition of the receptive mechanism and its individuality.

But even ignoring the physiological aspect of the question, the nearest approach we have to formulæ for radiation are the approximate empirical formulæ of Dulong and Petit and of Stefan; which are for total radiations of a low temperature and limited range. They cannot apply to light alone, nor even to total radiation of high degree.

It is unfortunate that there exists no instrument more reliable than the retina of the living eye for measuring the intensity of radiant luminiferous energy, and no method of reading the measurements more accurately than individual guesses. We can measure radiant thermal energy of low degree with Langley's bolometer; we can measure radiant actinic energy by the chemical action it produces; and we can measure total radiant energy in a variety of ways; but how can we isolate or measure radiant luminiferous energy? If there were a high temperature thermometer or other instrument for accurately measuring high temperatures, we might attack this problem with some hope of results.

It is sufficient, however, for our present purpose to know what the eye is able to tell us, namely, that increasing the temperature increases the light in a greater ratio. From this it follows that the efficiency of an incandescent filament is some direct function of its temperature above that of the surrounding space.

Granting that the efficiency increases with increased temperature, we have now to determine whether the efficiency does or does not depend upon any other conditions. It has frequently been claimed that the efficiency depends upon the form of the filament, whether it be cylindrical, flat or square. At a meeting of this Institute of June 8, 1886, for instance, this was by general consent considered an established fact.[2] Others have asserted, but without giving any proof, that the efficiency depends upon the pressure; some claiming that low-tension series lamps and others that high-tension multiple lamps are the more efficient. Others, again, have claimed that at the same pressure and with the same shaped filament, lamps of greater candle-power have a different efficiency from lamps of smaller power.

The trouble seems to have been that we are not always careful to distinguish between efficiency and convenience or adaptability. Each individual finds that a certain form of filament or a certain

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[2] Trans. Am. Inst. E. E., vol. IV, p. 20–23.

method of distribution gives better satisfaction than another or is more suited to his purpose, and soon persuades himself that it is "more efficient" than any other.

In order to study the effects of these various conditions upon the efficiency of a filament, it will be convenient to eliminate the effects which we know will be produced by variations in temperature. We assume, therefore, in this discussion that all the filaments and all parts of the filaments under consideration are at the same temperature.

Let T represent this temperature and T', the temperature of the supposed vacuous space surrounding the filament. We must assume further that the material of which the filaments are constructed is perfectly homogeneous in itself and that it is uniform in all the various forms of filaments considered.

Let S denote the specific radiating power of the material at the temperature, T, and S', its specific resistance at the same temperature.

Any variation in S or S' might affect the efficiency, and for this very reason experimental proof is very difficult to obtain. Comparisons of filaments made by different processes are entirely worthless in determining the effect of form or length of a filament on its efficiency.

It is a difficult matter to produce by the same process two carbons of different sizes and shapes that shall have the same specific radiating power and specific resistance at the same temperature, and to produce them by different processes is entirely out of the question. Any comparison, therefore, of short series filaments with long, multiple filaments of a different manufacture is of no value whatever in settling this question.

Let C, represent the candle-power of any filament at temperature, T.
c, its current,
E, the potential difference at its terminals,
R, its resistance,
r, its radius (considered as a cylinder),
L, its length,
H, the energy received per unit of time,
H', the energy radiated per unit of time.

Since the energy developed in any portion of an electric circuit is proportional to the current and pressure, we have

$$H = K E c, \qquad \ldots\ldots(1)$$

in which K is a constant depending upon the units employed.

If we neglect the small quantity of heat lost by conduction from the ends of the filament to the conducting wires, and assume that the space surrounding the filament is a perfect vacuum, then the entire energy of the current will be expended in radiation, and we have

$$H = H'. \qquad \ldots\ldots(2)$$

Let K' represent a constant depending on $(T - T')$.

The rate of radiation of any surface depends only upon the elevation of its temperature above that of the surrounding space and the specific radiating power of the surface at that temperature. We have, therefore,

$$H = H' = K' S \times 2 \pi r L = K E c. \qquad \ldots\ldots(3)$$

The resistance of any conductor is proportional to its length and inversely proportional to its cross section, and we have

$$R = S' \frac{L}{\pi r^2} \qquad \ldots\ldots(4)$$

From Ohm's law and (4)

$$c = \frac{E}{R} = \frac{E \pi r^2}{S' L} \qquad \ldots(5)$$

By eliminating successively E, c and r from (3) and (5) we get

$$\frac{K' S \times 2 \pi r L}{K c} = \frac{c S' L}{\pi r^2} \text{ or, } \frac{c^2}{r^3} = \frac{2 \pi^2 K' S}{K S'} \ldots(6)$$

$$\frac{K' S \times 2 \pi r L}{K E} = \frac{E \pi r_2}{S' L}, \text{ or } E^2 r = \frac{2 K' S L^2 S'}{K} \ldots(7)$$

and

$$\frac{K E c}{K' S \times 2 \pi L} = \sqrt{\frac{c S' L}{\pi E}} \qquad \ldots(8)$$

From (6)

$$K' S \times 2 \pi^2 r^3 = K S' c^2. \text{ Hence,}$$

$$r = \sqrt[3]{\frac{K S' c^2}{2 K' S \pi^2}} \qquad \ldots(9)$$

and

$$c = \pi \sqrt{\frac{2 K' S r^3}{K S'}} \qquad \ldots(10)$$

showing that the square of the current is proportional to the cube of the radius or diameter of the filament for all lamps at constant temperature; and that this relation is independent of the pressure, of the length and resistance of the filament and independent of the candle-power;

From (7) we have

$$K' S \times 2 L^2 S^2 = K E^2 r. \quad \text{Hence,}$$

$$L = \sqrt{\frac{K E^2 r}{2 K' S S'}}, = E \sqrt{r} \sqrt{\frac{K}{2 K' S S'}} \qquad \ldots.(11)$$

and

$$r = \frac{L^2}{E^2} \times \frac{2 K' S S'}{K} \qquad \ldots(12)$$

showing that the ratio of the square of the length to the diameter depends only upon the pressure and is independent of candle-power and resistance;

From (8)

$$K^2 E^3 c = 4 \pi K'^2 S^2 S' L^3. \quad \text{Hence,}$$

$$E = \frac{L}{\sqrt[3]{c}} \sqrt[3]{\frac{4 \pi K'^2 S^2 S'}{K^2}} \qquad \ldots\ldots(13)$$

and

$$c = \left(\frac{L}{E}\right)^3 \times \frac{4 \pi K'^2 S^2 S'}{K^2} \qquad \ldots\ldots(14)$$

showing that the current is proportional to the cube of the ratio of the length to the pressure.

Since K', S and S' are constant, both the light and the heat radiated will be proportional to the surface and we have

$$C = K'' L \times 2 \pi r \qquad \ldots\ldots(15)$$

in which K'' is a constant depending upon the units employed and upon T.

Hence,

$$L = \frac{C}{2 \pi K'' r} \qquad \ldots\ldots(16)$$

and

$$r = \frac{C}{2 \pi K'' L} \qquad \ldots\ldots(17)$$

Combining (15) with (3)

$$\frac{K' S C}{K''} = K E c, \text{ or}$$

$$\frac{C}{K E c} = \frac{K''}{K' S} \qquad \ldots\ldots(18)$$

which shows that the candle-power is proportional to the energy, $K E c$; or that the efficiency, $\frac{C}{K E c}$, is constant and independent of the pressure, length, radius, resistance, current and candle-power of the filament. This means that the energy required to produce a given candle-power will be proportional to the candle-power and will be the same, whether it is expended in driving a large current through a short, thick filament, or a small current through a long, slender filament; provided the temperature or state of incandescence is the same and provided no heat is lost by conduction through the terminal wires.

Equation (18) shows also that the efficiency does depend upon K', K'' and S, that is, upon the temperature of the filament, the temperature of the surrounding space and the specific radiating power, and upon them only.

From (18)

$$C = E\,c \times \frac{K\,K''}{K'\,S} \qquad \ldots\ldots(19)$$

$$E = \frac{C}{c} \times \frac{K'\,S}{K\,K''} \qquad \ldots\ldots(20)$$

and

$$c = \frac{C}{E} \times \frac{K'\,S}{K\,K''} \qquad \ldots\ldots(21)$$

From (4)

$$r = \sqrt{\frac{S'\,L}{\pi\,R}} = \sqrt{\frac{S'}{\pi}} \times \sqrt{\frac{L}{R}} \qquad \ldots\ldots(22)$$

and

$$L = \frac{\pi\,r^2\,R}{S'} \qquad \ldots\ldots(23)$$

From (5)

$$E = R\,c \qquad \ldots\ldots(24)$$

From (21) and (24)

$$c = \frac{C}{c\,R} \times \frac{K'\,S}{K''\,K} = \sqrt{\frac{C}{R}} \times \sqrt{\frac{K'\,S}{K''\,K}} \qquad \ldots\ldots(25)$$

We have thus far confined ourselves to the consideration of cylindrical filaments varying in length and diameter. It remains now to show that the efficiency is independent of the form of cross section.

Let us suppose we have a number of lamps, all made of the same material, having the same specific resistance and radiating power, all burning at the same temperature and all giving the same amount of light. Let them be of any pressure and let the cross-section be of any form—circular, elliptical, square, triangular, etc. The filaments will all have equal radiating surfaces; since unequal surfaces of the same character at the same temperature could not radiate equal amounts of light. But equal surfaces of the same character at the same temperature must radiate equal amounts of heat of all wave lengths. Hence, the total amounts of radiant energy are equal. Since the energy received is equal to the energy radiated and the amounts of light are equal, we have

$$C = Q\,E\,c \qquad \ldots\ldots(26)$$

in which Q is a constant, and from (18) we find its value to be

$$Q = \frac{K\,K''}{K'\,S} \qquad \ldots\ldots(27)$$

This shows that the efficiency is independent of the form of cross-section of the filament and depends only upon its temperature, the specific radiating power of its surface, and the temperature of the surrounding space.

When we find, therefore, that one lamp is more efficient than another we must infer, not that it is on account of larger or smaller size, not because it is on a series or on a multiple circuit, not that it is because it has high or low resistance, or a certain form of cross-section, but that it is at a higher temperature, or is made of material having a different specific radiating power.

Of all possible forms of cross-section the circular has the largest area for the same radiating surface per unit of length, and consequently, has the advantage of great strength. The current density will also be less in this than any other form; and hence, any "disintegrating" or "electrolytic effect" of the current, if there is any such, will be least in the cylindrical filament. This will be made clear by supposing a number of lamps of equal candle-power to be burning at the same temperature and pressure, but having various forms of filament, cylindrical, flat, square, etc. We have already seen that these lamps all consume equal amounts of energy, and since they have the same pressure, they must take the same current. Therefore, the filament which has the greatest area of cross section will have the least current density. The fact that the cylindrical filament has the greatest cross-section does not signify that it will require a greater current to keep it at the same temperature. Its form is better adapted for retaining heat than any other. Again, the several lamps will have equal resistances; otherwise they could not take equal currents with the same pressure. From this it follows that the cylindrical filament will be longer and will have less surface and greater mass per unit of length than any other.

A tubular filament would have a greater external diameter, but smaller cross-section of material, and would be shorter than the cylindrical.

The advantages and disadvantages of the various forms may be summed up as follows:—

The cylindrical has the advantage of greater strength and less current density on account of greater cross-section. It has the disadvantage of greater length and fragility.

The tubular filament is strongest in form, being shorter and of greater external diameter than the cylindrical. It has the disadvantage of greater current density than the cylindrical. Both the tubular and the cylindrical have the advantage of uniform illumination in all directions except in and near the plane of the filament.

Flat, or angular filaments have the disadvantage of great current density, and of being unequally heated, since the edges or projecting corners will always be a little cooler[3] than the other parts.

A short, thick filament has the advantage of being stronger and more durable than a long, slender one; but the disadvantage of wasting a greater percentage of heat by conduction through the terminal wires. In this latter respect, it is true, that the long filament is slightly more efficient than the short. A 10-ampere filament, a foot long, would waste no more energy by conduction than a 10-ampere filament an inch long.

Aside from the difficulty of obtaining lamps of different types from different makers which have the same specific resistance and radiating power, there is the difficulty, or rather impossibility, of getting them all to burn at the same temperature, and the difficulty of knowing, even approximately, what that temperature is, and whether it is the same in two lamps or not. For these reasons no experimental comparisons yet published are of any value, either in corroborating or refuting these conclusions. There is one method, however, of testing the formulæ. This method the writer has tried upon some twenty-five different

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[3] It was asserted before this Institute, June 8, '86, that the edges or corners of a square filament are hotter than the remainder of the surface; but no reason was given. Such a statement scarcely needs refutation, being contrary, not only to established laws, but to the most commonplace and every-day experience. We need only observe the cooling of a square bar of red-hot iron to convince ourselves that the sharp corners are the coolest parts And they would remain the coolest parts even if heat were continually supplied by an electric current to the interior of the bar.

types with the most satisfactory results. The method is as follows:—

A lamp is constructed with a filament of known dimensions, treated to the required process and exhausted to a certain pressure, as determined by a McLeod vacuum gauge. It is then placed on a circuit of a certain pressure, which gives it a certain desired temperature or efficiency, and the candle-power and current are measured. From the data thus obtained, we calculate by the above equations all the data for lamps of other types. The lamps when made according to this data from the same material, by the same process, and exhausted to the same pressure, should give the calculated candle-power at the calculated pressure.

It must be remembered that the formulæ take no account of the small amount of heat that escapes by conduction through the wires. With lamps of high resistance this may be safely ignored in practice, but in changing from a long to a very short and thick filament it must be taken into account.

The following examples may serve to better illustrate the method—

Let us suppose that a lamp has been constructed to give a certain candle-power on a circuit of given pressure, and it is desired to construct other lamps having the same efficiency (temperature) and life, but of different candle-power, for the same or for different pressure. We will call this lamp for convenience the zero lamp, and denote its particular values of the variables by the subscript, *o*.

Let $C = C_o =$ 16 candles.
$c = c_o =$ .6 ampere.
$E = E_o =$ 100 volts.
$R = R_o =$ 167 ohms.
$L = L_o =$ 170 millimeters.
$r = r_o =$ .07 millimeter.

The constants K, K', K'', S and S' will have the same value for all values of C, c, E, R, L and r, and hence, will be found by substituting for these quantities the simultaneous values, C_o, c_o, E_o, R_o, L_o and r_o, respectively.

