Discussion on "An Imperfection in the Usual Statements of the Fundamental Law of Electromagnetic Induction." Atlantic City, N. J., July 2, 1908

Chas. P. Steinmetz (by letter): Mr. Hering's paper is interesting in that it draws attention to a looseness in the form of expressing this law, which is frequently the cause of serious misunderstanding, and the waste of much energy and time. For instance, in the attempts to invent a coil-wound unipolar machine much useless effort could have been avoided by a clearer distinction between the general law and the special case of its application to a continuous closed conductor. While Mr. Hering's experiment is interesting in showing an instance of a closed electric circuit in which the number of interlinkages with the lines of force changes without inducing an electromotive force, it is not startling to me, as the reverse case, the electromagnetic induction of an electromotive force, in a closed circuit, without any change of the number of interlinkages of the circuit with the magnetic flux, is illustrated by practically every unipolar machine. There are thousands of kilowatts of such machines now in commercial operation.

The general law of electromagnetic induction is:

> In a conductor moving relatively to a magnetic field, an electromotive force is induced which is proportional—and in absolute units equal—to the product of the intensity of the magnetic field, and the components of the length, and of the velocity of the conductor at right angles to the magnetic field and to each other.
>
> If an electric conductor moves relatively to a magnetic field, an electromotive force is induced in the conductor, which is proportional to the intensity of the magnetic field, to the length of the conductor, and to the speed of its motion perpendicular to the magnetic field and the direction of the conductor.

Using the pictorial representation of the magnetic field by the lines of magnetic force, as given by Faraday, this means, that the electromotive force induced in the conductor equals the rate of cutting of the conductor through the lines of magnetic force, that is, gives: $e = \frac{d\,\phi}{d\,t}\,10^{-8}$ volts, where $d\,\phi$ are the lines of force cut by the conductor during the time $d\,t$.

Applying this general induction law to the special case—which is the most important, but not the only case met in electrical engineering—of a continuous conductor closed upon itself, or a turn, it follows, as conclusion, that the total electromotive force or resultant electromotive force induced in the turn equals the rate of change of the total number of magnetic interlinkages of lines of magnetic force enclosed by the turn, hence is: $e = \frac{d\,\phi}{d\,t}10^{-8}$, where ϕ is the number of lines of magnetic force enclosed by the turn or, leaving Faraday's pictorial representation, ϕ is the magnetic flux enclosed by the turn.

Maxwell and J. J. Thomson's statement, as quoted by Mr. Hering, are not the most general expressions of the law of induction, but its formulation for the special case discussed by these scientists, of a turn moving with regard to the magnetic field.

Mathematically speaking, we may see that Maxwell's law is the integral expression derived from the general or differential law by integration over the whole circuit, under the " terminal " or " limit " conditions of continuity of conductor and continuity of motion, and does not apply to Mr. Hering's experiment, or to the general design of unipolar machines, which do not fulfil the conditions of continuity of motion, but have parts of the conductor sliding over other parts.

Faraday's expression, of cutting of lines of magnetic force by the conductor, is the general law; but in its application to unusual cases it must be kept in mind that the " line of magnetic force " is merely a pictorial representation of the magnetic field in space, as characterized by the two constants, intensity, and direction. This pictorial representation, when carried so far as to apply to its physical existence, may lead to wrong conclusions; for instance, when discussing whether the lines of magnetic force of a revolving magnet move with the magnet or stand still. Assuming, for instance, a bar magnet of circular section $x^2+y^2=r^2$, revolving around its axis z. Then in any point in space, outside of the magnet as well as inside, the intensity as well as the direction of the magnetic field is constant; that is, the magnetic field is constant, or stationary in space, regardless of whether the magnet stands still or revolves. Assuming a second system of coördinates with the same axis, z, as the magnet, and with the other two axes, x_1, and y_1, stationary with regard to the magnet, and revolving in space with the revolutions of the magnet, then with this coördinate system, x_1, y_1, z, the magnetic field at any point, inside of the magnet as well as outside, is also constant in intensity and in direction; that is, is stationary. Or in other words, while the two coördinate systems x, y, z, and x_1, y , z_1, revolve with regard to each other, the magnetic field of the magnet is constant in intensity and in direction, that is, it is stationary, with regard to either. Physically, this is nothing exceptional; it merely means that the condition of stress, which we call magnetic field, is unvarying in its distribution in space as well as with regard to the revolving magnet. Picturing to ourselves the magnetic field as lines of magnetic force, it would mean that the lines of force are at the same time stationary in space and also revolving with the magnet. This suggests that all pictorial representations, no matter how useful, may occasionally become ambiguous. In such cases the only safe way is to go back to the entities proper, in the present case, the magnetic field as a quantity characterized by intensity and direction.

Unfortunately, in teaching, instead of the general law of induction, there is frequently given to the student, its specific application to the turn or closed continuous conductor as more convenient to illustrate and to understand. While in the introduction to the elements to electrical engineering this is permissible, to get a complete understanding of the phenomena of induction, it must be supplemented by an exact discussion of the general induction law, that is, the mathematical formulation of Faraday's pictorial representation.

A. E. Kennelly (by letter): The experiment described in the paper is both interesting and instructive. Although the experiment illustrates the proper application of the law of induction when applied to electric circuits, it does not in my opinion controvert the existing law when properly interpreted; that is, when interpreted as intended to be expressed by its founders, Faraday, Maxwell, and others.

