II

ON VERY RAPID ELECTRIC OSCILLATIONS

(*Wiedemann's Annalen*, **31**, p. 421, 1887.)

THE electric oscillations of open induction-coils have a period of vibration which is measured by ten-thousandths of a second. The vibrations in the oscillatory discharges of Leyden jars, such as were observed by Feddersen,[1] follow each other about a hundred times as rapidly. Theory admits the possibility of oscillations even more rapid than these in open wire circuits of good conductivity, provided that the ends are not loaded with large capacities; but at the same time theory does not enable us to decide whether such oscillations can be actually excited on such a scale as to admit of their being observed. Certain phenomena led me to expect that oscillations of the latter kind do really occur under certain conditions, and that they are of such strength as to allow of their effects being observed. Further experiments confirmed my expectation, and I propose to give here an account of the experiments made and the phenomena observed.

The oscillations which are here dealt with are about a hundred times as rapid as those observed by Feddersen. Their period of oscillation—estimated, it is true, only by the aid of theory—is of the order of a hundred-millionth of a second. Hence, according to their period, these oscillations range themselves in a position intermediate between the acoustic oscillations of ponderable bodies and the light-oscillations of the ether. In this, and in the possibility that a closer observa-

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[1] For the literature see Colley, *Wied. Ann.* **26**, p. 432, 1885. (See also Note 1 at the end of this book.)

tion of them may be of service in the theory of electrodynamics, lies the interest which they present.

Preliminary Experiments

If, in addition to the ordinary spark-gap of an induction-coil, there be introduced in its discharging circuit a Riess's spark-micrometer, the poles of which are joined by a long metallic shunt, the discharge follows the path across the air-gap of the micrometer in preference to the path along the metallic conductor, so long as the length of the air-gap does not exceed a certain limit. This is already known, and the construction of lightning-protectors for telegraph-lines is based on this experimental fact. It might be expected that, if the metallic shunt were only made short and of low resistance, the sparks in the micrometer would then disappear. As a matter of fact, the length of the sparks obtained does diminish with the length of the shunt, but the sparks can scarcely be made to disappear entirely under any circumstances. Even when the two knobs of the micrometer are connected by a few centimetres of thick copper wire sparks can still be observed, although they are exceedingly short. This experiment shows directly that at the instant when the discharge occurs the potential along the circuit must vary in value by hundreds of volts even in a few centimetres; indirectly it proves with what extraordinary rapidity the discharge takes place. For the difference of potential between the knobs of the micrometer can only be regarded as an effect of self-induction in the metallic shunt. The time in which the potential of one of the knobs is appreciably changed is of the same order as the time in which such a change is transmitted to the other knob through a short length of a good conductor. The potential difference between the micrometer-knobs might indeed be supposed to be determined by the resistance of the shunt, the current-density during the discharge being possibly large. But a closer examination of the quantitative relations shows that this supposition is inadmissible; and the following experiment shows independently that this conjecture cannot be put forward. We again connect the knobs of the micrometer by a good metallic conductor, say by a copper wire 2 mm. in diameter and 0·5 metre long, bent into rectangular form; we do not, however, introduce this into the discharging-circuit of the induction-coil, but we simply place one pole of it in communication with any point of the discharging circuit by means of a connecting wire. (Fig. 6 shows the arrangement of the apparatus; *A* represents diagrammatically the induction-coil, *B* the discharger, and *M* the micrometer.) Thereupon we again observe, while the induction-coil is working, a stream of sparks in the micrometer which may, under suitable conditions, attain a length of several millimetres. Now this experiment shows, in the first place, that at the instant when the discharge takes place violent electrical disturbances occur, not only in the actual discharging-circuit, but also in all conductors connected with it. But, in the second place, it shows more clearly than the preceding experiment that these disturbances run on so rapidly that even the time taken by electrical waves in rushing through short metallic conductors becomes of appreciable importance. For the experiment can only be interpreted in the sense that the change of potential proceeding from the induction-coil reaches the knob 1 in an appreciably shorter time than the knob 2. The phenomenon may well cause surprise when we consider that, as far as we know, electric waves in copper wires are propagated with a velocity which is approximately the same as that of light. So it appeared to me to be worth while to endeavour to determine what conditions were most favourable for the production of brilliant sparks in the micrometer. For the sake of brevity we shall speak of these sparks as the side-sparks (in order to distinguish them from the discharge proper), and of the micrometer discharging-circuit as the side-circuit (*Nebenkreis*).