If H is measured in horse-power, we have from (1) $K = \frac{1}{746}$. From (4)

$$R_o = S' \frac{L_o}{\pi r_o^2} \text{ or}$$

$$S' = \frac{\pi R_0 r_0^2}{L_0} = \frac{\pi \times 167 \times (.07)^2}{170} = .015122.$$

from (3)

$$K' S = \frac{K E_0 c_0}{2 \pi r_0 L_0} = \frac{100 \times .6}{746 \times 2 \pi (.07) \times 170} = .0010757,$$

from (15)

$$K'' = \frac{C_0}{2 \pi r_0 L_0} = \frac{16}{2 \pi (.07) \times 170} = .21399.$$

Substituting these values of K, K'', S' and $K' S$ in equations (4) to (25), we are enabled to solve any problem involving R, E, C, r, c, and L, when any two of the quantities except c and r are given. The equations apply only to lamps of the same material and at the same temperature as the zero lamp. By starting with a lamp of different material, or by burning the same lamp at a different temperature, we obtain a different set of constants, which substituted in equations (4) to (25) adapt them to the changed conditions.

Suppose we wish to construct a lamp of 20 candle-power for a 90-volt circuit:—

$$C = 20.$$
$$E = 90.$$

From (21)

$$c = 3.75 \frac{20}{90} = .8333 \text{ ampere}$$

from (9)

$$r = .098466 \sqrt[3]{(.8333)^2} = .087196 \text{ millimeters}$$

from (11)

$$L = 6.4190 \times 90 \sqrt{.087196} = 170.59 \text{ millimeters}$$

and from (4)

$$R = .0048134 \frac{170.59}{(.087196)^2} = 108.00 \text{ ohms.}$$

The calculations may be verified by substitution in some of the other formulæ.

In constructing the lamp we take a filament 170.59 millimeters long, and of such a radius that, when treated to the given process until at the proper exhaustion it takes .833 ampere with 90 volts, it will have a radius of .0872 mm. If the treatment of the carbon does not increase its diameter, the initial and final radius will be the same; but the radius of the *finished carbon* is to be .0872 mm. When these conditions are fulfilled, we know with certainty that the lamp has the proper temperature if it gives 20 candles. If any single condition is not fulfilled we know with the same certainty that the lamp has not the proper temperature. If two or more of the required conditions are simultaneously not fulfilled, then we know nothing about the resulting temperature.

We are thus enabled to construct lamps of uniform life for varying conditions of current and candle-power; and also to regulate the length of life and the efficiency, increasing or diminishing either the life or the efficiency at pleasure to suit the requirements of particular conditions, remembering always that the life and efficiency are inverse functions of each other.

The fact that the life of most commercial lamps is very irregular, shows that either the material or the temperature is not uniform, and hence, that there is lack of uniformity either in the process or in the dimensions. The writer has examined a number of different makes of lamps, and found that they generally vary in length and cross-section enough to produce serious differences in temperature.

Suppose, again, we wish to construct a 50-candle lamp for a series circuit of 5 amperes:

$$C = 50.$$
$$c = 5.$$

From (20)

$$E = 3.75 \times \frac{50}{5} = 37.5 \text{ volts.}$$

from (9)

$$r = .098466 \sqrt[3]{(5)^2} = .28792 \text{ millimeter}$$

from (11)

$$L = 6.419 \times 37.5 \sqrt{.28792} = 129.16 \text{ millimeters}$$

from (4)

$$R = .0048144 \frac{129.16}{(.28792)^2} = 7.5 \text{ ohms.}$$

In this case the loss of heat by conduction through the connecting wires is considerable, and must be taken into account.

The above equations are not limited to incandescent filaments, but apply equally to any conductor which may be kept at a cer tain fixed temperature by an electric current. By using the proper constants, useful formulæ may be derived for calculating conductors, so that under given conditions they will be heated to a required temperature.

DISCUSSION.

PROF. EDWARD L. NICHOLS:—Every one, I think, who is interested in the study of incandescent lamps will be very glad to hear reiterated the fact, which I think none of us will desire to question, that efficiency is a function of the temperature and not a function of the shape of the carbon and of the various other things to which it has often been ascribed. I think there can be no question on this point. It seems to me the clearest point that we have in reference to radiant energy—that the thing which we must get in order to get increased efficiency is higher temperature, and that any method that will enable us to do this will enable us to increase the efficiency. That the temperature of the lamp cannot be measured to-day is probably true. I have seen, however, within a few weeks, a paper by H. F. Weber, of Zurich, which leads me to believe that the day is much nearer than we thought when we shall be able to express the efficiency of any incandescent body in terms of its temperature directly. Mr. Weber has worked out a formula which he has applied to a great variety of examples where the temperature could be measured; that is, he has gone over the literature of incandescence and has selected all those experimental investigations which can be said to be quantitative in any fair sense of the word. Almost all the experiments deal with platinum, because platinum is a substance of which we know quite accurately the law of expansion under change of temperature, and also the law of resistance with change of temperature. These various methods, however, have not been regarded as directly comparable; but Mr. Weber seems to have been able to get out a formula which can be applied to each of them separately. They are found to fall into line beautifully, so that he has put to a very severe test the formula which he proposes; and in the article to which I referred which appeared in the *Repertorium der Physik*, and which I believe has not been translated into English, he claims that he has applied this formula to a variety of incandescent lamps, with great success. I hope this may be true. I am sorry to say I cannot give the formula itself from memory. It contains two or three constants which are to be determined, but comparatively speaking it is a simple formula. I am in hopes, therefore, that we shall before very long be in a position to modify Mr. Reed's statement, which at the present time is undoubtedly justified—that we do not have means of measuring the temperature of incandescent lamps. The thing, of course, which has led to much confusion in this matter, is the fact that lamps behave very differently towards different observers and at different times, and at different times towards the same observer. I think the point there is largely a matter of vacuum. The formula which we have seen developed this evening assumes either a perfect vacuum or a vacuum which is equally good in all cases, and it is a matter of fact, well known, of course, to all those who work with incandescent lamps, that an apparently slight difference in vacuum—really a very large difference—is the thing which changes altogether the incandescence of the filament. In other words, while conduction is an almost negligible factor in this discussion, convection, even where the amount of air remaining in the bulb is small, is by no means a negligible factor, and the failure to recognize more promptly on all sides this simple relationship is due to the fact that we have to deal with lamps which may be identical in every other respect, and yet which vary quite widely in the matter of exhaustion. I must confess that it is one of the chief difficulties which lie in the way of any one, who will experiment with lamps of this kind. It can be overcome by the use of a pressure gauge measuring the vacuum in each case, but ordinarily as we know, this is not done. I should like to call attention to a very well known method by means of which any one can detect slight differences in temperature in two incandescent lamps, even though he may not be able to express either one of those in degrees centigrade, and that is by means of a very old form of photometer known as the Rumford or shadow photometer. Let any one take two lamps made with the very greatest care by our methods of to-day, and of the same type and marked to give 16 or 20 or whatever candle power it may be, at the same voltage. Set those up so that the light from them will shine upon a sheet of white paper, and interpose a block so as to get partial shadow, there being one portion in complete shadow and two portions in partial shadow. An inspection will show that one of those shadows is always a shade bluer than the other.

Now, this method, I presume, many of you have used. I have used it with very much satisfaction in attempting oftentimes to get two lamps as nearly alike as possible. This method is one which gives a very delicate means of getting relative differences of temperature or at least of determining when two lamps are of precisely the same temperature; so that while we are not in position to express that temperature, we are in position to determine equality of temperature with a considerable degree of accuracy. It seems to me that a formula of the kind presented in this paper, which enables one to calculate, as it does evidently with great readiness, just what the dimensions of a lamp should be, will be of great value.

MR. GEO. B. PRESCOTT, JR.—Among the points that I understood Mr. Reed to make, and I am rather afraid I may be in error about it, was this—that one of the advantages of a cylindrical filament or one of circular cross-section over any other form was that the current density would be less, and to illustrate this I understood him to select a number of carbons of all sorts of cross-section, all sorts of shapes of cross-section at the same temperature or rather of the same difference of potential and the same current flowing, showing that the resistance was the same. Now, if they were of the same resistance and the same length, the cross-section must have been alike and I should suppose that the current density would be the same in all events.

MR. REED:—They are not the same length. I have stated that the E. M. F. and resistance were the same—the same current at the same potential. Now, they have different areas of cross-section and different lengths, which you will find to be true by inspecting this formula. Of course, if the lengths were the same the formula could not be true. The length bears a certain relation to the other constants.

THE CHAIRMAN (Vice-President T. C. Martin):—As you will have noted, Mr. Reed in his paper mentioned the fact that the paper itself was called out by a statement made before the Institute in 1886. When Mr. Reed first brought this to my attention it appeared to me that we were not at all wishful to make the publications of the Institute a vehicle for any kind of errors, or heresies, or heterodoxies, and it therefore seemed to me well that he should bring his paper before us on that subject, so that it might be cleared up, no matter how well there might be an understanding on the point in the better informed circles and where the investigation had been pursued to any length, and I think Mr. Reed has certainly rendered us a service in giving us so admirable a paper in so brief and succinct a form as he has given it to us this evening.

There being no other discussion, we will proceed to the next paper upon the programme—that by another member who is well known to you, Mr. Delany. I have much pleasure in asking Mr. Delany to give us his paper on his New Line Adjusting System.

A paper read before the American Institute of Electrical Engineers, New York, November 19, 1889, and discussion thereon.

TELEGRAPH LINE ADJUSTMENT.

BY P. B. DELANY.

I take pleasure in bringing before you this evening an improvement in telegraphy, which, although of recent discovery, has already been proven to be practical and beneficial in its operation. I have called it "The Line Adjusting System." This title is not strictly correct; but it is the most comprehensive one I could think of. The system keeps relays at intermediate stations in a telegraph circuit in adjustment in bad weather.

In this country we call a partial loss of current to the earth "escape" to distinguish that condition from a "ground," which means total loss. The terms "light escape," "heavy escape" and "ground" express the different degrees of depletion of a telegraph wire of its current. Demoralization in the operation of telegraphs is proportioned relatively to each degree. No one who is not, or has not been, a telegrapher can appreciate the trouble and turmoil which a rainy day brings to a telegraph wire, especially to a way wire, such, for instance, as the dispatching wire of a railway on which there are usually a large number of intermediate offices. Light escape means trouble, but heavy escape means chaos. Improvement in insulation has done much toward remedying the difficulty, but perfect insulation cannot be obtained, nor would it be altogether desirable, for then the wire could not in all cases be worked at as high a speed as with the present faulty insulation. What is needed is an insulation constant in its imperfection. So long as bare wires are strung on poles, and rain and sunshine succeed each other, there will be a fluctuating condition of insulation, and consequently an ever-changing current on the line, and, as a matter of course, corresponding changes in the attraction of the relay magnet for its armature. These changes in the magnetic pull necessitate readjustments of the retractile spring, the tension of which must always be regulated so as to allow the armature to move quickly between its limits, in response to the make-and-break of the circuit at the operator's key at any station on the line.

It does not follow, however, that a loss of current through bad insulation means a weakening of the electro-magnetism in all the relays. This is generally the effect near the middle of the line, provided the escape is about equal on either side, but at stations near the ends of the line, where the main batteries are located, a heavy escape means a reduction of resistance of the circuit by the partial ground established beyond them, so that they get the force of the main battery from the terminal near them over a comparatively short circuit. Under such conditions the stations near the ends of the line can work with the near terminal, and stations between, with a very low tension on the retractile spring, while it would be impossible for them to receive signals from the distant terminal, or offices beyond the middle of the line, on the same adjustment. A much higher tension must be put on the spring. On a line having an evenly distributed escape throughout, offices near the middle should not have as much difficulty in adjusting as offices near the ends, provided, of course, that the escape is not so heavy as to leave them insufficient current to work with. It frequently happens, however, that a storm extends only over half the line. Under this condition all the offices on the clear end of the line work with each other on a low adjustment, but require a high adjustment to hear the offices on the storm-bound end of the wire. Operators naturally prefer a low adjustment to a high one. When they are sending, their relay armatures and sounder levers respond promptly to the manipulation of the keys, making the work much easier. With a high adjustment the drag of the sounder click behind the movement of the key renders the manipulation laborious. Hence it is, that an operator who has at one minute been receiving from a distant station, requiring high adjustment, will, when he comes to send a return message, turn down his adjustment, so the sounder will follow his key smoothly. Then if the distant station finds it necessary to stop him, he does not feel the break, but goes right on sending.

I have noticed that nearly all operators, good and bad, are vain of their abilities to send rapidly, and nearly all are ambitious to send faster than the operator at the receiving station can write it down, or, in other words, to "rush" him. This is especially true of young operators; but taking them collectively, I do not think there is a body of craftsmen in existence who work so willingly as telegraphers. Each seems individually impelled to "salt" the man at the other end of the line, if possible, and when he succeeds in making him "break," he mentally records a victory and goes at it again with renewed vigor. To outsiders this self-imposed rapid pace may seem foolish, but to the knight of the key there is great glory in it. The great artist never lived, actor, orator or musician, whose soul was more thrilled at the plaudits of thousands, than is the soul of the expert telegrapher when with faultless and rapid transmission he humbles a great receiver by compelling him to beg for quarter. There is music in it, too, and the pride is pardonable, as every one who has been chained to a mad stream of dots and dashes by the hour in the stilly night, bending all his energies to keep the thread and write it down legibly, will admit. Therefore, it is not to be wondered at that on way lines where ambition and ability are greatly out of proportion, there is always trouble in bad weather. The operators are, in the main, young and inexperienced. The lines, on account of the large number of instruments in the circuits, work hard. There is "breaking in," delay and bad temper. This is especially the case with railway telegraphs, where perfection is most needed. I have never been able to understand the niggardy policy pursued by many great railway corporations toward their telegraph department. Poor pay, overwork, miserable accommodations have been the rule in the past. While money has been lavishly spent on sleepers, buffets and parlor cars, and all kinds of luxurious upholstery and conveniences, to attract the traveller by rail, the rail itself, the safety of the car, train and passengers have been made to depend on the ability of some overworked boy at some far-off way station to resist sleep, at a time when he ought to be snugly tucked away in bed. If the travelling public could look more discriminatingly at the electrical equipment of railroads and take less account of tapestry and tinsel, they would in many thousands of cases, I am sure, change their line. It is an encouraging sign that at last the keystone and foundation of railroading, the telegraph service, is beginning to receive better recognition. Instead of allowing the telegraph department to flounder along under the direction of the master of transportation, or division superintendent of the road, with no electrical knowledge whatever, progressive roads now have superintendents of telegraph, and division operators, who, by their practical training and experience, are making great strides in the improvement of the service. When this plan is carried out generally, and when purchasing agents, who may know all about the various other supplies necessary for running railroads, are relieved from the selection of electrical apparatus, a great improvement will be effected. It may appear all right from the commercial standpoint of the purchasing agent to look upon a relay as a relay, a battery as a battery, and buy those which cost the least, but ten cents in the price of an instrument upon the effectiveness of which lives and property depend from one year's end to another, could not influence a practical telegrapher. Instead of forcing bad iron, bad wire, bad insulation, and clumsy, cheap workmanship into telegraph apparatus, the quality and efficiency of instruments will some day be the prime stipulation. I have seen many telegraph relays rendered almost useless for want of a proper retractile spring. This may seem a very small affair, but it is really very important. It affects the efficiency of the entire outfit and the quality of the service. Upon the quality of a relay spring depends the working of the instrument quite as much as upon the quality of the current, and yet it is only necessary to glance at the apparatus in any telegraph office to almost invariably discover that the relay spring is nearer a straight piece of wire than a spiral, and that a half turn either way will throw the relay out of operation. At the risk of having these few criticisms and suggestions ascribed to interested motives, I earnestly commend them as worthy of consideration.