When it is stated that the electromotive force round a circuit is equal to the time-rate of change of the flux linked with that circuit, it is inherently assumed that the circuit is not interrupted and then established around a new boundary. It means, as I understand it, that the circuit contains a simply connected region of magnetic flux, through which the boundaries may be flexibly caused to wander at will. Maxwell specifically rules out the case of multiple-connected regions, by a special proposition to that effect. In Mr. Hering's experiment, the boundary of the circuit is cut at one point, and simultaneously a second circuit embracing flux is introduced at the gap, in such a manner that by sliding along the boundaries of the second circuit, the magnetic flux may be caused to disappear from the embrace of the first circuit without any intersection of flux by the edge of that circuit. This, to my mind, is juggling with terms, just as though the circuit were cut and then reclosed through a quiescent loop linked with a magnet. Manifestly, no current would be induced through the galvanometer by reason of that change, although in one sense of the word "circuit", all of the flux in the loop has suddenly been introduced into the circuit.

The instructive value of the experiment described in the paper lies, to my mind, in clearing up the question as to which is the primordial proposition; that electromotive force is induced (1) by the movement of magnetic flux across the boundary, or (2) by the introduction of flux into a loop or circuit. As ordinarily stated, the two propositions are as closely connected as the propositions concerning the priority of the chicken and the egg, because flux cannot cut the boundary of a loop at any point without altering the flux contents of the loop, nor can the flux in a loop alter without cutting the boundary somewhere. The experiment shows, however, that where flux comes into a field from a balloon, as distinct from walking over a fence, no electromotive force is induced, and this indicates hat the cutting is the primordial conception, to which enclosing is secondary. Of course this cutting electromotive force occurs as much in insulators as conductors, but can ordinarily be revealed only through the use of conductors. When, therefore, a closed circular solenoid, or anchor-ring, is wound with a primary and with a secondary coil, we know that in the steady state, if the solenoid is properly wound, there will be no external magnetic flux due to current in either winding; but the change of internal flux due to change of current in one winding induces electromotive force in the other, as in the ordinary transformer. At first sight, this would look as though there were change of enclosing flux without any cutting, but in the light of this experiment it seems clear that this is not the case. There must be cutting of flux passing from outside to inside of the secondary coil, but, incidentally, all the external flux cancels off, or becomes zero in the final steady state.

Summing up, then, I should think that this experiment shows that when the ordinary proposition is enunciated concerning induction of electromotive force with change of flux enclosed in a circuit, it should be borne in mind that the circuit is not to be juggled with by interrupting it and changing it from a simply-connected to a multiple-connected space. It must consist of a single, continuous boundary which, if it moves, moves continuously through simply connected space. In this understanding, I think we shall all agree.

Elihu Thomson (by letter): I agree with Dr. Kennelly in his view of the state of the case. By passing the spring clip over the magnet leg the circuit is virtually opened so far as magnetic induction is concerned, and an immovable section of conductor is substituted in the gap where all the flux to be cut then exists. The conclusion that the real physical substance of the circuit (the matter of the circuit) must cut or be cut by the lines is in accordance with my view of the subject. There are innumerable phenomena which have confirmed that idea of what is really the circuit to be considered. Otherwise in fact, the circuit is more metaphysical than physical, a sort of mental image only. I am glad Mr. Hering has tried the experiment, as it will tend to clear up matters which have troubled students in unipolar induction. I have long regarded Faraday's view of *line cutting* as much preferable and more universal than the theory of linkages simply.

The unipolar dynamo is quite practicable for large units of 220 to 550 volts or more, understanding that greater collector losses offset the commutation difficulties with ordinary types and that magnetic losses will probably exceed the calculated losses considerably.

There seems to be one universal law which might be expressed as follows: "It is not possible without chemical or thermoelectric action to generate a continuous current in a closed circuit without sliding contacts," or "A magnetic induction machine for direct currents must have a commutator or sliding contacts of some sort."

The experiment might be modified by boring holes through the magnet and inserting copper pins, the ends of which would be traversed by the ends of the spring clip. Manifestly since there would be no movement of these pins, there could be no electromotive force generated when they were traversed.

In a magnetic blowout arrester the static stress is exerted at full intensity across the narrower part of the gap in spite of the powerful field surrounding it. As soon, however, as the first slight spark jumps the gap (at the narrower part of course) the gases traversed by the current are deflected. Not, however, until the matter carrying the current has actually bridged the gap does the deflecting action of the field begin.

If the current be assumed to be a flow of electrons (negative) from one molecule to the next contiguous and so on, it is easy to understand why the *matter* of the circuit is the thing concerned and not merely an assumed line or direction.

It may be proper to regard the experiment as involving a form of unipolar induction, inasmuch as it is not possible to use a coil of numerous turns but one of a single turn. If the coil were to be used, it would have to be made up of loops which could open and pass over the magnet by riding upon a series of pins, insulated from each other, projecting through the magnet section, as suggested by me in a modification. Some twenty-five or thirty years ago I leaned to the more generalized view of Maxwell's law referred to in the paper, but gradually grew out of it and adopted Faraday's view of line cutting as the essential thing. The lines must be cut by the moving conductor, or the lines must move and cut the conductor in order to generate an electromotive force, and the potential difference arises in only that section of the conductor which cuts or is cut by the lines.