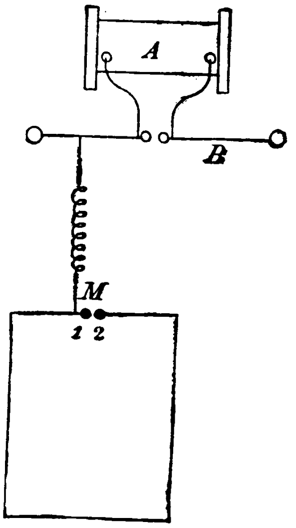

Fig. 6.

First of all it became evident that powerful discharges are necessary if side-sparks of several millimetres in length are desired. I therefore used in all the following experiments a large Ruhmkorff coil, 52 cm. long and 20 cm. in diameter, which was provided with a mercury interrupter and was excited by six large Bunsen cells. Smaller induction-coils gave the same qualitative results, but the side-sparks were shorter, and it was therefore more difficult to observe differences between them. The same held good when discharges from Leyden jars or from batteries were used instead of the induction-coil. It further appeared that even when the same apparatus was used a good deal depended upon the nature of the exciting spark in the discharger (*B*). If this takes place between two points, or between a point and a plate, it only gives rise to very weak side-sparks; discharges in rarefied gases or through Geissler tubes were found to be equally ineffective. The only kind of spark that proved satisfactory was that between two knobs (spheres), and this must neither be too long nor too short. If it is shorter than half a centimetre the side-sparks are weak, and if it is longer than $1\frac{1}{2}$ cm. they disappear entirely.

In the following experiments I used, as being the most suitable, sparks three-quarters of a centimetre long between two brass knobs of 3 cm. diameter. Even these sparks were not always equally efficient; the most insignificant details, often without any apparent connection, resulted in useless sparks appearing instead of active ones. After some practice one can judge from the appearance and noise of the sparks whether they are such as are able to excite side-sparks. The active sparks are brilliant white, slightly jagged, and are accompanied by a sharp crackling. That the spark in the discharger is an essential condition of the production of shunt-sparks is easily shown by drawing the discharger-knobs so far apart that the distance between them exceeds the sparking distance of the induction-coil; every trace of the side-sparks then disappears, although the differences of potential now present are greater than before.

The length of the micrometer-circuit naturally has great influence upon the length of the sparks in it. For the greater this distance, the greater is the retardation which the electric wave suffers between the time of its arrival at the one knob and at the other. If the side-circuit is made very small, the side-sparks become extremely short; but it is scarcely possible to prepare a circuit in which sparks will not show themselves under favourable circumstances. Thus, if you file the ends of a stout copper wire, 4-6 cm. long, to sharp points, bend it into an almost closed circuit, insulate it and now touch the discharger with this small wire circuit, a stream of very small sparks between the pointed ends generally accompanies the discharges of the induction-coil. The thickness and material (and therefore the resistance) of the side-circuit have very little effect on the length of the side-sparks. We were therefore justified in declining to attribute to the resistance the differences of potential which arise. And according to our conception of the phenomenon, the fact that the resistance is of scarcely any importance can cause us no surprise; for, to a first approximation, the rate of propagation of an electric wave along a wire depends solely upon its capacity and self-induction, and not upon its resistance. The length of the wire which connects the side-circuit to the principal circuit has also little effect, provided it does not exceed a few metres. We must assume that the electric disturbance which proceeds from the principal circuit travels along it without suffering any real change of intensity.