I will now proceed to explain how a great improvement may be effected in the operation of telegraph lines having any considerable number of way stations. I claim that this improvement is almost as necessary to the dispatching wire as the air brake is to the train. It gives the dispatcher control of all his operators, prevents interruptions and delays, and, if the line can be worked between the terminal stations, all the intermediate offices will be in perfect working order. Their instruments will work as well in bad weather as in good. Operators cannot evade responsibility for neglect of duty or seek refuge behind the excuse that their instruments were out of adjustment on account of the bad condition of the line. Their instruments will always be in adjustment, so long as the terminal offices can communicate. This I will, in a few minutes, demonstrate in a practical way over this miniature line, after a brief reference to the diagram of the circuit and connections which you have.

Fig. 1. shows a telegraph line extending from New York to Philadelphia, with New Brunswick and Trenton as way stations. There might be forty such stations, but these two will suffice for our illustration. The figure shows the ordinary outfit for closed circuit, single current working; the system used almost exclusively in this country, and comprising a relay, sounder, and key, the only adjustment being the *adjuster* A. This instrument consists of a magnet, armature lever, *l*, pawl, *p*, and stops *a* and *b*, ratchet wheel, *r*, on the shaft which carries the trailing finger, *t*, and the circle of segments. Alternating segments are joined to a common plate. Set S form part of the circuit, while set S′, are not connected. but simply serve to make a smooth track for the trailing finger. Beginning at the Philadelphia end, the circuit may be easily traced from the main battery, M B, and key to segment plate, S, upon which the trailer, *t*, rests. From the trailing finger to relay, R, through relays at Trenton and New Brunswick to New York, where it goes from relay, R′, to trailer, *t′*, segment plate, S, to key, main battery, M B, and to earth. The adjuster magnets are in the local circuit with the sounders, SR. Fig. 2 is a diagrammatic view of the switch connections of the adjuster for throwing it in or out of use, as the state of the weather or line may require; MM are the main line wires, LL the locals. The switch is now in neutral position. Of course it is never allowed to remain so in practice, as both the main and local circuits would be broken. Fig. 3 is a perspective view of the instrument, inclosed and ready for use.

Let us assume that the weather is fine, and the line clear. The switch will be thrown to the left. The adjuster will be out of use, the line being connected outside of the trailer and segments, while a small resistance coil, shown on the switch, will be substituted for the adjuster magnet in the local circuit, the battery of which, on account of the adjuster is increased from two gravity cells to five. While the switch is in this position we have the present ordinary organization for working. Now, if a rain storm comes on, an intermediate earth route for the main batteries at New York and Philadelphia is established, and trouble with the

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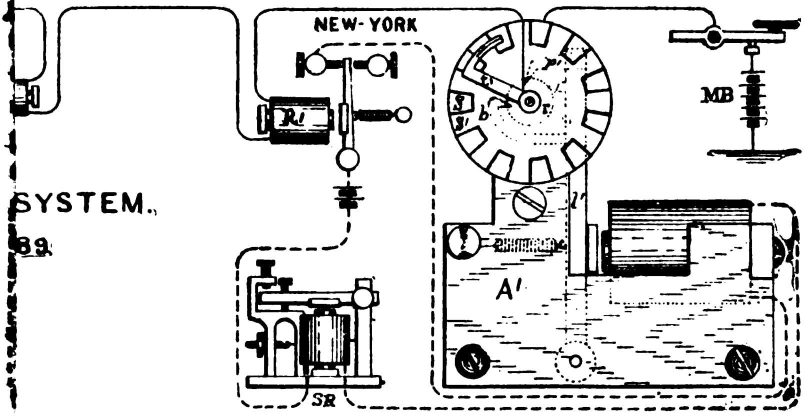

Fig. 3.

TEM.

line, the New York relay, R′, will be affected, the armature pulled back by its spring, will leave its front stop and break the local circuit, in which are included the sounder and the adjuster magnets, the pawl on the adjuster lever engaging a tooth of the ratchet wheel, *r*, on the shaft carrying the trailer, *t′*, will push the trailer across the blank or dead segment of the plate s′, thereby disconnecting the main battery at New York *for an instant*, during which time there is no battery on the line at either end, and consequently no current to keep the armatures of intermediate relays attracted to their magnets. Therefore, so long as the terminal offices can communicate with each other, all the instruments between will be in adjustment and intercommunication rendered almost as easy as during good weather. The operators at these offices are not only kept aware that the line is in use, but their relays work on a much lower adjustment, notwithstanding that the opening at one end is but for an instant. The armatures of the relays having been released by this short break will not be attracted again readily by the re-establishment of the partial ground, or not until the battery is connected again at both ends, by closing of the key. When an intermediate office is sending, the line is opened at three points, the key, as long as it is held open, and at both ends for an instant.

Now, if you will continue your kind indulgence for a few moments, I will endeavor to give you a practical demonstration of what I have been trying to explain.

The adjusters are now switched out. We start out with a clear day. All the relays on the line have a normal adjustment and respond to the manipulation of any key Philadelphia is sending. The instruments at Trenton, New Brunswick and New York all receive the signals. By placing a lead from the line and ground wires in this jar of water we have in effect a rain storm, with its centre between Trenton and New Brunswick, making quite a heavy escape. Now, when Philadelphia operates his key, the Trenton instrument speaks up as before, but New Brunswick and New York are silent. New York being a head office, however, has experienced and careful operators who take the precaution to try the relays on a high adjustment to see if anything is going on. Finding the line in use, they adapt the adjustment to the changed condition. Now the adjuster comes into action and wakes up our friend at New Brunswick, who would otherwise be deaf to calls from Trenton or Philadelphia, and would at the same time be a ruthless trespasser if he had a message to send. It will therefore be plain that the instruments at all intermediate offices must work whenever it is possible to work the line from one terminal to another under the most delicate adjustment.

THE CHAIRMAN:—Mr. Delany said "Let us assume that the weather is fine." The operators between New York and Philadelphia this evening would, no doubt, be glad to assume those conditions, and in default of those conditions I have not the least doubt that not only they, but the operators on the long railway lines would be very glad indeed to have placed at their command so valuable an assistance as this which Mr. Delany has brought before us this evening. It is fifty odd years since the telegraph was first introduced and a great many of us are apt to assume that it is perfect. There is probably hardly any electrical invention to-day which is so imperfect as the telegraph, and we have reason to be thankful to such men as Mr Delany for devoting their time and attention and talent to perfecting what has been one of the great inventions of our time. It is important, in connection with such a body as the Electrical Engineers that we should impress upon our own constituency as well as the public how large and varied is our field, and I think it could hardly have been done in a more emphatic manner than we have accomplished it this evening, with Mr. Reed at one end of the line discussing incandescent lamps and Mr. Delany at the other showing how important it is that the travelling public should have placed at their service a complete telegraph system. When Mr. Delany promised to give us this paper, it occurred to me that it would be well if he would also favor us with a few remarks on another subject, namely, the gravity cell, to which he has been devoting some attention. One merit of Mr Delany's work in connection with primary batteries is, that he does not claim to light the universe with it. What its demerits are I will now leave him to tell you.

NOTE ON A NEW GRAVITY CELL.

BY P. B. DELANY.

I have taken too much of your time already, and I will make but the briefest reference to this modification of the standard gravity battery. In fact it requires but little explanation. The sulphate of copper is inclosed in the straw board box. The zinc in a paper envelope, and the rim of the jar has attached to it on the inside by a sticky substance, a band of rubber cloth. The advantages claimed for the cell are as follows: When the battery is first set up the dust of sulphate of copper is not instantly dissolved and diffused throughout the liquid, coating the zinc with copper, as is the case with the ordinary cell. Several minutes elapse before discoloration of the fluid begins, and then only at the bottom of the cell, from whence it rises very gradually, never reaching the zinc. If the box be filled, the charge of copper is always uniform. Deposited or spongy metallic copper cannot fall upon the crystals from the zinc, and caking or massing in the bottom of the jar is thus prevented. The copper electrode is held firmly in position, always the same distance from the zinc.

In a battery of any considerable number of ordinary cells it would be difficult to find two alike in this respect. There are little or no stalactic formations from the zinc, and consequently no local action, rendering the battery very useful for open circuit work. When water is poured in to make up for evaporation, the equilibrium of the fluids is not disturbed. The deposit on the zinc thus protected is easily removable, requiring no hacking or scraping. One zinc will endure two charges of sulphate of copper.

The band around the rim is one of the most important features of the cell, as it prevents zinc sulphate from creeping over. It offers simply a mechanical obstruction. It works perfectly in practice. Of course these strips or bands may be applied to any battery requiring them. They serve equally well for Leclanche battery, and to attach them to cells of any kind already up, it is only necessary to see that the rims of the jars are clean and dry. The sticky side of the strip should be heated slightly and pressed on firmly all around.

DISCUSSION.

MR. C. L. HEALY:—I would like to ask Mr. Delany in regard to the resistance of the cell.

MR. DELANY:—I have not had accurate measurements of it, but from the measurements that I have had—they were communicated to me by people who had tried the battery—I find that the resistance is about the same, and the current about the same as in the ordinary gravity cell. You would suppose that the resistance would be increased slightly on account of the paper box and the paper envelope, but you will observe in this battery that the copper electrode is considerably nearer the zinc, and probably that compensates for the intervention of the envelope and the box.

MR. HEALY:—Does the introduction of the adjusting apparatus make any difference with the working of the instrument?

MR. DELANY:—None at all. There is no perceptible lagging. It has been in operation for some years on prominent railways and they make no complaint in that regard.

MR. R. W. POPE:—I would like to assure our members that Mr. Delany's remarks in regard to the condition of the way lines in this country are no fancy sketch at all. A great many people have an idea that there has been progress in telegraphy as well as other branches, but so far as the way lines are concerned I can speak from actual experience during the past summer, that they are in many cases just as bad or worse than they were twenty years ago. I have learned from some inquiries on the subject that the same care is not taken in keeping up the lines as was formerly the case. This matter of adjustment at way stations comes home to every man, for the reason that all may have occasion some time during our lives to send telegrams to small stations, and it is a fact that very often there are annoying delays that are accounted for by companies on the plea of bad weather. That is taken as something that cannot be prevented; it is an act of God. But here we see a device for preventing a large proportion of this trouble, which, to my own knowledge, has been in practical operation for some time. I have recommended it to some of the railway telegraph superintendents, who have since tried it, and have talked with them about it and found that it is giving perfect satisfaction. In fact, at the meeting of Railway Telegraph Superintendents in Washington one of the gentlemen from Boston said that a storm came up that week and his dispatcher told him that they could not have used the wire at all if it had not been equipped with this device. The difficulty arises from the fact that at the smaller offices the operators do not give constant attention to their instruments. They have multifarious duties in railroad stations—selling tickets, checking baggage and things of that kind, and the instrument is left to take care of itself. They may be away for from half an hour to an hour. In fact I knew of a case this summer where a freight train was detained for an hour, simply because the office where it lay could not get instructions for the train to proceed. It was a bad night, and it is fair to assume that in this case if a system of this kind had been in use they would have had no difficulty. That is, providing the dispatcher's office was manned with sufficient force to take care of all wires. That I am free to confess is not always the case. A great many times an operator is supposed to take care of three or four wires because they do not require constant attention, but as a matter of fact he may be wanted on three or four wires at the same time. The fact that railroad companies do not pay sufficient attention to their telegraph equipment, I think is due, to a certain extent, to the fact that there is little or no revenue from the telegraph service. It is an expense to the road, and being an expense and not a source of revenue, they do not give it that attention that they would if it were an actual revenue-earning part of the system. Of course, it saves them a great deal of expense and a great deal of annoyance, and yet at the same time its importance is not recognized as it would be if it were a profit earning part of the system. That may not be a good reason, but I think it is the reason.

There was one interesting fact came up in connection with this experiment here. It was interesting to me, knowing the circumstances, although it has no bearing on this particular subject. It was necessary to arrange an equipment here of twenty-five cells of carbon battery, and I assured Mr. Delany he would have no trouble in getting them. I went to the Gold and Stock Telegraph Company where, four or five years ago, they had 10,000 cells of carbon battery, and it was with some difficulty they scraped up the number of cells that I wanted. They had all been replaced by the use of dynamo machines. The probability is that if we should want to make a similar exhibition in the course of a year, we should have to go out and buy the cells, and it is quite possible that they may go out of the market altogether.

PROF. ALFRED G. COMPTON:—A question occurs to me—not as an expert at all, but simply as a layman—and I should like to ask Mr. Delany about it. The difficulties along the line occasioned by storms are, of course, increased by the bad condition of the line. I wonder whether putting this adjuster into the hands of the operators may not lead to continued neglect in the care of the lines, and if so, whether by-and-by we might not reach a condition in which, even with the adjuster, we might be unable in bad weather to operate at all. I don't know what the minimum of leakage or demoralization on the line might be under which the adjuster itself might become unmanageable. I should like to hear a word on that.