I think I should have to modify the universal law, which I proposed above, a little in view of Dr. Bruger's direct-current machine. Inasmuch as the resistance of a coil of bismuth wire may be changed by a magnetic field, it is evident that if the magnetism is so used as to increase the resistance of the bismuth when a current in one direction would be induced in it or a part of the circuit in series with it, and if, when the opposite pulse was induced, the resistance of the bismuth were lower, this would amount to a partial commutation but would be accomplished without slipping contacts or any ordinary form of commutator. The result, however, would seem to be more of a pulsating current or a current in which the values of the half waves were unequal, or the wave unsymmetrical with respect to the zero line; a partially rectified current, so to speak. The mercury arc rectifier accomplishes the production of an alternating current from a direct current, and other instances exist, as is well known, of partial rectification by electrolytic cells, vacuum tubes, and the like. It is conceivable that it might be possible to use even a selenium cell in place of the bismuth of Dr. Bruger, putting the selenium in the light at one time and in the dark at another, to affect synchronous alternating pulses differently and give a balance of direct current. A partially rectified current can be sifted so that the direct-current component goes one way, and the alternating-current component another way, as by connecting the terminals by a non-inductive resistance and connecting the direct-current receiving apparatus from the same terminals through a high inductance.

In my paper many years ago, on "Magnetism in its Relation to Induced Electromotive Force and Current" read before the American Institute of Electrical Engineers, I rather emphasized the view that a line of force can only finish by collapsing to a point or infinitesimal closed chain, and can never be broken or opened. By this view, to my mind, we have the only possible explanation for the varied phenomena of induction; that is, we have the only possibility of getting any good physical conception of what takes place.

W. S. Franklin (by letter): What is said in this paper concerning the law of induced electromotive force is, I think, entirely correct. I must say, however, that I have never looked upon Maxwell's statement of this law in a way which would lead me to think of it as not strictly correct. It has always seemed to me that the differential equations of the electromagnetic field in *stationary* and in *moving* media cover the ground completely without any possibility of a misunderstanding.

The point of view of looking upon the circuit as abstracted from definite identifiable material may be illustrated in its extreme form as follows: imagine a sheet of copper placed at right angles to a steady magnetic field; imagine an electric circuit in the form of a circle which starts at a point on the copper sheet and spreads outward like a circular ripple on the surface of a pond. This moving circuit, according to Mr. Hering's interpretation of Maxwell's generalization of Faraday's law, should have electromotive force induced in it, and therefore the stationary sheet of copper in a steady magnetic field should have eddy currents circulating in it. Of course I understand it is precisely this absurd conclusion that is being objected to. My point is this: I do not believe that those who accept Maxwell's generalization have forgotten the idea of *actual cutting of lines of force* by the material of the electric circuit; but it may be said they are likely to be led to forget it because of the form of Maxwell's statement of the generalized Faraday law. Perhaps that is true, and yet the difficulty reduced to its simplest terms is the difficulty of partial differentiation; a physical condition is a function of several independent variables, such, for example, as space and time. One is *obliged* to think of one variable only changing at a time, but one should never for this reason surrender the *knowledge* that all things change together.

Percy H. Thomas (by letter): If I understand this paper correctly, the experiment is not conclusive, and for the following reasons: in drawing off the loop, as long as the ends are rubbing on the sides of the magnet, the lines of force may be said to be still within the loop. When next those ends come in contact, preliminary to their separation from the magnet, the galvanometer becomes short-circuited and protected from any influence of a later change of lines in the loop. In other words, the original loop becomes two loops by being connected across the middle. None of the lines of force is evidently in the lower loop; all are in the upper loop. The upper loop is then *broken* by withdrawing the wires entirely from the magnet, which of course will give no deflection. What has really been done, if I understand the experiment, is that a portion of the original loop containing the galvanometer (but none of the lines) has been *cut off* by short-circuiting, from the whole loop while it still contains the lines, and then the main loop has been opened.

W. P. Graham (by letter): It is perhaps open to question whether the statements of the law of electromagnetic induction by Maxwell and by J. J. Thomson, as quoted by Mr. Hering, are sufficiently precise. Their lack of precision may be simply illustrated as follows: let a wire be bent round the pole of a magnet so as to form a closed circuit linking the magnetic flux. Now let the ends of the wire be separated and the magnet slipped out between the ends so that the flux ceases to link with the wire. Finally, let the wire be again formed into a closed circuit with which the flux does not link. If this re-established circuit be regarded as the original circuit, the flux through the circuit has certainly changed without inducing any electromotive force in the wire.

I think most of us would regard such an interpretation of Maxwell or Thomson as a quibble. But essentially the same interpretation is made by Mr. Hering in discussing the experiment with the galvanometer and copper strips. As soon as the copper strips strike the iron, the original circuit, made up of copper strips and galvanometer is opened, but a new circuit including copper strips, magnet iron and galvanometer is established. And as long as the copper strips make contact with the magnet, there is no change of flux through this new circuit. When the strips leave the magnet, this new circuit is broken, and the old circuit from which the flux has been removed after opening it, is re-established. A more accurate statement would perhaps be that as soon as the strips strike the iron, the old circuit is opened and a new circuit established, to be followed by an infinite succession of other new circuits as the strips slip over the iron, and that the flux is removed from each one of this succession of circuits, by opening each in turn, and establishing its successors; but that there is no change of flux through any individual circuit of the series so long as that circuit is closed.

I agree entirely with Mr. Hering that in presenting the law of induction to a class of students, the cutting of the flux by the conductor is the point to be emphasized and that Faraday's statement of the law is to be preferred.

George T. Hanchett (by letter): It would appear from this that some modification of Maxwell's version of the laws of induction should be made. Let us consider two other methods of trying this experiment. The first of these is illustrated in Fig. 1. The test circuit is slipped over the magnet in the usual manner and the initial deflection is observed. It is then unlinked (?) with the magnetic circuit by the following device: The wires are short-circuited at *A* and the clips at *B* drawn over the magnet limb and removed. There will be absolutely no deflection at the galvanometer as a result of this action, provided, of course, that the short-circuiting is perfect.