On the other hand, the position of the point at which contact with the side-circuit is made has a very noteworthy effect upon the length of the sparks in it. We should expect this to be so if our interpretation of the phenomenon is correct. For if the point of contact is so placed that the paths from it to the two knobs of the micrometer are of equal length, then every variation which passes through the connecting wire will arrive at the two knobs in the same phase, so that no difference of potential between them can arise. Experiment confirms this supposition. Thus, if we shift the point of contact on the side-circuit, which we have hitherto supposed near one of the micrometer-knobs, farther and farther away from this, the spark-length diminishes, and in a certain position the sparks disappear completely or very nearly so; they become stronger again in proportion as the contact approaches the second micrometer-knob, and in this position attain the same length as in the first. The point at which the spark-length is a minimum may be called the null-point. It can generally be determined to within a few centimetres. It always divides the length of the wire between the two micrometer-knobs into very nearly equal parts. If the conductor is symmetrical on the right and left of the line joining the micrometer and the null-point, the sparks always disappear completely, the phenomenon can be observed even with quite short side-circuits. Fig. 7 shows a convenient arrangement of the experiment; *a b c d* is a rectangle of bare copper wire 2 mm. in diameter, insulated upon sealing-wax supports;[1] in my experiments it was 80 cm. broad and 125 long. When the connecting wire is attached to either of the knobs 1 and 2, or either of the points *a* and *b*, sparks 3-4 mm. long pass between 1 and 2; no sparks can be obtained when the connection is at the point *e*, as in the figure; shifting the contact a few centimetres to right or left causes the sparks to reappear. It should be remarked that we consider sparks as being perceptible when they are only a few hundredths of a millimetre in length.

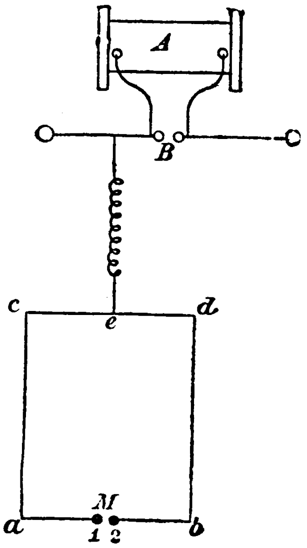

Fig 7.

The following experiment shows that the above is not a complete representation of the way in which things go on. For if, after the contact has been adjusted so as to make the sparks disappear, we attach to one of the micrometer-knobs another conductor projecting beyond it, active sparking again occurs. This conductor, being beyond the knob, cannot affect the simultaneous arrival of the waves travelling from *e* to 1 and 2. But it is easy to see what the explanation of this experiment is. The waves do not come to an end after rushing once towards *a* and *b*; they are reflected and traverse the side-circuit several, perhaps many, times and so give rise to stationary oscillations in it. If the paths *e c a* 1 and *e d b* 2 are equal, the reflected waves will again arrive at 1 and 2 simultaneously. If, however, the wave reflected from one of the knobs is missing, as in the last experiment, then, although the first disturbance proceeding from *e* will not give rise to sparks, the reflected waves will. We must therefore imagine the abrupt variation which arrives at *e* as creating in the side-circuit the oscillations which are natural to it, much as the blow of a hammer produces in an elastic rod its natural vibrations. If this idea is correct, then

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[1] [See Note 2 at end of book.]

the condition for disappearance of sparks in *M* must substantially be equality of the vibration-periods of the two portions *e* 1 and *e* 2. These vibration-periods are determined by the product of the coefficient of self-induction of those parts of the conductor into the capacity of their ends; they are practically independent of the resistance of the branches. The following experiments may be applied to test these considerations and are found to agree with them :—