MR. DELANY:—From the experience on railroads in the past, I don't know but it may come about that the gentleman's fears may be verified but I hope that that objection will not be raised against the introduction of the system. As regards the amount of escape which would entirely prevent communication, that would be hard to determine. It would depend entirely on the length of the circuit and the character of the magnets in the circuit and the number of them, and the total extra induced currents from the magnets and the condition of the wire, its resistance, and the pressure and the current generally that was in use. I think, however, that as soon as the railway companies reach the point when they cannot under any circumstances communicate from one terminal to another, and consequently cannot work with intermediate stations either, probably they will be driven to some expedient to improve the condition of the line.

THE SECRETARY:—I would like to announce to the members that the American Society of Mechanical Engineers is in session in this city, and will be for two days, at No. 12 W. 31st St., the Academy of Medicine. They have invited such of our members as have any desire to be present at their sessions, that they will be pleased to have them attend.

The CHAIRMAN:—As electrical engineers are nine-tenths mechanical engineers, according to the definition that Sir William Thomson has given of the electrical engineer, I have no doubt we ought to derive a good deal of instruction and benefit from those meetings, and I am sure that Prof. Hutton and his fellow officers and the members of the Society of Mechanical Engineers will be glad to welcome our members among them.

I would like to announce that the subject of our next meeting, that of December, is a very important and interesting one, namely, "Transformers," upon which subject we shall have the pleasure of hearing Prof. Ryan, of Cornell University, who is associated with our esteemed Vice-President, Prof. Nichols. This paper will embody some very important valuable data, and will be one of the most interesting papers of the season. The paper following that in January, will be given by Prof. Anthony on a subject yet to be named by him.

Adjourned.

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Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St W. C.; weekly. £1.16.
Industries.	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1 8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
India Rubber & Gutta Percha & Electrical Trades Journal.	*I. R. & G. P. Jour.*	London, 115 Finsbury Pavement, E. C ; monthly. 15 shillings.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building; weekly. $3.
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row ; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street ; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 178 Devonshire St.; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute ; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
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Rhoads's Electric Arc Lamp. (Ill.) *Lond. Elec. Rev.*, Sept. 7, p. 261

A New Miner's Lamp. *Elec. World*, Sept. 22, p. 161. (J. P. Rees.) *Lond. Elecn.*, Sept. 21, p. 620; *Lond. Elec. Engr.*, Sept. 21, p. 238.

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A New Search Lamp. (Ill.) (Thomson-Houston Naval Projector.) *Md. Lt. & Ht.*, Sept. 13, p. 334; *Elec. World*, Sept. 15, p. 150.

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Peculiar Behavior of Incandescent Lamps. A. J. Newell. *Elec. World*, Sept. 8, p. 114.

Incandescent Lamps Affected by Static Electricity. John J. Dewey. *Elec. World*, Sept. 22, p. 157.

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The New Constitution. *N. Y. Elec. Rev.*, Sept. 15, p. 5. (Elec. Lt. Assn.)

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Electric Light Applied to Night Navigation upon the Suez Canal. (Ill.) R. Percy Sellon. Brit. Assn., Bath. *Lond. Elecn.*, Sept. 28, p. 669; *Lond. Elec. Rev.*, Sept. 14, p. 279; *Lond. Elec. Engr.*, Sept. 21. p. 241.

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On the Measurement of Electricity in a House-to-House Supply. (Ill.) W. Lowrie. Brit. Assn., Bath. *Lond. Elecn.*, Sept. 21, p. 638; *Lond. Elec Rev.*, Sept. 21, p. 308; *Lond. Elec. Engr.*, Sept. 21, p. 248.

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On a System of Electrical Distribution. (Ill.) Henry Edmunds. Brit. Assn., Bath. *Lond. Elecn.*, Sept. 21, p. 636; *Lond. Elec. Rev.*, Sept. 14, p. 284; *Lond. Elec. Engr.*, Sept. 21, p. 250.

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English Views on Some Recent Patent Decisions. *Indust.; N. Y. Elec. Engr.*, Sept., p. 401. (Alternating current patents, &c.)

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Electric Power Transmission in Switzerland. (Ill.) *N. Y. Elec. Rev.*, Sept. 1, p. 3; *Elec. World*, Sept. 8, p. 115. (Oerlikon motors.)

The Electric Transmission of Power. (Ill.) Prof. Ayrton. Brit. Assn. *Lond. Elecn.*, Sept. 14, p. 598; *Lond. Elec. Rev.*, Sep. 21, p. 302; *Lond. Elec. Engr.*, Sept. 14, p. 230; Sept. 28, p. 272; *Eng.*, Sept. 28, p. 310.

New Engine of War. M. Ramazoti. *West. Elecn.*, Sept. 22, p. 158.

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A Basis from which to Calculate Charges for Electric Motor Service. H. L. Lufkin. Nat. Elec. Lt. Assn. *Elec. World*, Sept. 8, p. 127; *N. Y. Elec. Engr.*, Sept., p. 460; *West. Elecn.*, Sept. 8, p. 137; *N. Y. Elec. Rev.*, Sept. 22, p. 10; *Sci. Am. Supp.*, Sept. 22, p. 10,608; *Md. Lt. & Ht.*, Sept. 6, p. 283.

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Motive Power in Mines. A. E. Ann. (Note.) *Lond. Elec. Rev.*, Sept. 21, p. 314.

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On the Tesla Alternating Current Motor. J. Swinburne. *N. Y. Elec. Engr.*, Sept., p. 400.

A New Electric Dog Cart. (Ill.) *Lond. Elecn.*, Sept. 14, p. 593. (Made by Messrs. Immisch, for the Sultan of Turkey.) *Lond. Elec. Rev.*, Sept. 21, p. 299; *Lond. Elec. Engr.*, Sept. 14, p. 215; *Indust.*, Sept. 21, p. 285; *Eng.*, Sept. 14, p. 264.

A Novel Electrical Boat. (Ill.) *Elec. World*, Sept. 22, p. 157. (Catamaran.)

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The Electric Cable Railway on the Burgenstock. (Note.) *Lond. Elecn.*, Sept. 14, p. 584. *Indust.*, Sept. 7, p. 236; *St. Ry. Gaz.*, Sept., p. 123; *Elec. World*, Sept. 29, p. 171.

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Notes on Three New Sprague Roads. *Elec. World*, Sept. 29, p. 176.

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The Mechanism of Electrolysis by Alternating Currents. J. Chappuis & G. Maneuvrier. *Comptes Rendus: Elec. World*, Sept. 8, p. 130 *Sci. Am. Supp.*, Sept. 1, p. 10,560; *N. Y. Elec. Engr.*, Sept., p. 396.

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On a Constant Daniell Cell, for use as a Standard of Electromotive Force. (Ill.) Cosmo I. Burton. *Sci. Am. Supp.*, Sept. 15, p. 10,598.

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Practical Notes Concerning the Construction, Use and Management of Storage Batteries. A. Reckenzaun. (Cont'd from p. 390, vol. VI) *N. Y. Elec. Engr.*, Sept., p. 391.

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On the Change of Potential of a Voltaic Couple by Variation of Strength of its Liquid. G. Gore. *N. Y. Elec. Engr.*, Sept., . 397.

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Earth for a Lightning Conductor. O. Lodge. *Lond. Elec. Rev.; West. Elecn.*, Sept. 22, p. 156.

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The New York Subways. L. F. Beckwith. Nat. Tel. Ex. Assn. *Elec. World*, Sept. 15, p. 144; *West. Elecn.*, Sept. 15, p. 151; *N. Y. Elec. Rev.*, Sept. 15, p. 13; *Md. Lt. & Ht.*, Sept. 13, p. 325.

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Shallenberger's Alternating Current Meter. (Ill.) *Elec. World*, Sept. 15, p. 149; *Lond. Elecn.*, Sept. 7, p. 556; *Lond. Elec. Rev.*, Sept. 28, p. 349; *Lond. Elec. Engr.*, Sept. 21, p. 239; *Science*, Sept. 28, p. 153; *N. Y. Elec. Engr.*, Sept., p. 382.

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Name.	Abbreviation.	Published, &c.
Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St. W. C.; weekly. £1.16.
Industries	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building: weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $1.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
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Elastic Foundations for Dynamos. (Note.) *Lond. Elec. Engr.* Jan. 18, p. 43. (M. G. Anthoni.)

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The Electric Railway at Washington, D. C. (Ill.) *Elec. World*, Jan. 26, p. 46.

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The Transition from Metallic to Electrolytic Conduction. C. Barns. *Am. Jour. Science; Elec. World*, Jan. 12, p. 17.

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Gas Mains as Electrical Conduits. (Ill.) *Elec. World; Lond. Elec. Rev.*, Jan. 11, p. 46; *Lond. Elec. Engr.*, Jan. 25, p. 67.

Statistics of Wires in Brooklyn. *Elec. World*, Jan. 5, p. 3.

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A New Form of Condenser. (Ill. Corr.) W. Perren-Maycock. *Lond. Elec. Rev.*, Jan. 4, p. 24.

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An Electric Magic Lantern. (Ill.) *Elec. World*, Jan. 12, p. 16.

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Engineering.	*Eng.*	London, 35–36 Bedford St. W. C.; weekly. £1.16.
Industries.	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building; weekly. $3
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The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
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Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St. W. C.; weekly. £1.16.
Industries	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building; weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $1.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
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Mechanics.	*Mech.*	Philadelphia, 907 Arch St.; monthly, $1.
Railroad Gazette.	*R'd. Gaz.*	New York, 73 Broadway; weekly, $4 20.
Street Railway Journal	*St. Ry. Jour.*	New York, 113 Liberty St.; monthly, $2
Street Railway Gazette.	*St. Ry. Gaz.*	Chicago, 9 Lakeside Building; monthly, $2.
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Arc Lamp for Incandescent Circuits. *Science*, March 29, p. 235. (The Silvey.)

An Improved Electric System. (Ill.) *Science*, March 15, p. 192. (The Sperry.)

The "K. & W." Cut Out for Electric Light Circuits. *Elec. World*, March 16, p. 156.

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A New Application for Glow Lamps. *Indust.*, March 1, p. 212.

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NAME.	ABBREVIATION.	PUBLISHED, &C.
Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St W. C.; weekly. £1.16.
Industries	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building: weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row ; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street ; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo.; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $1.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
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Mechanics.	*Mech.*	Philadelphia, 907 Arch St.; monthly, $1.
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German Central Electric Lighting Stations. *Lond. Elecn.; Elec. World*, April 20, p. 229.

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A Compact Plant for Railway Trains. (Ill.) *N. Y. Elec. Rev.*, May 18, p. 8. (J. H. Bunnell & Co.)

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The New Edison Dynamo for Marine Work. (Ill.) *Elec. World,* April 6, p. 201.

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Alternating Current Dynamos. (Ill.) *Lond. Elec. Rev.,* May 17, p. 563.

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The Silvertown Dynamo. *Sci. Am. Supp.,* May 11, p. 11,133.

Right and Left Hand Dynamos. (Ill) *West. Elecn.,* April 13, p. 199.

The Fritsche Wheel Dynamo. (Ill.) *Elec. World,* April 6, p. 205; *N. Y. Elec. Rev.,* April 6, p. 2; *Lond. Elecn.,* April 12, p. 655; (Corr.) *Indust.,* April 12, p. 350; *Sci. Am. Supp.,* May 4, p. 11,120.

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The Kester Dynamos and Motors. (Ill.) *N. Y. Elec. Rev.*, May 11, p. 8.

ELECTRIC MOTORS.

Alternate Current Motors. *Lond. Elec. Rev.; Elec. Pr.*, May, p. 123.

The Tesla Motor. (Ill.) *Sci. Am. Supp*, April 6, p. 11,053.

Mr. Nikola Tesla on Alternating Current Motors. (Corr.) Nikola Tesla. *Elec. World*, May 25, p. 298.

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Ten Horse Power Constant Current Motor for Ampere Circuits. *N. Y. Elec. Rev.*, April 6, p. 9.

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The Edgerton Electric Motor. H. N. Edgerton. *Elec. Pr.*, May, p. 111.

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An Amphibious Motor. (Corr.) Merle J. Wightman. *Elec. World*, April 13, p. 220. (Thomson-Houston waterproof motor.)

The Connecticut Motor Company. (Ill.) *Elec. Pr.*, April, p. 95.

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The Electric Traveling Crane at the Paris Exposition. (Ill.) *Elec. World*, May 4, p. 261.

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Electric Coal Digger. (Ill.) *Science*, April 5, p. 250. (Sperry's.) *Elec. Pr.*, May, p. 123.

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A Unique Electric Power Station. *Science*, April 5, p. 251. (Scranton, Pa.) *Elec. Pr.*, April, p. 85.

Electric Power Transmission at Virginia City, Nev. *Territorial Enterprise; Elec. Pr*, April, p. 100; *N. Y. Elec. Rev.*, May 25, p. 1; *West. Elecn.*, May 25, p. 271; *Elec. World*, May 25, p. 293.

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Electrical Transmission of Power. *Lond. Elec. Rev.*, May 10, p. 526. Wattenser Paper Works, Austria, Hungary.

Electric Power from the Rhine. (Corr.) *Lond. Elec. Engr.*, May 17, p. 387.

A Power Station at Rheinfelden. *Lond. Elecn.*, April 19, p. 674; *Elec. World*, May 11, p. 268.

A New Device. *Md. Lt. & Ht.*, May 23, p. 609. (Lieut. Fiske's Shot Hoist.)

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On the Speed Regulation of a Motor in the Electrical Transmission of Power. M. Marcel Deprez. *Comptes Rendus; Lond. Elec. Rev.*, April 26, p. 468.

Note on the Efficiency of Small Electro-Motors. H. E. H. Clifford. *Lond. Elec. Rev.*, April 12, p. 408.

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Constant Current and Constant Potential Motors. C. O. Mailloux. *Power, Steam; Elec. Pr.*, April, p. 92.

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Canal-Boat Propulsion. H. C. Vogt. Brit. Assn. *Science*, April 12, p. 278.

An Electric Drill. *Md. Lt. & Ht.*, April 25, p. 509. (I. E. Story.)

The Hawkeye Electric Company. *West. Elecn.*, May 4, p. 240.

Davis Motor in a Press Room. (Ill.) *West. Elecn.*, April 20, p. 211.

Lamp Cut-Out for Motor Circuits. (Ill.) *West. Elecn.*, April 20, p. 211.

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The Daft "Double Trolley" Road at Cincinnati, O. (Ill.) *Elec. World*, May 25, p. 295; *West. Elecn.*, May 25, p. 275; *Science*, May 24, p. 391.