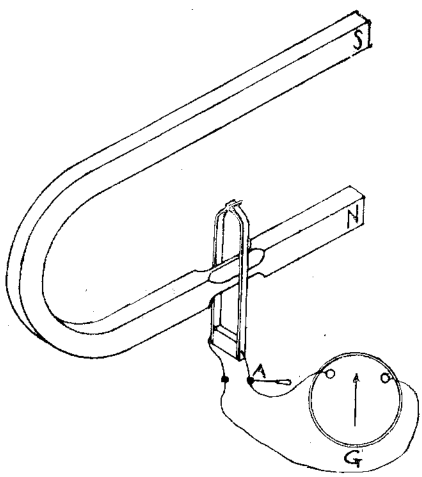

Fig. 1

The second experiment, illustrated in Fig. 2, provides two clips similar to those mentioned by Mr. Hering, but instead of drawing them over the body of the magnet, they are drawn over a tube or annulus surrounding the limb of the magnet and insulated therefrom. It is observed that the detector circuit is first short-circuited outside the flux field before any attempt is made to unlink it, and by this means the circuit is divided into two portions, one containing the galvanometer or detector, which is short-circuited and removed, and the other containing the flux, which is short-circuited and not removed from the magnetic circuit at all. No lines of force are cut by or varied in number within a circuit, and in strict accordance with Maxwell and Faraday no deflection results. In Fig. 2 I have left the loop or ring surrounding the magnet limb in order to point out clearly what Mr. Hering has evidently failed to see—that in performing his experiment he has left an exactly similar flux containing a portion of the circuit behind him in the form of a loop of steel which is a part of the magnet limb itself.

There is no unlinking of the magnetic and electric circuits, but instead a mechanical unlinking which deceives the eye.

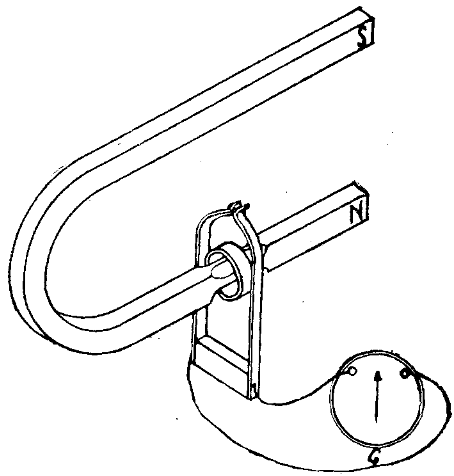

FIG. 2

Electrically, the circuit is linked with the magnet, then a new portion of circuit is added in the form of the magnet limb itself which is ingeniously caused to do all the linking. Next the original circuit now free from flux is removed. This is the exact analogue of the experiment which is the subject of this discussion.

George A. Campbell (by letter): The very simplicity of the facts and principles which Maxwell stated is, apparently, the reason for their being overlooked in the present discussion. The points requiring restatement seem to be the following:

1. Electric currents and magnetic lines of force are both closed. This statement is true irrespective of whether the electric conductors and magnetic cores are open or closed. The current is determined by the integral electromotive force around the circuit, and thus any experimental measurement of the current gives us the total, and not the localized electromotive force. An exact statement of fact must then relate only to the integral values for the entire closed circuit; accordingly, Maxwell's statement (Vol. II, paragraph 541) is in terms of these integral qualities:

"The total electromotive force acting round a circuit at any instant is measured by the rate of decrease of the number of lines of magnetic force which pass through it."

This is the fundamental statement of fact, as distinct from theory, in electromagnetic induction.

2. As the current and the lines of force are invariably closed, it follows as a mathematical necessity that the rate of cutting of lines of force by the circuit is equal to the rate of change in the number of lines of force through the circuit. Maxwell expresses this (Vol. II, paragraph 541) as follows:

"If, therefore, the number of lines which pass through a conducting circuit is made to vary, it can only be by the circuit moving across the lines of force, or else by the lines of force moving across the circuit. In either case, a current is generated in the circuit."

The ordinary "cutting of lines of force" statement is thus a derived law and involves something more than the experimental facts. This addition may be merely the mathematical theorem that for any closed circuit the integral result of the "cutting" statement is the same as that of the "flux" statement; or it may also include, consciously or unconsciously, the hypothesis that the induced electromotive force is physically and locally associated with the cutting of the induction. However natural it may have been for Faraday to localize the induction at the point where there was cutting of the lines of force, it is evident that this was theory and not experiment, for the experimental facts may be accounted for on the hypothesis of action at a distance. If Faraday's theory rather than Faraday's experiments is made the starting point, then, of course, "cutting of lines of force" is fundamental and the flux statement becomes the derived law.

3. The law of induction applies to circuits in non-conductors as well as to circuits in conductors. One of the most important applications of the law is to the free ether, where no materials are present. Mr. Hering's restriction to conductors would therefore be perfectly arbitrary and extremely inconvenient. Where the conductor is not closed the circuit is always completed through the dielectric. The induction in the dielectric must always be borne in mind.

4. It is self-evident that the fundamental statement of the law of induction applies to a linear circuit and that it can be extended to a circuit having a finite cross-section only after the system has been resolved into infinitesimal filaments. This principle is an elementary one in applied theory, but its recognition eliminates all difficulty in the application of the flux statement to Mr. Hering's "crucial" experiment. Thus the sketch shows the springs in the process of sliding over the magnet, which is resolved into a network of conducting filaments. As it is found to be immaterial how this resolution is made in the present case, a simple illustrative network is all that is attempted in the sketch. In the experiment, all of the induction passes through the iron and none of it through circuit 1. Motion of the springs does not change the number of lines

of force threading circuit 1 or circuit 2 or any of the remaining 17 circuits. Therefore, the integral electromotive force around each circuit is zero; and no currents can flow; hence the experimental result.