If the connection is placed at the null-point and one of the micrometer-knobs is touched with an insulated conductor, sparking begins again because the capacity of the branch is increased. An insulated sphere of 2-4 cm. diameter is quite sufficient. The larger the capacity which is thus added, the more energetic becomes the sparking. Touching at the null-point has no influence since it affects both branches equally. The effect of adding a capacity to one branch is annulled by adding an equal capacity to the other. It can also be compensated by shifting the connecting wire in the direction of the loaded branch, *i.e.* by diminishing the self-induction of the latter. The addition of a capacity produces the same effect as increasing the coefficient of self-induction. If one of the branches be cut and a few centimetres or decimetres of coiled copper wire introduced into it, sparking begins again. The change thus produced can be compensated by inserting an equal length of copper wire in the other branch, or by shifting the copper wire towards the branch which was altered, or by adding a suitable capacity to the other branch. Nevertheless, it must be remarked that when the two branches are not of like kind, a complete disappearance of the sparks cannot generally be secured, but only a minimum of the sparking distance.

The results are but little affected by the resistance of the branch. If the thick copper wire in one of the branches was replaced by a much thinner copper wire or by a wire of German silver, the equilibrium was not disturbed, although the resistance of the one branch was a hundred times that of the other. Very large fluid resistances certainly made it impossible to secure a disappearance of the sparks, and short air-spaces introduced into one of the branches had a like effect.

The self-induction of iron wires for slowly alternating currents is about eight to ten times as great as that of copper wires of equal length and thickness. I therefore expected that short iron wires would produce equilibrium with longer copper wires. This expectation was not confirmed; the branches remained in equilibrium when a copper wire was replaced by an iron wire of equal length. If the theory of the observations here given is correct, this can only mean that the magnetism of iron is quite unable to follow oscillations so rapid as those with which we are here concerned, and that it, therefore, is without effect. A further experiment which will be described below appears to point in the same direction.

Induction-Effects of unclosed Currents

The sparks which occur in the preceding experiments owe their origin, according to our supposition, to self-induction. but if we consider that the induction-effects in question are derived from exceedingly weak currents in short, straight conductors, there appears to be good reason to doubt whether these do really account satisfactorily for the sparks. In order to settle this doubt I tried whether the observed electrical disturbances did not manifest effects of corresponding magnitude in neighbouring conductors. I therefore bent some copper wire into the form of rectangular circuits, about 10-20 cm. in the side, and containing only very short spark-gaps. These were insulated and brought near to the conductors in which the disturbances took place, and in such a position that a side of the rectangle was parallel to the conductor. When the rectangle was brought sufficiently near, a stream of sparks in it always accompanied the discharges of the induction-coil. These sparks were most brilliant in the neighbourhood of the discharger, but they could also be observed along the wire leading to the side-circuit as well as in the branches of the latter. The absence of any direct discharge between the inducing and induced circuits was carefully verified, and was also prevented by the introduction of a solid insulator. Thus it is scarcely possible that our conception of the phenomenon is erroneous. That the induction between two simple straight lengths of wire, traversed by only small quantities of electricity, can yet become strong enough to produce sparks, shows again the extraordinary shortness of the time in which these small quantities of electricity must pass backwards and forwards along the conductors.

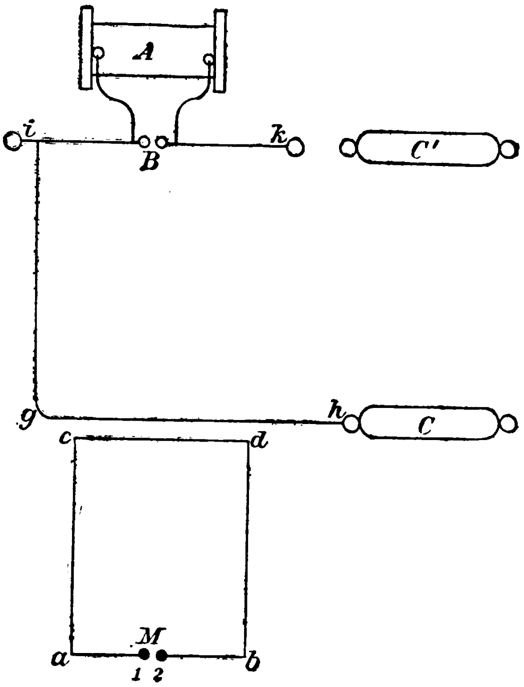

Fig. 8.