Largest Electric Railway System in the World, Topeka, Kans. *Md. Lt. & Ht.*, April 11, p. 461; May 23, p. 610; *Science*, April 26, p. 319; *N. Y. Elec. Rev.*, May 4, p. 2; May 25, p. 10; *West. Elecn.*, May 25, p. 277; *Elec. World*, May 25, p. 296.

The Electric Railway in St. Joseph, Mo. (Ill.) *Science*, April 5, p. 252.

The Thomson-Houston System on West End Street Railway, Boston. (Ill.) *Elec. World*, April 6, p. 204; May 11, p. 269; *Md. Lt. & Ht.*, April 4, p. 427; May 9, p. 557; *N. Y. Elec. Engr.*, May, p 206.

Rapid Transit in Boston. *Md. Lt. & Ht.*, May 16, p. 586.

Passed Unanimously. (West End Railway Company.) *Md. Lt. & Ht.*, May 9, p. 557.

Drawbridge on Cambridge Division of West End Street Railway. (Ill.) *West. Elecn.*, April 6, p. 186.

Electric Propulsion on the New York Elevated Railways. (Ill.) *Lond. Elec. Rev.*, April 12, p. 429.

A New Daft Electric Street Car. (Ill.) *Elec. World*, April 20, p. 229.

New Bentley-Knight Double Motor Truck. (Ill.) *Elec. World*, May 4, p. 258; *West. Elecn.*, May 4, p. 235.

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Improvements in the Bentley-Knight Electric Railway System. (Ill.) *N. Y. Elec. Rev*, May 4, p. 3; *Elec. Pr.*, May, p. 112.

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Thomson-Houston Electric Roads. *Elec. World*, May 4, p. 262.

Carbon Brush for Railway Motors. *Elec. Pr.*, May, p. 125. (Thomson-Houston.)

The Thomson-Houston Electric Railway Snow Broom. (Ill.) *Elec. World*, April 13, p. 220; *Elec. Pr.*, May, p. 132; *Science*, April 12, p. 273; *N. Y. Elec. Rev.*, April 13, p. 3; *Md. Lt. & Ht.*, April 11, p. 462.

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Electric Street Railways in America. *N. Y. Elec. Engr*, April, p. 94.

Electric Locomotives. *Elec. Pr.*, April, pp. 96, 102. (Daft, Ninth Avenue.)

Sprague Electric Railway, St. Joseph, Mo. (Ill.) *Elec. Pr.*, April p. 96.

Riverside and Suburban Railway, Wichita, Kan. (Ill.) *Elec. Pr.*, April, p. 84.

Sprague Electric Railway at Akron, Ohio. (Ill.) *Elec. World*, April 27, p. 245; *Elec. Pr.*, May, p. 116; *N. Y. Elec. Rev.*, April 27, p. 10; *West. Elecn.*, April 27, p. 227.

Recent Additions to the Sprague Electric Roads. *Lond. Elec. Rev*, April 12, p. 420. (St. Joseph, Harrisburg & Wilmington.) April 19, pp. 455–457. (Ill.) Brockton, Mass., Reading. *Elec. World*, April 6, p. 208.

Electric Locomotive for Mines. *Elec. Pr.*, April, p. 86.

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Storage Batteries for Street Cars. (Ill.) *Science*, May 3, p. 331. (Julien.)

Secondary Battery Car in Detroit. *Elec. Pr.*, April, p. 101; *Science*, April 19, p. 297.

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Julien Storage Battery Cars. *West. Elecn.*, April 20, p. 217.

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The "Edco" Storage Car in Philadelphia. *Ledger; Elec. World*, April 20, p. 234; May 11, p. 271; *N. Y. Elec. Rev.*, April 27, p. 2; *West. Elecn.*, May 4, p. 237; *West. Elecn.*, April 20, p. 213.

Electric Street Railways. Fred. H. Whipple. *West. Elecn.*, May 4, p 240.

Producing, Transmitting and Applying Electricity for Street Railway Purposes. Fred. H. Whipple. *West. Elecn.*. May 25, p. 273.

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Increased Traction Due to Electric Currents. (Corr) Elias E. Ries. *Elec. Pr.*, May, p. 134

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Electric Railways. Eugene Griffin. Bost. Soc. Arts. *Md. Lt. & Ht.*, May 2, p 534; May 9, p. 556. Merchants Club. *Md. Lt. & Ht.*, May 16, p. 588; *N. Y. Elec. Rev.*, May 11, p. 2.

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Portelectric Transportation. (Ill.) *Md. Lt. & Ht.*, May 23, p. 612.

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The Friction Electric System for Street Railroads. *N. Y. Elec. Rev.*, April 13, p. 3.

Sprague Electric Transfer Table at Altoona, Pa. (Ill.) *Elec. World*, April 13, p. 219; *Elec. Pr.*, April, p. 91.

The White Rail for Electric Street Railways. (Ill.) *Elec. World*, April 27, p. 250; May 18, p. 286. (R. T. White.) *Science*, May 24, p. 396; *Md. Lt. & Ht.*, May 16, p. 587; *N. Y. Elec. Rev.*, April 27, p. 10; May 18, p. 8.

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On the Growth of City Traffic in New York and London. A. Reckenzaun. *Lond. Elec. Rev.*, May 24, p. 588.

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Re-Series Line at Northfleet. (Corr.) E. Manville. *Lond. Elec. Rev.*, April 12, p. 436. Wilfred L. Spence. April 19, p. 464

An Engine-Dynamo Accumulator Car. *Lond. Elec. Engr.*, April 12, p. 287.

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The "National Galvanic" Open Circuit Battery. (Ill.) *Elec. World*, April 20, p. 234 ; *N. Y. Elec. Rev.*, April 20, p. 1 ; *West. Elecn.*, April 20, p. 211.

The Holtzer Cylinder Battery. (Ill.) *Elec. World*, April 13, p. 220.

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The "Duplex" Open Circuit Battery. (Ill.) *Elec. World*, May 25, p. 302.

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A New Method of Testing Batteries at Work. *N. Y. Elec. Rev.*, May 25, p. 7.

Mance's Battery Resistance Test. (Corr.) R. L. Hippisley. *Lond. Elecn.*, April 5, p. 634.

On Batteries. A. E. Dolbear. *Md. Lt. & Ht.*, May 16, p. 587.

The Arrest of Growth of Cancer by Powerful Interrupted Voltaic Currents. J. I. Parsons. *Lond. Elec. Rev.*, May 3, p. 498.

Relative Amounts of Voltaic Energy of Dissolved Chemical Compounds. G. Gore. Roy. Soc. *Elec. World*, April 20, p. 232 ; *Lond. Elecn.*, April 26, p. 718.

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On the Loss of Voltaic Energy of Electrolytes During Chemical Union. G. Gore. Phil. Soc., Birmingham. *Lond. Elecn.*, April 5, p. 626.

Relative Amounts of Voltaic Energy of Electrolytes. G Gore. Roy. Soc. *Lond. Elecn.*, April 5, p. 626.

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Experiments with the Accumulators of Farbaky and Schenck. *La Lumière Électrique; N. Y. Elec. Rev.*, May 4, p. 8.

The Macraeon Storage Battery. (Ill.) *West. Elecn.*, April 13, p. 197.

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Views on the Coxe Decision as to Storage Batteries. *Elec. World*, April 6, p. 206. (D. H. Bates.)

Storage Battery Patents. (Corr.) J. Irving Courtenay. *Lond. Elecn.*, April 19, p. 692.

Storage Battery Litigation. *N. Y. Elec. Engr.*, April, p. 194. (Electrical Accumulator Co. *vs.* Julien Electric Company.)

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Prescott's System of Storage Battery Distribution. (Ill.) *Elec. World*, May 11, p. 273.

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Electrolytic Treatment of Sewage. (Ill.) *West. Elecn.*, April 20 p. 210. (Webster's.)

Electrolytic Dissociation. Prof. W. Ostwald. *Lond. Elecn.*, May 17, p. 30.

On Ostwald's Experiments on Electrolytic Dissociation. J. Brown. *Lond. Elecn.*, April 19, p. 676. O. J. Lodge, p. 619.

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NAME.	ABBREVIATION.	PUBLISHED, &C.
Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St. W. C.; weekly. £1.16.
Industries.	*Indust.*	358 Strand, London W. C. weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 167–177 Times Building; weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3
Electric Power.	*Elec. P'wr.*	New York, 132 Nassau St. monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo.; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $2.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
Scientific American Supplement.	*Sci. Am. Supp.*	New York, 361 Broadway; weekly, $5.
Mechanics.	*Mech.*	Philadelphia, 907 Arch St.; monthly, $1.
Railroad Gazette.	*R'd. Gaz.*	New York, 73 Broadway; weekly, $4 20.
Street Railway Journal	*St. Ry. Jour.*	New York, 113 Liberty St.; monthly, $2.
Street Railway Gazette.	*St. Ry. Gaz.*	Chicago, 9 Lakeside Building; monthly, $2.
Light, Heat and Power.	*Lt. Ht. and Pr.*	Philadelphia, Drexel Building; weekly, $4.
Progressive Age.	*Prog. Age.*	New York, 18 Broadway; semi-monthly, $2.
Railroad and Engineering Journal.	*Rd. and Eng. Jour.*	New York, 45 Broadway; monthly, $3.

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A New Portable and Stationary Tachometer. (Ill.) *Elec. World*, June 22, p. 364.

Reference List of Leading Authorities Cited in the Index of Current Electrical Literature.

NAME.	ABBREVIATION.	PUBLISHED, &C.
Engineer.	*Engr.*	London, 163 Strand. weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St W. C.; weekly. £1.16.
Industries.	*Indust.*	358 Strand, London W. C. weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1 2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 150 Broadway; monthly, $3
The Electrical World.	*Elec. World.*	New York, 167–177 Times Building; weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3
Electric Power.	*Elec. P'wr.*	New York, 132 Nassau St. monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $2.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
Scientific American Supplement.	*Sci. Am. Supp.*	New York, 361 B. 'way; weekly, $5.
Mechanics.	*Mech.*	Philadelphia, 9[illegible] Arch St.; monthly, $1.
Railroad Gazette.	*R'd. Gaz.*	New York, 73 Broadway; weekly, $4 20.
Street Railway Journal	*St. Ry. Jour.*	New York, 113 Liberty St.; monthly, $2.
Street Railway Gazette.	*St. Ry. Gaz.*	Chicago, 9 Lakeside Building; monthly, $2.
Light, Heat and Power.	*Lt. Ht. and Pr.*	Philadelphia, Drexel Building; weekly, $4.
Progressive Age.	*Prog. Age.*	New York, 18 Broadway; semi-monthly, $2.
Railroad and Engineering Journal.	*Rd. and Eng. Jour.*	New York, 45 Broadway; monthly $3.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

LIST OF MEMBERS.

OCTOBER 1ST, 1889.

HONORARY MEMBERS.

Name.	Address.	Date of Membership.
PREECE, WM. H. *F. R. S.*	Electrician, General Post Office, London, Eng. Residence, Gothic Lodge, Wimbledon.	Oct. 21, 1884

MEMBERS.

Name.	Address.	Date of Membership.
ACHESON, EDW. G.	Electrician, Box 952, Pittsburgh, Pa.	A Jan. 3, 1888 M May 1, 1888
AHRARN, T.	Ahearn & Soper, Electrical Supplies, Ottawa, Ont.	A July 12, 1887 M Sept. 6, 1887
ANTHONY, PROF. W. A.	Consult'g Electrician. The Mather Electric Co., Manchester, Conn.	A Dec. 9, 1884 M Jan. 6, 1885
BATCHELLER, B. C.	Superintendent of the Pneumatic Dynamite Gun Co., 71 Broadway, New York.	A Sept. 6, 1887 M Nov. 1, 1887
BATCHELOR, CHAS.	Electrical Engineer, 33 West Twenty-fifth St., New York.	A June 8, 1887 M July 12, 1887
BAYLY, B.	Surveyor and District Engineer, Western District Colonial Gov't. Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6. 1887
BELL, PROF. A. GRAHAM	1336 Nineteenth St., Washington, D. C.	A April 15, 1884 M Oct. 21, 1884
BERNARD, EDGAR G.	Electrical Engineer, Utica, N. Y.	A Jan. 5, 1886 M July 12, 1887
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BLAKE, EDWARD	Treasurer of New England Electric Co., 55 Oliver St., Boston, Mass.	A May 24, 1887 M Sept. 3. 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

Name.	Address.	Date of Membership.
BRADLEY, CHAS. S.	Electrical Engineer, Yonkers, N. Y.	A May 24, 1887 M Dec. 6, 1887
BROWN, JO. STANFORD	Electrical Engineer, with Daft Electric Light Co., 115 Broadway, New York.	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
BYLLESBY, HENRY M.	Westinghouse Electric Co., Pittsburgh, Pa.	A Sept. 7, 1888 M Oct. 2, 1888
CHAMBERLAIN, J. C.	Supt. Julien Electric Traction Co., Madison Ave. & Eighty-Fifth St., New York.	A Dec. 6, 1887 M Jan. 3, 1888
CHEEVER, CHAS. A.	13 Park Row, New York.	A April 15, 1884 M Oct. 21, 1884
CHURCH, WM. LEE	Member of Westinghouse, Church, Kerr & Co., Engineers, 620 Atlantic Ave., Boston, Mass.	A April 5, 1887 M May 17, 1887
CLARK, ERNEST P.	Electrical Engineer, 192 Broadway, New York.	A Jan. 8, 1887 M Nov. 1, 1887
CLARKE, CHAS. L.	Mechanical and Electrical Engineer, Gibson Electric Co., 74 Cortlandt St., New York.	A April 15, 1884 M Jan. 6, 1885
COLBY, EDWARD A.	Electrician of the Weston Electrical Instrument Co., 606 High St., Newark, N. J.	A April 2, 1889 M May 7, 1889
CONANT, THOMAS P.	General Supt. of Construction, United Edison Mfg. Co., 65 Fifth Ave., New York.	A Nov. 1, 1887 M Jan. 3. 1888
CONDICT, G. HERBERT	General Manager, The Electric Car Co. of America, 4720 Green St., Germantown, Pa.	A July 12, 1887 M Sept. 6, 1887
COWLES, ALFRED H.	Technical Adviser to the Cowles Smelting and Aluminum Co., Lockport, N. Y.	A Mar. 5. 1889 M May 7, 1889
CROCKER, FRANCIS B.	(*Manager*). Instructor in Electrical Engineering, School of Mines, Columbia College, New York.	A May 24. 1887 M April 2, 1889
CROSS, 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
CUTTRISS, CHAS.	(*Manager*). Electrician, The Commercial Cable Co., 1 Broad St., New York.	A Nov. 1, 1887 M Dec. 6, 1887
DAFT, LEO	Electrician, Plainfield, N. J.	A Dec. 9. 1884 M Jan. 6, 1885
D'ANVERS, ALAN	Managing Director and Chief Electrician, The Anglo-Portugese Telephone Co., L't'd. 60 Travessa Santa Justa, Lisbon, Port'l.	A Nov. 1, 1887 M Sept. 3, 1889
D'ANVERS, ERNEST	(Casilla 74) Bajada 48, Rosario de Santa Fe, Argentine Republic.	A Jan. 3, 1888 M May 1, 1888
DIEHL, PHILIP	Inventor, Singer Sewing Machine Co., Elizabeth, N. J.	A April 15, 1884 M Dec. 9, 1884
D'INFREVILLE, GEORGES	Electrician, Western Union Telegraph Co., 195 Broadway, New York.	A Nov. 1, 1887 M Dec. 6, 1887