5. When it is possible to state a law in more than one way it is ordinarily true that the application of the law to different problems will be facilitated by the choice of different forms of the law, and that this is the case with the law of induction is not surprising or significant. As Mr. Hering happened on a unipolar experiment, he found the cutting statement more convenient, as have others before. Had he happened upon an experiment involving toroidal coils or slotted armatures, he would presumably have advocated the flux statement.

6. Mr. Hering's new machine is actually a unipolar dynamo. Faraday in his machines, preferred to make both the cutting of lines of force and the sliding of contacts perfectly uniform; this condition seems to be the ideal one and it is now being found

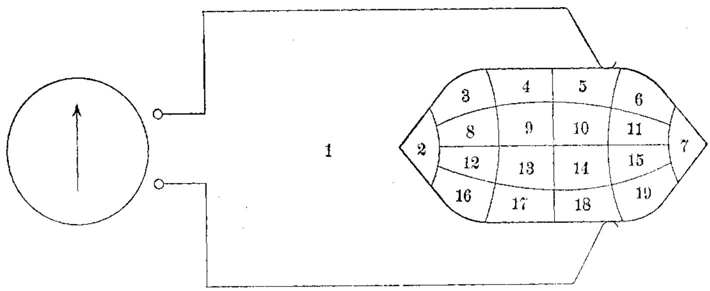

Fig. 1

to be commercial. Obviously this particular arrangement is not necessary. This is Mr. Hering's machine, while the induction cut by each spring is (on the whole) in one direction throughout the motion, the cutting and the sliding of contacts are made intermittent and alternate with each other. The expectation aroused at the beginning of Mr. Hering's paper, that the limitations of the magnetic induction machine were about to be extended, meets with disappointment. It still remains true that for a direct-current machine magnetic induction alone is not sufficient; sliding contacts, a commutator, a variable resistance or some other auxiliary device is necessary.

To summarize: The "flux" law is a literal statement of the observed facts of electromagnetic induction and enables the results to be predicted in any case whatsoever. The unipolar induction experiment described by Mr. Hering presents no exception to this rule and throws no new light upon the subject. The "cutting of lines of force" statement may be regarded either as a mathematical substitute for the "flux" statement, or as a theory localizing the observed action. Both statements of the law are useful in practice.

Tracy D. Waring (by letter): Any great generalization when concisely worded is a likely cause of misapprehension. The novel and ingenious experiment by which Mr. Hering links and unlinks magnetic flux into and out of an electrically closed conducting circuit without producing any inductive effect should go far towards removing a prevalent misunderstanding or misinterpretation of the law which for brevity we may term the Linkage Law of electromagnetic induction.

In considering some of the questions raised by Mr. Hering, it would seem appropriate to call to mind the physical conceptions as to the nature of electromagnetic induction from the point of view of electromagnetic theory.

In any circuit undergoing electromagnetic induction, the electromotive force produced in that circuit is due to, or rather owes its existence to, the electric intensity[1] developed at some or all of the points of the circuit. The seat of the electric intensity (electromagnetically produced) can only be at that place in a medium where the magnetic field is in some way changing relatively to the medium, so we may perhaps describe electromagnetic induction thus: Consider any point of a medium in which a magnetic field is sustained; then electromagnetic induction, if it exists there, may be described as the production of an electric intensity, at the point considered, by a change in the magnetic field at that point.

If this statement be true, what shall we understand by a change in a magnetic field at a point? How is such a change to be specified and by what physical conception may we picture it?

An adequate answer to these questions, if such be possible, would involve a complete physical theory of the ether and matter, a problem so profound and vast that the considerations here presented have by comparison but slight significance. These considerations are ventured, however, as laying stress on a certain aspect of the common conceptions relating to electromagnetic induction.

Taking the case of a magnetic field in an isotropic medium having unit permeability (that is, a medium in which the magnetic intensity and the magnetic induction are numerically identical), how shall we describe a change of the magnetic state at a point in such a medium and the related electric state or electric field accompanying the change of magnetic field? The magnetic field has at every point intensity and direction, either or both of which may change. A change of magnetic intensity at the point will suffice to produce an electric intensity there, the directions of the electric and the magnetic intensities being so related as to be at right angles to each other. But that is not all. Both the magnetic intensity and its direction may remain

1. Also variously termed electromotive intensity, electric force, and intensity of electric field.

constant and yet some change in the magnetic field may take place, at a point in the medium, that will produce an electric intensity there. Relative motion between the magnetic field and the medium will do this. Such relative motion then also represents a change in the magnetic field at a point.

But how shall we picture such motion? Consider any point or points of a medium in which a uniform magnetic field is sustained. Conceive, if you can, a relative translational motion of the uniform field with respect to the point or points fixed in the medium. The magnetic intensity and its direction are everywhere constant, and no instrument fixed in the medium and indicating only intensity and direction would give any indication that the field was moving; yet we can hardly doubt that something does, or may, move relatively to the medium and that the medium responds by having an electric intensity produced in it (unless the line of motion happen to coincide with the direction of the magnetic intensity).