In order to study the phenomena more closely, the rectangle which at first was employed as the side-circuit was again brought into use, but this time as the induced circuit. Along the short side of this (as indicated in Fig. 8) and at a distance of 3 cm. from it was stretched a second copper wire *g h*, which was placed in connection with any part of the discharger. As long as the end *h* of the wire *g h* was free, only weak sparks appeared in the micrometer *M*, and these were due to the discharge-current of the wire *g h*. But if an insulated conductor *C*—one taken from an electrical machine—was then attached to *h*, so that larger quantities of electricity had to pass through the wire, sparks up to two millimetres long appeared in the micrometer. This was not caused by an electrostatic effect of the conductor, for if it was attached to *g* instead of to *h*, it was without effect; and the action was not due to the charging-current of the conductor, but to the sudden discharge brought about by the sparks. For when the knobs of the discharger were drawn so far apart that sparks could no longer spring across it, then the sparks disappeared completely from the induced circuit as well. Not every kind of spark produced a sufficiently energetic discharge; here, again, only such sparks as were before found to occasion powerful side-sparks were found to be effective in exciting the inductive action. The sparks excited in the secondary circuit passed not only between the knobs of the micrometer but also from these to other insulated conductors held near. The length of the sparks was notably diminished by attaching to the knobs conductors of somewhat large capacity or touching one of them with the hand; clearly the quantities of electricity set in motion were too small to charge conductors of rather large capacity to the full potential. On the other hand, the sparking was not much affected by connecting the two micrometer-knobs by a short wet thread. No physiological effects of the induced current could be detected; the secondary circuit could be touched or completed through the body without experiencing any shock.

Certain accessory phenomena induced me to suspect that the reason why the electric disturbance in the wire *g h* produced such a powerful inductive action lay in the fact that it did not consist of a simple charging-current, but was rather of an oscillatory nature. I therefore endeavoured to strengthen the induction by modifying the conditions so as to make them more favourable for the production of powerful oscillations. The following arrangement of the experiment suited my purpose particularly well. I attached the conductor *C* as before to the wire *g h* and then separated the micrometer-knobs so far from each other that sparks only passed singly. I then attached to the free pole of the discharger *k* (Fig. 8) a second conductor *C′* of about the same size as the first. The sparking then again became very active, and on drawing the micrometer-knobs still farther apart decidedly longer sparks than at first could be obtained. This cannot be due to any direct action of the portion of the circuit *i k*, for this would diminish the effect of the portion *g h*; it must, therefore, be due to the action of the conductor *C′* upon the discharge-current of *C*. Such an action would be incomprehensible if we assumed that the discharge of the conductor *C* was aperiodic. It becomes, however, intelligible if we assume that the inducing current in *g h* consists of an electric oscillation which, in the one case, takes place in the circuit *C*—wire *g h*—discharger, and in the other in the system *C*—wire *g h*, wire *i k*—*C′*. It is clear in the first place that the natural oscillations of the latter system would be the more powerful, and in the second place that the position of the spark in it is more suitable for exciting the vibration.

Further confirmation of these views may be deferred for the present. But here we may bring forward in support of them the fact that they enable us to give a more correct explanation of the part which the discharge of the Ruhmkorff coil plays in the experiment. For if oscillatory disturbances in the circuit *C*—*C′* are necessary for the production of powerful induction-effects, it is not sufficient that the spark in this circuit should be established in an exceedingly short time, but it must also reduce the resistance of the circuit below a certain value, and in order that this may be the case the current-density from the very start must not fall below a certain limit. Hence it is that the inductive effect is exceedingly feeble when the conductors *C* and *C′* are charged by means of an electrical machine[1] (instead of a Ruhmkorff coil) and then allowed to discharge themselves; and that it is also very feeble when a small coil is used, or when too large a spark-gap is introduced; in all these cases the motion is aperiodic. On the other hand, a powerful discharge from a Ruhmkorff coil gives rise to oscillations, and therefore to powerful disturbances all round, by performing the following functions:—In the first place, it charges the ends *C* and *C′* of the system to a high potential; secondly, it gives rise to a disruptive discharge; and thirdly, after starting the discharge, it keeps the resistance of the air-gap so low that oscillations can take place. It is known that if the capacity of the ends of the system is large—if, for example, they consist of the armatures of a battery of Leyden jars—the discharge-current from these capacities is able of itself to reduce the resistance of the spark-gap considerably; but when the capacities are small this function must be performed by some extraneous discharge, and for this reason the discharge of the induction-coil is, under the conditions of our experiment, absolutely necessary for exciting oscillations.