Name.	Address.	Date of Membership.
DUNCAN, DR. LOUIS	(*Vice-President*). Johns Hopkins University, Baltimore, Md.	A July 12, 1887 M Sept. 6, 1887
DYER, R. N.	Patent Attorney. 40 Wall St., New York.	A July 12, 1887 M Sept. 6, 1887
EDISON, THOMAS A.	Electrician and Inventor, Orange, N. J.	A April 15, 1884 M Oct. 21, 1884
FARNHAM, ISAIAH H.	Electrician for the N. E. Telephone & Telegraph Co., 50 Pearl St., Boston, Mass.	A June 8, 1887 M July 12, 1887
FIELD, C. J.	General Manager and Engineer of Edison Electric Illuminating Co. of Brooklyn, 358–362 Pearl St., Brooklyn, N. Y.	A June 8, 1887 M Nov. 1, 1887
FIELD, STEPHEN D.	Electrician and Inventor, Stockbridge, Mass.	A April 15, 1884 M Oct. 21, 1884
FLEMING, W. H.	Care of Electric Club, New York.	A Dec. 6, 1887 M Jan. 3, 1888
FOSTER, HORATIO A.	Electrical Engineer, 425 East Twenty-fourth St., New York.	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
GARRATT, ALLAN V.	Secretary and Treasurer of the National Electric Light Association, 18 Cortlandt St., New York.	A April 2, 1889 M May 7, 1889
GARVER, M. M.	645 High St., , Newark, N. J.	A July 10, 1888 M Sept. 3, 1889
GEYER, PROF. WM. E.	(*Manager*). Stevens Institute of Technology, Hoboken, N. J.	A June 5, 1888 M Sept. 7, 1888
GISBORNE, HARTLEY, *C. E.*	District Superintendent and Electrician, Government Telegraph Service of Canada, Qu'Appelle Station, Assa., N. W. Territory, Canada.	A Dec. 9, 1884 M May 17, 1887
GREEN, DR. NORVIN	(*Past President*). Pres't Western Union Telegraph Co., 195 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
HALL, CLAYTON C.	Civil Engineer, 810 Park Ave., Baltimore, Md.	A April 15, 1884 M Oct. 21, 1884
HAMBLET, JAMES	Manager Time Service, W. U. Tel. Co., 195 Broadway, P. O. Box 3393, New York.	A Nov. 1, 1887 M Dec. 6, 1887
HAMILTON, GEO. A.	Electrician, Western Electric Co., 22 Thames cor. Greenwich St., New York.	A April 15 1884 M Oct. 21, 1884
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HASKINS, CHARLES H.	Electrician, care of New York Club, Fifth Ave. and Thirty-fifth St., New York.	A April 15, 1884 M Oct, 21, 1884
HAZARD, COL. ROWLAND R.	N. Y. District Railway Co., 120 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
HAYNES, F. T. J.	Divisional Telegraph Engineer, Great Western Railway, Taunton, Eng.	A Dec. 6, 1887 M Jan. 3, 1888

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Hering, Carl	Consulting Electrical Engineer, 3816 Spring Garden St., Philadelphia, Pa.	A Jan. 3, 1888 M June 5, 1888
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Howell, John W.	(*Manager*). Electrician, Edison Lamp Co., Harrison N. J.	A July 12, 1887 M June 5, 1888
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Hyde, Jerome W.	Springfield, Mass.	A June 8, 1887 M Nov. 1, 1887
Jenks, W. J.	Technical Department, United Edison Manufacturing Co., 65 Fifth Ave., New York.	A June 8, 1887 M Nov. 1, 1887
Johnson, E H.	President, Sprague Electric Railway & Motor Co., 16 Broad St., New York.	A Feb. 7, 1888 M May 1, 1888
Jones, Francis W.	Assistant Gen'l-Manager and Electrician, Postal-Telegraph Cable Co., 187 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
Knowles, E. R.	Electrical Engineer, 439*a* Waverly Ave., Brooklyn, N. Y.	A June 8, 1887 M July 12, 1887
Knudson, A. A.	Electrician, Care of American Institute of Electrical Engineers, 5 Beekman St., New York.	A Dec. 6. 1887 M Jan. 3, 1888
Lange, Philip A.	Superintendent of Detail Department, Westinghouse Electric Co., Pittsburgh, Pa.	A Mar. 6, 1888 M June 5, 1888
Langton, John	General Manager, Canadian Edison M'f'g Co., Sherbrooke, P. Q.	A Mar. 6, 1388 M June 5. 1888
Lattig, J. W.	Sup't Telegraph, Lehigh Valley R. R., South Bethlehem, Pa	A June 8, 1887 M July 12, 1887
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Leonard. M. B.	Electrical Engineer, and Supt. of Telegraph, Chesapeake & Ohio R'y. Co., Richmond, Va.	A Nov. 6, 1886 M May 1, 1888
Lever, Charles	Electric Light Engineer, Bowdon, Cheshire, Eng.	A Dec. 6, 1887 M Jan. 3, 1888
Lieb, John W., Jr.	Chief Engineer. Società Generale Italiana di Elettricita (Sistema Edison). Via S. Radigonda N. 4, Milan, Italy.	A Sept. 6, 1887 M Nov. 1, 1887
Lockwood, Thomas D.	(*Manager*). Electrical Engineer. and Advisory Electrician. P O., Drawer 2, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
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Marks, William Dennis,	*Ph. B., C. E.* Engineer-in-Chief, Edison General Electric Co., 44 Wall St., New York.	A Feb. 7, 1888 M May 1, 1888

Name.	Address.	Date of Membership.
MAYNARD, GEO. C.	Electrical Engineer, 1409 New York Ave., Washington, D. C.	A April 15, 1884 M Dec. 9, 1888
MICHAELIS, OTHO ERNEST,	*Ph. D.* (*Vice-President*). Major of Ordnance, U. S. Army, Commanding Kennebec Arsenal, Augusta, Me.	A Dec. 9, 1884 M Jan. 6, 1885
MILLIS, JOHN	Lieutenant of Engineers, U. S. Army, P.O. Box 2128, New York.	A July 7, 1884 M Mar. 3, 1885
MILLS, F. P.	Superintendent Cleveland Iron Mining Co., Ishpeming, Mich.	A Jan. 6, 1885 M Mar. 3, 1885
NICHOLS, EDWARD L.	(*Vice-President*). Professor of Physics at Cornell University, Ithaca, N. Y.	A Oct 4, 1887 M Dec. 6, 1887
PAINE, SIDNEY B.	Electrical Constructor, member of firm "Paine & Francis," 38 Pearl St., Boston, Mass.	A June 8, 1887 M Nov. 1, 1887
PARKS, C. WELLMAN	Electrician. 1825 Fifth Ave., Troy, N. Y	A July 12, 1887 M May 1, 1888
PATTEN, F. JARVIS,	Lieutenant U. S. A., Electrician, 23 East Twenty-fourth St., New York.	A Sept. 6, 1887 M Nov. 1, 1888
PFANNKUCHE, GUSTAV	Engineer and Inventor, Cleveland, O.	A Feb. 7, 1888 M May 1, 1888
POPE, FRANKLIN L.	(*Past President*). Expert for Westinghouse Electric Co., Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
PORTER, J. F.	Pres't and Gen'l Manager, Central Electric Construction Co. and Pres't, Southern Elec. Supply Co. 919 Spruce St., St. Louis, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
PRATT, J. HOWARD	Professor of Physics, Jacksonville, Ill.	A June 5, 1888 M Sept. 7, 1888
PRATT, ROBERT J.	Electrician. Manager, Electric M'f'g Co., Troy, N. Y.	A July 12, 1887 M Sept. 6, 1887
PRESCOTT, GEO. B., JR.	(*Manager*). Electrical Engineer, Electrician to Electrical Accumulator Co., 44 Broadway, New York.	A July 12, 1887 M Nov. 1, 1887
RAYMOND, CHAS. W.	Civil and Electrical Engineer, 36 Dearborn St., Chicago, Ill.	A June 8, 1887 M May 17, 1887
RECKENZAUN, ANTHONY	Electrical Engineer, 7 Albert Terrace, Hemberton Road, Stockwell, London, S. W. England.	A Nov. 1, 1887 M Dec. 6, 1887
RECKENZAUN, FREDERICK,	Electrician, 220 Sutter St., San Francisco, Cal.	A Mar. 6, 1888 M June 5, 1888
RICE, E. WILBUR, JR.	General Superintendent and Consulting Electrician, Thomson-Houston Elec. Co., Lynn, Mass	A Dec. 6, 1887 M Jan. 3, 1888
RIES ELIAS E.	Electrician and Electrical Engineer, 430 South Broadway, Baltimore, Md.	A July 12, 1887 M Sept. 6, 1887
ROBERTS, E. P.	Electrician and Sup't, Swan Lamp Co., Belden St., Cleveland, O.	A Jan. 6, 1885 M Feb. 3, 1885
ROHRER, ALBERT L.	Electrical Engineer, with Thomson-Houston Electric Co., Lynn, Mass.	A Nov. 1, 1887 M May 1, 1888

Name.	Address.	Date of Membership.
SALOMONS, *Sir* DAVID LIONEL, *Bart. M. A.*,	Engineer and Barrister, Broomhill, Tunbridge Wells, Kent, and 49 Grosvenor St., London, W., England.	A Feb. 7, 1888 M May 1, 1888
SCHLESINGER, WM. M.	Electrician of Schlesinger, Kimball & Co., Manufacturers of Electric Mining Machinery, 228 West Broad St., Columbus, O.	A Nov. 1, 1887 M Dec. 6, 1887
SHALLENBERGER, O. B.	Electrician, Westinghouse Electric Co., Pittsburgh, Pa.	A Sept. 7, 1888 M Dec. 4, 1888
SLATER, HENRY B.	Electrical Engineer and Electro Metallurgist, Leadville, Col.	A April 15, 1884 M Dec. 9, 1884
SMITH, HERBERT G.	New York and Honduras Rosario Mining Company, San Juancito, Honduras, C. A.	A Mar. 6, 1888 M June 5, 1888
STANDFORD, WILLIAM	Ass't Sup't Telegraphs, Colonial Gov't, Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6, 1887
STIERINGER, LUTHER	Electrical Expert, 44 Wall St., Room 71, New York.	A June 8, 1887 M Nov, 1, 1887
STUMP, CLARENCE E.	Bus. M'g'r and Treas'r, The W. J. Johnston Co., Ltd., Publishers of the *Electrical World*, 167–177 Times Building, New York.	A May 17, 1887 M May 17, 1887
TERRY, CHARLES A.	Lawyer, Westinghouse Electric Co, Pittsburgh, Pa.	A April 5, 1887 M May 17, 1887
THURNAUER, ERNST	Chief Engineer, Thomson-Houston International Elec. Co , Michaelisbrücke, Hamburg, Germany.	A Oct. 14, 1887 M Dec. 6, 1887
TROWBRIDGE, PROF. W. P.	School of Mines, Columbia College, New York.	A April 15, 1884 M Oct. 21, 1884
TURNER, WILLIAM S.	Electrical Engineer, Woodbridge & Turner, 74 Cortlandt St., New York.	A Dec. 7, 1886 M Oct. 2, 1888
VAIL, J. H.	Electrical Engineer, Ass't to the Pres't, Sprague Electric Railway & Motor Co. 16 Broad St., New York.	A June 8 1887 M Nov. 1, 1887
VAIL, THEO. N.	President, Metropolitan Telephone & Telegraph Co., 18 Cortlandt St., New York.	A April 15, 1884 M Oct. 21, 1884
VAN DEPOELE, CHARLES J.	Electrician, Electrical Railway Department, Thomson-Houston Electric Co., Lynn, Mass.	A June 5, 1888 M Sept. 7, 1888
VANSIZE, WILLIAM B.	Solicitor of Patents, 44 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
WADDELL, MONTGOMERY	Engineer, The Edison Machine Works, Schenectady, N. Y.	A Feb. 7, 1888 M May 1, 1888
WALKER, SYDNEY F.	Electrical Engineer, 195 Severn Road, Cardiff, England.	A June 2 1885 M May 17, 1887
WALDO, DR. LEONARD	Electrical Engineer, The Aluminum Brass & Bronze Co., Bridgeport, Conn.	A June 5, 1888 M Dec. 4, 1888
WEAVER, W. D.	Lieutenant U. S. N., Brooklyn Navy Yard. Residence, 90 Pierpont St , Brooklyn, N. Y.	A May 17, 1887 M May 17, 1887

Name.	Address.	Date of Membership.
Weeks, Edwin R.	General Manager, Kansas City Electric Light Co., National Bank of Kansas City Building. Kansas City, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
Weston, Edward	(*Past President* and *Vice-President*). Electrician, 645 High St., Newark, N. J.	A April 15, 1884 M Oct. 21, 1884
Wetzler, Joseph	(*Manager*). Editor, *The Electrical World*, Times Building. New York.	A April 15, 1884 M Dec. 9, 1884
Wheeler, Schuyler S.	(*Manager*). President Crocker-Wheeler Electric Motor Co., 322 Seventh Ave., Electrical Expert, Board of Electrical Control, 1266 Broadway, New York.	A June 2, 1885 M Sept. 1, 1885
Wilson, Fremont	Electrician, 293 Lenox (6) Ave., New York.	A Mar. 6, 1888 M June 5, 1888
Winchester, A. E.	Designer of Steam Electrical Plants. Wilton, Conn.	A June 8, 1887 M Nov. 1, 1887
Woodbridge, J. L.	Electrical Engineer, Woodbridge & Turner, 74 Cortlandt St., New York,	A June 8, 1887 M Nov. 1, 1887
Zetsche, Dr. Carl Eduard,	Telegraph Engineer, N. Tieckstrasse 7, Dresden, Saxony.	A Nov. 1, 1887 M Jan. 3, 1888

Members, - - - 125.