By what physical conception can we make such relative motion have a physical meaning? Perhaps we may put it as follows: Intensity and its direction, as we measure them, do not fully specify a magnetic field at a point. It is not to be expected that so simple an expression could give more than the faintest clue to the physical state we call magnetic, for even a so-called uniform field is not really uniform for very minute dimensions—let us say, for instance, for dimensions of atomic size or perhaps much smaller. If we could only see closely enough, we would see that a so-called uniform field possesses some sort of polarized structure, magnetic filaments of some sort all running the same way and perhaps uniformly spaced.

And so we appear compelled to fall back on a conception of lines of force or tubes of induction having some sort of a real physical existence, if the relative motion between a uniform field and the medium in which it is sustained is to have physical significance.

Take a simple concrete example, say a uniformly magnetized permanent magnet, shaped something like the letter C, with pole faces broad and flat and not very far apart (see Fig. 1). Let the surrounding medium be a dielectric, insulating oil for instance, which we shall suppose remains quiescent around the fixed point, *P*.

The field near the center of the pole faces is practically uniform. Nearer the edges its intensity is weaker and changes in inclination, that is, in direction. Now what happens as the magnet moves forward, relative to the point *P* fixed in the oil, and in the direction indicated by the arrow? It is not difficult to conceive of the magnetic state being created in front of the poles as the magnet advances, and the same kind of state disappearing or being destroyed behind them, while in that part where the field is uniform the magnetic state is constant and apparently does not in any way change as the upper pole face passes over and the lower one under it. But consider the matter more closely. Imagine a minute but intelligent being, stationed at P, equipped with an instrument suitable for indicating to him the intensity and direction of the magnetic field. Watching his instrument as the magnet begins to approach him, he sees the magnetic intensity growing stronger and stronger and ever changing in direction (inclination), but presently it reaches a maximum and remains constant in magnitude and direction. He is then in that part of the field which we call uniform. But is that state, which we call magnetic, really at rest about him? Could he perhaps not see, with a wonderful imaginary eye, the magnetic filaments or magnetic whirls or lines of stress or flow or whatever they are—could he not see them file past him as the pole faces move above and below him, or, as a last resort, could he not devise an instrument that would indicate an electric intensity in the field about him?

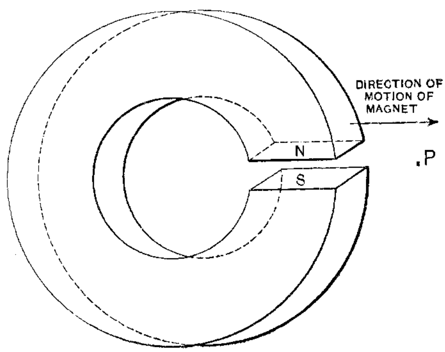

Fig.1

Considerations similar to these arise in considering the rotation of a magnetic field about a line of force as an axis which happens to be also an axis of symmetry for the field. For instance, Mr. Hering raises the question: Does the magnetic field of a round, uniformly magnetized bar magnet rotate with the magnet if the magnet be made to rotate about its axis by mechanical means? Here for any given point in the field the magnetic intensity is constant in direction and magnitude, whether the field rotates or remains stationary?[2]

Sir Oliver Lodge believes that the field does rotate with the magnet, or at least he says: "If a magnet were spun on its axis rapidly by mechanical means, there is very little doubt but that it would act on charged bodies in its neighborhood, tending to

2. We are of course not considering enormous hypothetical peripheral speeds, such as would be comparable with the rate of propagation of magnetic disturbances.

make them move radially either to or from it. This, however, is an experiment that ought to be tried; and the easiest way of trying it would be to suspend a sort of electrometer needle, electrified positive at one end and negative at the other, near the spinning magnet, and to look for a trace of deflection—to be reversed when the spin is reversed. A magnet of varying strength might be easier to try than a spinning one."[3] Mr. Hering suggested the use of a single wire and a condenser for an experiment of a similar character.[4]

To fix our ideas, imagine the arrangement indicated in Fig. 2, which represents a bar magnet capable of rotating around its axis and encircled by two concentric and coaxial cylindrical conducting surfaces. The two surfaces, which we may look upon as the two plates of a cylindrical condenser, are fixed in space, while the magnet may be made to rotate by some mechanical means not indicated in the figure.

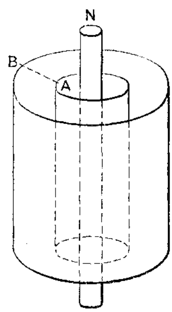

Fig. 2

Now assume the magnet to be rotating around its axis at a high speed and then place a conductor, say a piece of wire, across from *A* to *B*. If it be true that the field rotates with the magnet, then such of the flux as passes between the cylinders will cut the wire and the cylinders will become charged electrically. Their electrical condition could be investigated after first removing the wire and then allowing the magnet to come to rest.

The experiment might perhaps be modified so as to get cumulative results by means of water-drop collectors as in Fig. 3. With this arrangement the magnetic flux of the rotating field cuts the wire, establishing an electromotive force

3. Modern Views of Electricity, Section 73, 1907 edition, by Oliver Lodge. Also see Lodge, *Phil. Mag.*, June 1889, page 469.

4. A New Factor in Induction; the "Loop" *versus* the "Cutting Lines of Force Law", by Carl Hering, *Electrical World*, March 14, 1908.

between the ends *a* and *b*, in consequence of which the water-drops at the extremities of the conductor take on opposite electrical charges. The charges are carried off by the water-drops, and the latter in turn give up their charges to the collecting cylinders which should become more and more heavily charged.