As the induced sparks in the last experiment were several millimetres long, I had no doubt that it would be possible to obtain sparks even when the wires used were much farther apart; I therefore tried to arrange a modification of the experiment which appeared interesting. I gave the inducing circuit the form of a straight line (Fig. 9). Its ends were formed by the conductors *C* and *C′*. These were 3 metres apart, and were connected by a copper wire 2 mm. thick, at the centre of which was the discharger of the induction-coil. The induced circuit was the same as in the preceding experi-

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[1] [See Note 3 at end of book.]

ment, 120 cm. long and 80 cm. broad. If the shortest distance between the two systems was now made equal to 50 cm., induced sparks 2 mm. in length could still be obtained; at greater distances the spark-length decreased

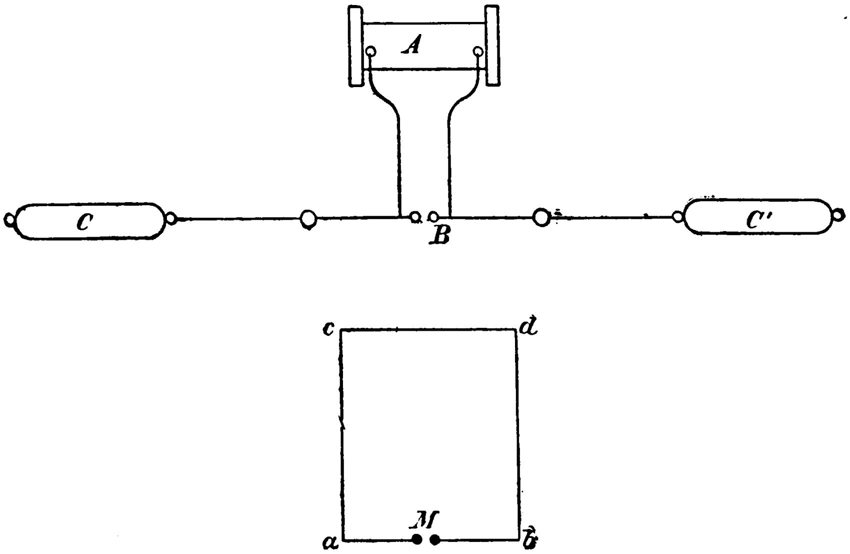

Fig. 9.

rapidly, but even when the shortest distance was 1·5 metres, a continuous stream of sparks was perceptible. The experiment was in no way interfered with if the observer moved between the inducing and induced systems. A few control-experiments again established the fact that the phenomena observed were really caused by the current in the rectilinear portion. If one or both halves of this were removed, the sparks in the micrometer ceased, even when the coil was still in action. They also ceased when the knobs of the discharger were drawn so far apart as to prevent any sparking in it. Inasmuch as the difference of electrostatic potential at the ends of the conductors C and C' are now greater than before, this shows that these differences of potential are not the cause of the sparks in the micrometer.