ASSOCIATE MEMBERS.

Name.	Address.	Date of Membership.
Abernethy, J. P.	Superintendent of Telegraph, 53 City Hall, Cleveland, O.	July 7, 1884
Adams, H. C.	New York Agent, Fort Wayne Jenney Electric Light Company, 115 Broadway, New York.	April 15, 1884
Anderson, Alexander	Superintendent and Manager of the Thomson-Houston Electric Co., Norfolk, Va.	Sept. 6, 1887
Andrews, Wm. S.	Vice-President, Leonard & Izard Co., 425, The Rookery, Chicago, Ill.	Mar. 5, 1889
Backstrom, Chas. A.	Manufacturer of Electric Incandescent Lamps, 12 Jacob St., New York.	Nov. 13, 1888
Barton, Enos M.	President Western Electric Co., 227 South Clinton St., Chicago, Ill.	July 12, 1887
Barrett, John A.	Electrician, 18 Cortlandt St , New York	June 8, 1887
Bates, D. H.	Vice-President and Gen'l Manager, The Electrical Accumulator Co., 44 Broadway, New York.	April 15, 1884
Bates J. H.	Box 2702, New York.	Sept. 6, 1887
Bates, Maurice E.	Manager, Western Electric Construction Co., 503 Delaware St., Kansas City, Mo.	Aug. 6, 1889
Beattie John, Jr.	Manager and Superintendent, The Beattie Battery, Zinc and Electric Co., Fall River, Mass	Sept. 6, 1887

Name.	Address.	Date of Membership.
Berhgoltz, Herman	Mechanical Draughtsman, Thomson-Houston Electric Co., Lynn, Mass.	April 2, 1889
Berliner, Emile	Inventor, Columbia Road, between Fourteenth and Fifteenth Sts, Washington, D. C.	April 15, 1884
Blackwell, R. W.	Civil and Electrical Engineer, 27 Tenth Ave., New York.	July 7, 1884
Blake, Francis	Auburndale, Mass.	Sept. 3, 1889
Blake, Henry W.	Agent, Sprague Electric Railway & Motor Co., 16 Broad St., New York.	Nov. 13, 1888
Blood, W. Henry, Jr.	Electrical Engineer, Thomson-Houston Electric Co., Lynn, Mass.	April 2, 1889
Bogart, A. Livingston	Electrical and Patent Expert, 22 Union Square, New York,	July 10, 1888
Bottomley, Harry	Electrical Engineer, Thomson-Houston Electric Co., Lynn, Mass.	April 2. 1889
Boynton, Edward C.	General Superintendent and Electrician. Robinson-Foster Electric Co., Peabody, Mass.	Aug. 6, 1889
Bracken, William	President, The Julien Electric Company, 120 Broadway, New York.	May 24, 1887
Brackett, Prof. Cyrus F.	Princeton, N. J.	April 15, 1889
Brady, Paul T.	Superintendent, Central N. Y. Telephone & Telegraph Co., Cooperstown, N. Y.	July 12, 1887
Briner, C. J.	Vice-President, Central Electric Construction Co., 919 Locust St., St. Louis, Mo.	April 2, 1889
Brooks, David	President, Brooks Underground Tel. Co., Philadelphia, Pa,	June 5, 1888
Brophy, William	Inspector and Electrical Expert, The New England Insurance Exchange, Boston. Residence, 51 Salem St., Worcester, Mass.	Mar. 5, 1889
Brown, Charles A.	Manager Western Electric Co., 227 South Clinton St., Chicago, Ill.	July 12, 1887
Browning, Howard L.	Promoter and Contractor, 41 Dey St., New York.	Dec. 6, 1887
Buckingham, Chas. L.	Patent Attorney, Western Union Telegraph Co., 195 Broadway, New York.	April 15, 1884
Case, Willard E.	6 Fort St., Auburn, N. Y.	Feb. 7, 1888
Child, Frank W.	Contractor and Builder of Electric Railways, 115 Broadway, New York.	July 10, 1888
Childs, W. H.	Bookkeeper for The Estey Organ Co., Brattleboro, Vt.	Sept. 6, 1887
Chinnock, C. E.	Edison United M'f'g Co., 65 Fifth Ave., New York.	April 15, 1884
Chubbuck, H Eugene	Electrician and Manager, Champion City Electric Light Co., Springfield, O,	Dec. 4, 1888
Cleveland, Wm. B.	Electrical Engineer, 309 Perry-Payne Building, Cleveland, O.	April 15, 1884
Coffin, Chas. A.	Vice-President and Treasurer, Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	Dec. 6, 1887

Name.	Address.	Date of Membership.
Collins, W. Forman	Electrical Engineer, 346 West Twenty-first St., New York.	Dec. 6. 1887
Compton, Alfred G.	Professor of Physics, College of the City of New York, 17 Lexington Ave., New York.	Nov. 1, 1887
Cothren, Wm. H.	Superintendent of Wiring Department. Edison Electric Illuminating Co., 432 Fifth Ave., New York.	Aug. 6, 1889
Crane, W. F. D.	Electrician and Mechanical Engineer, The Electric Car Co. of America, Twenty-third St. and Washington Ave., Philadelphia, Pa	Feb. 7, 1888
Curtis, Chas. G.	114 East Thirtieth St., New York.	April 15, 1884
Curtiss, George F.	Electrician, Thomson-Houston Electric Co., Lynn, Mass.	April 2, 1889
Cuntz, Johannes H.	Assistant to Pres't Henry Morton, Stevens Institute of Technology, 137 Hudson St., Hoboken, N. J.	Mar. 5, 1889
Cushman, Holbrook	Electrical Engineer, 337 West Twenty-second St., New York.	June 5, 1888
Dana, R. K.	Agent Washburn and Moen M'f'g Co., 16 Cliff St, New York.	April 15, 1884
Danforth, A. H.	Vice-Pres't and General Manager, Colorado Coal & Iron Co., South Pueblo, Col.	July 12, 1887
Davenport, George W.	General Manager, Thomson-Houston International Electric Co, 620 Atlantic Ave., Boston, Mass.	June 4, 1889
Davis, Chas. M.	Superintendent, Light, Heat & Power Co., Pueblo, Col.	July 12, 1887
Davis, Delamore L.	Superintendent, Salem Electric Light & Power Co., Salem, O.	April 2, 1889
Davis, Joseph P.	Consulting Engineer, 18 Cortlandt St., New York.	April 15, 1884
Davis, Minor M.	Ass't Electrician. Postal Telegraph Cable Co, 5 Dey St., New York.	April 6, 1886
Delafield, A. Floyd, *Ph.D.*	Electrical Engineer, Noroton, Conn.	May 7, 1889
Delany, Patrick Bernard,	Inventor, 84 Broad St., New York.	April 15. 1884
Denton, James E.	Professor of Experimental Mechanics, Stevens Institute of Technology, Hoboken, N. J..	July 12, 1887
Dickenson, Samuel S.	Sup't, Commercial Cable Co., Hazel-Hill, Guysborough Co., N. S.	Mar. 6, 1888
Dickerson, E. N., Jr.	Attorney-at-Law, 5 Beekman St., New York.	April 15, 1884
Doane, S. Everett	Thomson-Houston Electric Co., Swampscott, Mass.	Aug. 6, 1889
Dobbie, Robert S.	Electrician, The Van Gestal Electric Street Car Co., 510 West Fifty-fourth St, New York.	Feb. 5 1889
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, 92 Lexington Ave., New York.	July 7, 1884

Name.	Address.	Date of Membership.
DURANT, GEO. F.	Vice-Pres't of Bell Telephone Co. of Mo., 322 Pine St., St. Louis, Mo.	April 15, 1884
EMMET, HERMAN L. R.	Publisher and Printer, 36 Cortlandt St., New York.	April 15, 1884
FARMER, PROF. MOSES G.	Electrician and Inventor, Eliot, Me.	April 15, 1884
FIELDING, FRANK E.	Chemist and Assayer, Virginia City, Nev.	Sept. 6, 1887
FITCH, D. H.	President, The Fitch Battery and Electric Co., Cazenovia, N. Y.	Sept. 3, 1889
FLACK, J. DAY	Ass't Electrician in charge of Tests, Edison Lamp Co., Harrison, N. J.	Dec. 6, 1887
FLEMING, WALTER M. *M. D.*	Vice-President of the Averell Insulating Conduit Co., 45 Broadway, Room 1, New York.	Jan. 3, 1888
FORD, ELLSWORTH	Superintendent Edison Electric Light Co., Ottumwa, Ia.	July 12, 1887
FULLER, LEVI K.	Vice-President, Estey Organ Co., Brattleboro, Vt.	Mar. 5, 1889
GILES, WALTER A.	Electrical Engineer, Westinghouse Electric Co., 120 Broadway, New York.	Nov. 1, 1887
GILLILAND, E. T.	Vice-Pres't Empire City Elec. Co., 15 Dey St. Residence, 179 West End Ave., New York.	April 15, 1884
GOLDMARK, CHAS. J.	963 Park Ave., New York.	June 5, 1888
GOULD, CHAS. W.	2 Wall St., New York.	May 24, 1887
GRISCOM, WM. W.	President, The Electro Dynamic Co., and Managing Electrician, The Electrical Accumulator Co., New York. Residence, Haverford College P. O., Pa.	June 5, 1888
GUTMANN, LUDWIG	Electrical Engineer, P. O. Box 374, Fort Wayne, Ind.	Sept. 14, 1888
HALL, GEO C. T.	Electrical Expert, Care of Thomson-Houston Electric Co., 148 Michigan Ave., Chicago, Ill.	July 12, 1887
HALL, HENRY D.	Promoter (or Exploiting of Electrical Enterprises), 15 Dey St., New York.	June 8, 1887
HALL, EDWIN H,	Assistant Professor of Physics, Harvard College, 5 Avon St., Cambridge, Mass.	Sept. 3, 1889
HARDING, H. McL.	General Agent, Sprague Electric Railway and Motor Co., 16 and 18 Broad St., New York.	May 24, 1887
HARVEY, WIRT B.	43½ Madison St., Memphis, Tenn.	Dec. 6, 1887
HATZEL, J. C.	Electrical Engineer and Contractor, 29 West Twenty-sixth St., New York.	Sept. 3, 1889
HEALY, CLARENCE L.	Sup't and Electrician, Commercial Telegram Co., 18 Broadway, New York.	April 15, 1884
HEBARD, GEORGE W	Pres't, United States Electric Light Co., 120 Broadway, New York.	April 15, 1884
HENDERSON, ALBERT H.	Ries and Henderson, 45 Chamber of Commerce, Baltimore, Md.	July 12, 1887
HENSHAW, FREDERICK V.	Assistant Electrician, The "C. & C." Electric Motor Co., 402 Greenwich St., New York.	Feb. 5, 1889

Name.	Address.	Date of Membership.
Hochhausen, William	Electrician, The Excelsior Electric Co., 196 Willoughby St., Brooklyn, N. Y.	April 15, 1884
Hopkins, J. Herbert	Superintendent, Central Electric Construction Company's Branch Office, 212 N. High St., Nashville, Tenn.	April 2, 1889
Howell, Wilson S.	Electrical Expert, Central Station, Edison Elec. Light System, New Brunswick, N. J.	Sept. 3, 1889
Howson, Hubert	Patent Lawyer, 38 Park Row, New York.	June 8, 1887
Hubrecht, Dr. H. F. R.	Director, Nederlandsche Bell Telephone Co., Amsterdam, Holland.	Oct. 4, 1887
Humphreys, C. J. R.	Manager, Lawrence Gas Co., Lawrence, Mass.	Sept, 6, 1887
Idell, Frank E.	Mechanical Engineer, 41 Dey St., New York.	July 12, 1887
Ihlder, John D.	Electrical Engineer, with Asterheld & Eickmeyer, Manuf'rs of Dynamos and Motors, Yonkers, N. Y.	Oct. 2, 1888
Insull, Samuel	Electrical Manufacturer, 44 Wall St., New York.	Dec. 7, 1886
Iselin, Henry S.	President, Gibson Electric Co., 74 Cortlandt St., New York.	June 2, 1885
Ives, Edward B.	Lieutenant U. S. A., David's Island, New York.	April 2, 1889
Izard, E. M.	Electrical Engineer, Secretary and Treas'r of the Leonard & Izard Co., 425, The Rookery, Chicago, Ill.	Mar. 5, 1889
Jackson, C. H.	Vice-Pres't, Standard Underground Cable Co., Allegheny, Pa.	May 24, 1887
Jackson, Dugald C.	Vice Pres't, The Western Engineering Co., Kearney, Neb.	May 3, 1887
Jackson, Francis E.	With Edison Lamp Co., Harrison, N. J.	Jan. 3, 1888
Jaeger, H. J.	Chemical, Physical and Electrical Glass Blowing, 173 Pearl S,., Brooklyn, N. Y.	July 12, 1887
Johnston, W. J.	President, The W. J. Johnston Co., Ltd., Times Building, New York.	April 15, 1884
Judson, Wm. Pierson,	U. S. Civil Engineer, Oswego, N. Y.	June 8, 1887
Kennelly, A. E.	Electrician, Edison Laboratory, Orange, N. J.	May 1, 1888
Kerr, Thomas B.	Attorney-at-Law, 32 Nassau St., New York.	Nov. 1, 1887
Kimball, A. S.	Professor of Physics, Worcester Polytechnic Institute, Worcester, Mass.	Sept. 3, 1889
Knapp, Allan C.	Electrician, 225 Dearborn St., Chicago, Ill.	Sept. 6, 1887
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
Law, M. D.	General Sup't, United States and Phila. Electric Light Co., Twentieth and Johnston Sts., Philadelphia, Pa.	Feb. 7, 1888