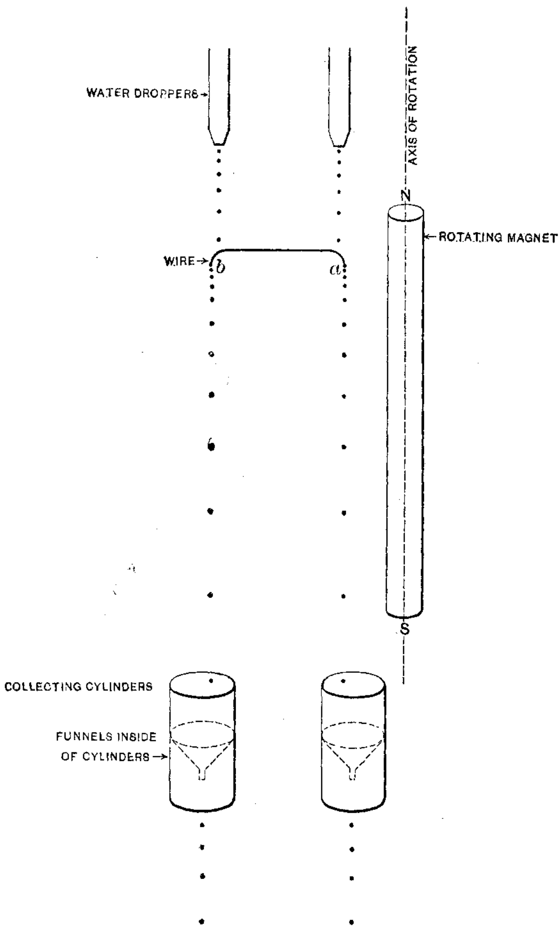

Fig.3

Of these suggested experiments that proposed by Lodge is perhaps of the greater theoretical interest, as it could be performed in some suitable dielectric without perhaps using any conducting material in the field, and is in line with the experiment so successfully carried out by Dr. H. A. Wilson in which he measured the electric displacement in a dielectric resulting from its motion in a magnetic field.[5]

Carl Hering (by letter): Concerning Dr. Steinmetz's remarks, I am greatly pleased to see that he agrees with me on the main points. The statement of the general law of electromagnetic induction which he gives is probably the first statement which has ever been published of a form of the law which is really universal, so far as we know now, and I hope that in the future it will be copied into text-books freely; I know of no case, no matter how complicated, in which the student would be misled into getting wrong conclusions, or even only doubtful conclusions, when applying this new statement of the law. The only comment I have to make about it is that, to be quite accurate, it should be preceded by a statement that it applies only to elemental conductors, that is, to conductors the cross-section of which is negligibly small; when the cross-section is relatively large, some further limitations would have to be introduced into the law to make it strictly correct and these limitations would complicate it.

What Dr. Steinmetz says concerning the form of induction in unipolar machines, was, of course, well known to me, but the usual unipolar machine does not come directly under Maxwell's law as there is no definite limited loop in which the enclosed flux increases or diminishes. Hence the induction in unipolar machines has, for many years, not been considered a direct contradiction of Maxwell's law, or of the law of linkages. In my experiment, however, the conditions are such that Maxwell's law can be applied directly, there being a very well-defined permanently closed loop and a well-defined increase and decrease of flux enclosed by that loop. It therefore contradicts Maxwell's law directly, while the induction in unipolar machines does not do so, directly. I am pleased that Dr. Steinmetz agrees with me that Maxwell's and Thomson's statements of the law are not universal, but refer only to special cases. If Maxwell's law applies only to " conditions of continuity of conductor

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5. See Proc. Roy. Soc., vol. 73, 1904. The experiment consisted in rotating a hollow cylinder or tube of ebonite about its axis, the rotation being performed in a magnetic field the direction of which coincided with the axis of rotation. The inner and the outer cylindrical surfaces were each covered with a metallic film, against each of which sliding brushes made contact. The brushes were connected to a quadrant electrometer, the deflection of the latter thus becoming a measure of the charge displaced through the walls of the cylinder.

The results indicated that an electromotive force was produced between the inner and the outer conducting cylindrical surfaces the same as though a conductor had been used instead of ebonite, but that the value of the electromotive force was less for the ebonite in the ratio of $\frac{K - K_0}{K}$; K being the electric permitivity of the ebonite and K_0 that of free ether, that is numerically $K_0 = 1$ when $K =$ the specific inductive capacity of the moving dielectric (in this case ebonite).

See also Blondlot, *Journal de Physique*, January 1902.

and continuity of motion", as Dr. Steinmetz states it, this limiting condition ought always to accompany the statement of Maxwell's law; but this is not the case in most text-books; and the student should not be left to find out these limiting conditions himself.

Dr. Steinmetz's description of why a magnetic field remains fixed in space while the magnet generating it revolves on its axis is interesting. It seems, however, that high authorities differ on this point,* hence, the question must still be considered an open one.

I am pleased to see that Dr. Kennelly agrees with me that the movement of magnetic flux across the boundary is the primeordial proposition and the increase or diminution of flux in a loop is secondary. This statement is merely another way of saying that Faraday's statement of the law is the fundamental one and explains the seat of the induction, while Maxwell's statement is a deduction from it, applying only to special cases with strict limitations (which latter do not generally accompany the law in text-books).

One of Professor Elihu Thomson's remarks, namely, that he formerly used Maxwell's law, but later adopted Faraday's view of line cutting as the essential thing, bears out the contention that Faraday's law is the more reliable. His proposed universal law, followed afterwards by an admitted exception, illustrates the difficulty of framing a universal law.

Dr. Franklin's remarks show that the teacher who looks at these phenomena in a broad and general way will be apt to supply in his own mind the omitted limitations of a briefly stated law or rule. Students, however, as also many who are engaged in practical work, accept a law as it is stated and apply it literally; hence they may be very seriously misled unless the limitations clearly accompany such a law.

Mr. Thomas's remarks, in my opinion, illustrate very forcibly my contention that the statement of Maxwell's law is imperfect; for if it requires such a complicated interpretation as he gives in order to make it fit this experiment, and perhaps even more intricate interpretations to fit other possible cases, it certainly is not in correct form to be given to students and practicing engineers to use as it reads. A careful student reading Mr. Thomas's explanation, would naturally feel great uncertainty in applying Maxwell's statement of the law to other unusual cases before he knew the result. If, in this experiment, the continuously closed circuit must be considered as being broken (as Mr. Thomas states in italics), then it must necessarily follow that in all other sliding contacts, such as those on alternators, revolving fields, induction motors, etc., the circuit must be considered to be broken all the time. Hence the

* See Modern Views of Electricity, Lodge, 1907, Section 73, page 142. Also A Treatise on Magnetism and Electricity, Andrew Gray, 1898, page 329, Section 423, in which the author says: " When the magnet moves, its field of force moves with it."

current is flowing through a broken circuit without causing an arc—which, of course, leads to an absurdity.

Replying to Mr. Graham, I need only say that the experiment deals only with permanently closed circuits through which a steady current could be flowing all the time. Hence his analogy to a circuit which is opened and the flux moved out through the opening, is not legitimate discussion of the experiment itself.

Mr. Hanchett's first experiment is essentially different from mine and has no bearing on mine, as it distinctly opens and closes a circuit at a switch. His second experiment is described in my paper, Fig. 3. It seems that Mr. Hanchett has evidently failed to see the very point of the experiment, which was that in teaching students the laws and rules for their practical use, these laws and rules should be so stated that the student will readily understand them, can feel absolute reliance in them, can predict results with certainty and reliance, and will not have to resort to complicated interpretations (like leaving a part of a circuit behind notwithstanding that all of it has been visibly removed) in order to make the laws fit special cases after experimental investigation has shown what the result is.

Mr. Waring's suggestions of methods to determine whether a field moves with its magnet or not, are very ingenious, particularly the one with the water-drop collectors, which it seems to me would not be difficult to carry out. As the result is a simple question of fact concerning which the theories of able authorities give contradictory results, it would be very interesting to have this experiment carried out. I had suggested the use of condensers for such an experiment, but his water-drop method would give cumulative results, which makes the measurement easier. The precaution of course should be taken to stop the rotation of the magnet before connections are made with the electrometer, so that there could not be any question of a possible induction in any leads, which has heretofore been the great stumbling-block in any direct measurements.

Mr. Campbell's statement that "the current is determined by the integral electromotive force around the circuit" is, in my opinion, an evident fallacy, as every student will know that a current is not determined by the electromotive force, but by the quotient of the electromotive force and the resistance; without a knowledge of the resistance the current is a decidedly indeterminate quantity. In his paragraph 2 he virtually says that "as the current" is "invariably closed, it follows as a mathematical necessity that" there cannot be any such a thing as an open circuit, hence Maxwell's loop law is universal!

Of course if one starts out with the arbitrary assumption that an electric circuit cannot ever be opened, then one thereby incapacitates himself to even consider such a thing as induction of electromotive force in an open circuit. Persons who have been left embarrassed in the dark by an opening of the electric light supply circuit, or whose motors have been stalled for a similar reason, will not have much faith in arguments as to the impossibility of opening circuits. If Mr. Campbell will read Faraday he will find that this distinguished experimenter invariably made experiments first and suggested theories to accord with the facts afterwards. I believe he was never guilty of torturing facts to fit premature theories; he never to my knowledge had to interpret a closed circuit to be open or an open one to be closed, in order to fit a pet theory to the facts. To Mr. Campbell's arbitrary assertion that induction " where no materials are present " is " one of the most important applications " of the law under discussion, it is I hope not necessary to reply; the usual industrial use of induction is to get useful currents, which could not flow where there are no materials to conduct and confine them. Why should we be expected to accept his very arbitrary and non-proved statement (4) that " it is self-evident that the fundamental statement of the law of induction applies to linear circuits " (presumably meaning closed circuits or complete loops, as he described before). Authorities, who are recognized both here and abroad, consider the general case to include all kinds of circuits whether open or closed, the closed circuit being therefore a special case; in order to be fundamental, a law must apply to the general case. Many who have been asked have acknowledged frankly, and in a true scientific spirit, that Maxwell's law as usually stated would not have predicted the correct results of this experiment before the results were known: hence Mr. Campbell's arbitrary statement that it " enables the results to be predicted in any case whatsoever " is not borne out by facts. That the experiment " throws no new light on the subject " is a matter about which others have expressed a different opinion to me. One of the chief objects of my paper was to try to show that there is room for improvement in teaching students the fundamental laws, so that they will obtain such a clear and unencumbered conception of those laws that they will have confidence in applying them for predicting results, without involving complicated " interpretations "—such as having to consider an open circuit to be a closed one, or the reverse.—which require an experimental determination before a correct prediction can be made. Mr. Campbell's discussion is a good illustration of this.

Mr. Thomas has claimed above that what is universally considered by electrical engineers to be a closed circuit must now be considered to be an open circuit in order to make his theory fit this case, and now Mr. Campbell claims that what would generally be considered to be an open circuit must now be considered to be a closed one in order to make his theory fit this case. I think this is a good illustration of the point raised in the paper, that Maxwell's law is not in a satisfactory form for teaching students, or for engineers to use.