Name.	Address.	Date of Membership.
Ledoux, A. R.	Chemical Expert, 10 Cedar St., New York.	Dec. 7, 1886
Leland, H. W.	Manager, Telephone Exchange, 259 Washington St, Jersey City N. J.	April 15, 1884
Lemp, Hermann, Jr.	Electrician, Thomson Electric Welding Co., Lynn, Mass.	April 2, 1889
Lewis, Henry Frederick William.	General Manager and Secretary of the Western Counties South Wales Telephone Co., England, 16 High St, Bristol, Eng.	Mar. 5, 1889
Liebig, Gustav A., Jr.	Elec'l Testing Bureau, Johns Hopkins University, Baltimore, Md.	Mar. 6, 1888
Lowry, Grosvenor P.	Lawyer, 15 Broad St., Residence 121 Madison Ave., New York.	Nov 1, 1887
Maclilie, C. H.	Broker in Electrical Securities, temporary address, 125 Temple Court, New York.	July 12, 1887
Madden, O. E.	President, Empire City Electric Co., 15 Dey St., New York.	April 15, 1884
Magie, Louis J.	Electrical Engineer, in charge of European Office of Thomson-Houston International Electric Co., Michaelisbrücke 1, Hamburg. Gy.	April 2, 1889
Mailloux, C. O.	Consulting Electrical Engineer, 32 Liberty St., New York.	April 15, 1884
Mansfield, Geo. W.	Electrical Engineer, with Thomson-Houston Electric Co, 620 Atlantic Ave., Boston, Mass.	June 2, 1885
Martin, T. Commerford	(*Past President* and *Vice-President.*) Editor, *The Electrical World*, Times Building. New York.	April 15, 1884
Maver, William, Jr.	(*Manager.*) Electrical Expert, 31 Nassau St., New York.	July 12, 1887
McIntire, Chas.	Manufacturer of McIntire's Patent Electric Wire Connectors. &c., 36 Crawford St., Newark, N. J.	July 12, 1887
McKibbin, George N.	Chemist and Electrician, 46 West Fifty-first St., New York.	June 8, 1887
McInstry, J. P.	185 Seneca St., Cleveland, O.	April 15, 1884
McMahan, John M.	Bergen Point, N. J.	Aug. 6, 1889
Mercer, Andrew G.	Treasurer and Electrician, Waterloo Electric Co., Waterloo, N. Y.	Sept. 3, 1889
Miller, Joseph A.	Civil and Consulting Engineer, Providence. R. I.	Dec. 9, 1884
Miner, W. M.	Electrician and Inventor, 89 East Second St., Plainfield, N. J.	July 12, 1887
Mitchell, John Murray	Lawyer, 45 Wall St., New York.	June 2, 1885
Mix, Edgar W.	Electrician, with Thomson-Houston Electric Co., Lynn, Mass.	Sept 3, 1889
Morrison, J. Frank	15 South St., Baltimore, Md.	April 15, 1884
Morton, Henry, *Ph. D*	President of Stevens Institute of Technology, Hoboken, N. J.	May 24, 1887
Moses, Dr. Otto A.	Electrician, 131 East Seventy-third St., New York.	May 17, 1887
Mosscrop, Wm. A.	26 Court St., Brooklyn, N. Y.	May 7, 1889

Name.	Address.	Date of Membership.
Nicholls, F. Victor	Electrician and Manager Vancouver Electric Illuminating Co., Vancouver, B. C.	Dec. 4, 1888
Nunn, Richard J., *M. D.*	Physician, 119 York St., Savannah Ga.	July 12, 1887
Oatis, John X.	Manager and Electrician, Amsterdam Electric Light & Power Co., Amsterdam, N. Y.	Sept. 7, 1888
Ockerhausen, H. A.	Electrical Engineer, Brooklyn & N. Y. Ferry Co., Brooklyn, E. D., N. Y.	Sept. 6, 1887
O'Dea, M.	Electrician, University of Notre Dame, Notre Dame, Ind.	June 8, 1887
Parshall, H. F.	Electrician, Sprague Electric Railway & Motor Co., 16 Broad St., New York.	Sept. 7, 1888
Paul, Chas. M.	Electrician, 172 Remsen St., Brooklyn, N. Y.	May 7, 1889
Peck, Samuel C.	Electrician, Thomson-Houston Electric Co., 620 Atlantic Ave, Boston Mass.	Sept. 6, 1887
Peirce, Wm. H.	Superintendent Marine Installations, United Edison M'f'g Co., 65 Fifth Ave., New York.	Sept. 7, 1888
Phelps, Geo. M.	(*Treasurer*). Electrical Engineer and Editor, 11 Wall St., New York.	April 15, 1884
Phillips, Eugene F.	M'f'r Insulated Electric Wire. Providence, R. I.	July 13, 1886
Plush, Dr. S. M.	Electrician, 319 South Tenth St., Philadelphia, Pa.	April 15, 1884
Poole, Cecil P.	Contracting Electrical Engineer, 206–8 Eighth St. Lynchburg, Va.	Jan. 3, 1888
Pope, Ralph W.	Secretary of the American Institute of Electrical Engineers, 5 Beekman St., New York.	June 2, 1885
Pratt, Herbert G.	Treasurer Samson Cordage Works, 164 High St., Boston, Mass.	Oct. 4, 1887
Rae, Frank B.	Electrical Engineer, Detroit Electrical Works, Detroit, Mich.	April 15, 1884
Randall, John E.	Incandescent Lamp Dep't, Thomson-Houston Electric Co., Lynn, Mass.	May 7, 1889
Reed, Chas. J.	Electrician, Care of American Institute of Electrical Engineers, 5 Beekman St., New York.	Mar. 5, 1889
Reed, Henry A.	Secretary and Manager, Bishop Gutta-Percha Co., 422 East Twenty-fifth St., New York.	June 4, 1889
Reilly, John C.	General Sup't, N. Y. & N. J. Tel Co, 16 Smith St, Brooklyn, N. Y.	April 15, 1884
Reinmann, A. L.	Electrician, Breckenridge Ave., Pittsburgh, Pa.	June 8, 1887
Riker, Andrew L.	Electrical Engineer. 15 East Fifty-fifth St., New York.	Nov. 1, 1887
Robinson, Almon,	Draughtsman, Expert in Methods of Gearing, P. O. Box, 943, Lewiston, Me.	Sept. 6, 1887
Roebling, Ferdinand W.	Manufacturer of Electrical Wires and Cables, Trenton, N. J.	June 8, 1887

Name.	Address.	Date of Membership.
Rogers, Winfield S.	Mechanical Engineer, Watervliet Arsenal, West Troy, N. Y.	Sept. 3, 1889
Roome, H. C.	32 Liberty St., New York.	April 15, 1889
Rosenbaum, Wm. A.	Electrical and Patent Solicitor, Care of *The Electrical World*, Times Building, New York.	Jan. 3, 1889
Royce, Fred W.	Electrician and Patent Solicitor, 1408 Pennsylvania Ave., Washington D. C.	April 15, 1884
Ryan, Harris J.	Professor of Electrical Engineering, Cornell University, Ithaca, N. Y.	Oct. 4, 1887
Sargent, W. D.	General Manager, N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
Sawyer, Frederick J.	Pres't, The New England Electric Co., 55 Oliver St., Boston, Mass.	June 8, 1887
Saxelby, Frederick,	Electrical Engineer, Superintendent of Exhausting Department, Edison Lamp Co., Harrison, N. J.	June 5, 1888.
Schmidt, Frederick,	Managing Director, The Schmidt-Douglas Electric Co., L'td., 31 Blenheim Rd, Bradford, England.	Jan. 3, 1888
Searing, Lewis	212 West One hundred and thirtieth St., New York.	April 3, 1888
Seely, J. A.	Electrician, Metropolitan Telephone & Telegraph Co., 18 Cortlandt St, New York,	April 15, 1884
Serrell, Lemuel Wm.	Mechanical Engineer and Managing Director. Daft Electric Light Co., Jersey City, N. J.	Nov. 1, 1887
Sheehan, William M.	Manager and Electrician, Newburgh Electric Light & Power Co., 65 Montgomery St., Newburgh, N.Y.	Sept. 3, 1889
Sise, Charles F.	Vice-President and Managing Director, Bell Telephone Co., of Canada, and Canadian Telephone Co, Ltd., Montreal, Canada.	June 8, 1887
Smith, Gerritt	Circuit Electrician, 195 Broadway, New York.	April 15, 1884
Smith, J. Elliot	Superintendent Fire Alarm Telegraph, 157 East Sixty-seventh St., New York.	April 15, 1880
Smith, Jesse M.	Consulting Electrical Engineer and Expert in Patent Causes, 36 Moffatt Block, Detroit, Mich.	April 15, 1884
Souza, Carlos Montiero e	Rua Castilleo 3, Lisbon, Portugal.	Sept. 6, 1887
Sprague, Frank J.	Electrician, and Vice-President, Sprague Electric Railway & Motor Co., 16 Broad St., New York.	May 24, 1887
Stanley, William, Jr.	Electrician, Great Barrington, Mass.	Dec. 6, 1887
Stearns, Charles K.	With Thomson-Houston Electric Co., Lynn, Mass.	Aug. 6, 1889
Stebbins, Theodore	Electric Railway Inspector, Thomson-Houston Elec. Co, Lynn, Mass.	July 9, 1889
Stockbridge, Geo. H.	Patent Attorney, 132 Nassau St., New York.	May 24, 1887
Stockly, Geo. W.	President, Brush Electric Co., Cleveland, O.	April 15, 1884

Name.	Address.	Date of Membership.
Stuart, Otis K.	Clerk, Thomson Electric Welding Co., Lynn, Mass.	Jan. 4, 1889
Taylor, Charles	Metallurgist, U. S. Assay Office, 30 Wall St., New York.	Nov. 1, 1887
Temple, William Chase	Electrical Engineer, 575 Madison Ave., New York.	May 3, 1887
Tesla, Nikola	Electrical Engineer and Inventor, Westinghouse Electric Co., Pittsburgh, Pa.	June 5, 1888
Thompson, Edward P.	Patent Attorney, 5 Beekman St., New York.	April 15, 1884
Thompson, William Geo. MacNeill,	Resident Engineer, Sault Ste. Marie Canal, Sault Ste. Marie, Ontario.	July 12, 1887
Thomson, Prof. Elihu	(*President*). Electrician, Thomson Houston Electric Co., and Thomson Electric Welding Co., Lynn, Mass.	April 15, 1884
Townsend, Henry C.	(*Manager*). Attorney and Expert in Electrical Cases, 5 Beekman St., New York.	July 10, 1888
Tregoning, John	Sup't, Thomson Electric Welding Co., 325 Boston St., Lynn, Mass.	April 2, 1889
Uhlenhaut, Fritz, Jr.	Electrician, with Edison Electric Illuminating Co. of Brooklyn, 26 Court St., Brooklyn, N. Y.	May 7, 1889
Upton, Francis R.	(*Vice-President*). Treasurer and General Manager, Edison Lamp Co,, Harrison, N. J.	May 17, 1887
Van Brunt, Walter	Manager, Duluth Telephone Co., Duluth, Minn.	Sept. 6, 1887
Vance, A. St. Clair	425, The Rookery, Chicago, Ill.	April 2, 1889
Van Hoevenbergh, Henry	(*Manager*). Electrical Engineer and Inventor, 108 Liberty St., New York.	June 5, 1888
Van Trump, C. Reginald	Under-Graduate in Electrical Engineering, Cornell University, Ithaca, N. Y.	Feb. 5, 1889
Van Valkenburgh, F. S.	Central and South American Tel. Co., Tehuantepec, Mexico.	June 5, 1886
Van Vleck, Frank	Chief Engineer, San Diego Cable Tramway Co., San Diego, Cal.	Nov. 16, 1886
Viduarre, Jose	Dealer in Electrical Supplies, Ponce, Porto Rico.	Sept. 6, 1887
Wacker, George G.	Electric Organs, 3644 Third Ave., New York.	Sept. 6, 1887
Wallace, William	Wire Manufacturer, Ansonia, Conn.	April 15, 1884
Walter, Henry E.	Schenectady, N. Y.	April 2, 1889
Waring, Richard S.	205 Penn Building, Pittsburgh, Pa.	April 15, 1884
Waterhouse, Frank G.	Ass't General Sup't, Westinghouse Electric Co., Pittsburgh, Pa.	Sept. 6, 1887
Weil, Leopold	Importer, 121 Mercer St., New York.	June 8, 1887
Welles, Francis R,	Manufacturer, Bell Telephone Manufacturing Co., Antwerp, Belgium	Sept. 6, 1887
White, H. C.	Manager, Phœnix Iron Works Co., 16 Dey St., New York.	April 15, 1884

Name.	Address.	Date of Membership.
WHITE, J. G.	Manager, The Western Engineering Co., Kearney, Neb.	April 2, 1889
WHITNEY, HENRY M.	President, West End Street Railway Co., 81 Milk St., Boston, Mass.	July 12, 1887
WIGHTMAN, MERLE J.	Electrical Engineer and Inventor, Thomson-Houston Electric Co., Lynn, Mass.	Mar. 5, 1889
WILEY, WM. H.	Scientific Expert, 15 Astor Place, New York.	Feb. 7, 1888
WILLIAMS, CHARLES, JR.	Electrician, 100 Sudbury St., Boston, Mass.	April 15, 1884
WILLIAMS, JAMES B. *M. D.*	Physician, 1068 Eighteenth St.. Oakland, Cal.	Sept. 7, 1888
WINKLER, CHARLES F.	Electrician. Troy Electric Dynamo Co., 4 Park Ave., Troy, N. Y.	Sept. 3, 1889
WINTRINGHAM, J. P.	Theorist, 36 Pine St., New York.	May 7, 1889
WOLCOTT, TOWNSEND	Consulting Electrician, 828 Monroe St., Brooklyn, N. Y.	Mar. 6, 1888
WOOD, E. J.	Consulting Engineer and Contractor, 243 Broadway, New York.	July 12, 1887
WOODRUFF, H. O.	Agent. Sprague Electric Railway & Motor Co., Des Moines, Iowa.	Oct. 2, 1888
WORTHINGTON, GEORGE	Manager, *Electrical Review*, 13 Park Row, New York	April 15, 1884
YOUNG, ALDEN M,	Contractor for Organization and complete Equipment of Electric Lighting Plants, 101 Bank St., Waterbury, Conn.	Sept. 6, 1887
YOUNG, C. GRIFFITH	Superintendent and Electrician, Mount Morris Electric Co., 56 Broad St., Residence, 262 West One hundred and twenty-third St., New York.	Jan. 3, 1889
ZALINSKI, CAPT. E. L.	U. S. A., Fort Hamilton, N. Y.	May 17, 1887

Associate Members, - - - - 224.

RECAPITULATION.

Honorary Members, - - - - - - - - -	1
Members, - - - - - - - - - - - -	125
Associate Members. - - - - - - - - -	224
Total - - - -	550

Any errors in this list, or changes in address should be reported to the Secretary.