Hitherto the induced circuit was closed; it was, however, to be supposed that the induction would take place equally in an open circuit. A second insulated copper wire was therefore stretched parallel to the straight wire in the preceding arrangement, and at a distance of 60 cm. from it. This second wire was shorter than the first; two insulated spheres 10 cm. in diameter were attached to its ends and the spark-micrometer was introduced in the middle of it. When the coil was now started, the stream of sparks from it was accompanied by a similar stream in the secondary conductor. But this experiment should be interpreted with caution, for the sparks observed are not solely due to electromagnetic induction. The alternating motion in the system $C\ C'$ is indeed superposed upon the Ruhmkorff discharge itself. But during its whole course the latter determines an electrification of the conductor C, and an opposite electrification of the conductor C'. These electrifications had no effect upon the closed circuit in the preceding experiment, but in the present discontinuous conductor they induce by purely electrostatic action opposite electrifications in the two parts of the conductor, and thus produce sparks in the micrometer. In fact, if we draw the knobs of the discharger so far apart that the sparks in it disappear, the sparks in the micrometer, although weakened, still remain. These sparks represent the effect of electrostatic induction, and conceal the effect which alone we desired to exhibit.

There is, however, an easy way of getting rid of these disturbing sparks. They die away when we interpose a bad conductor between the knobs of the micrometer, which is most simply done by means of a wet thread. The conductivity of this is obviously good enough to allow the current to follow the relatively slow alternations of the discharge from the coil; but in the case of the exceedingly rapid oscillations of the rectilinear circuit it is, as we have already seen, not good enough to bring about an equalisation of the electrifications. If after placing the thread in position we again start the sparking in the primary circuit, vigorous sparking begins again in the secondary circuit, and is now solely due to the rapid oscillations in the primary circuit. I have tested to what distance this action extended. Up to a distance of 1·2 metres between the parallel wires the sparks were easily perceptible; the greatest perpendicular distance at which regular sparking could be observed was 3 metres. Since the electrostatic effect diminishes more rapidly with increasing distance than the electromagnetic induction, it was not necessary to complicate the experiment by using the wet thread at greater distances, for, even without this, only those discharges which excited oscillations in the primary wire were attended by sparks in the secondary circuit.

I believe that the mutual action of rectilinear open circuits which plays such an important part in theory is, as a matter of fact, illustrated here for the first time.

Resonance Phenomena

We may now regard it as having been experimentally proved that currents of rapidly varying intensity, capable of producing powerful induction-effects, are present in conductors which are connected with the discharge circuit. The existence of regular oscillations, however, was only assumed for the purpose of explaining a comparatively small number of phenomena, which might perhaps be accounted for otherwise. But it seemed to me that the existence of such oscillations might be proved by showing, if possible, symphonic relations between the mutually reacting circuits. According to the principle of resonance, a regularly alternating current must (other things being similar) act with much stronger inductive effect upon a circuit having the same period of oscillation than upon one of only slightly different period.[1] If, therefore, we allow two circuits, which may be assumed to have approximately the same period of vibration, to react on one another, and if we vary continuously the capacity or coefficient of self-induction of one of them, the resonance should show that for certain values of these quantities the induction is perceptibly stronger than for neighbouring values on either side.

The following experiments were devised in accordance with this principle, and, after a few trials, they quite answered my intention. The experimental arrangement was very nearly the same as that of Fig. 9, excepting that the circuits were made somewhat different in size. The primary conductor was a perfectly straight copper wire 2·6 metres long and 5 mm. thick. This was divided in the middle so as to include the spark-gap. The two small knobs between which the discharge took place were mounted directly on the wire and connected with the poles of the induction-coil. To the ends of the wire were attached two spheres, 30 cm. in diameter, made of strong zinc-plate. These could be shifted along the wire. As they formed (electrically) the ends of the conductor, the circuit

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[1] Cf. Oberbeck, *Wied. Ann.* **26**, p. 245, 1885.

could easily be shortened or lengthened. The secondary circuit was proportioned so that it was expected to have a somewhat smaller period of oscillation than the primary; it was in the form of a square 75 cm. in the side, and was made of copper wire 2 mm. in diameter. The shortest distance between the two systems was made equal to 30 cm., and at first the primary current was allowed to remain of full length. Under these circumstances the length of the biggest spark in the induced circuit was 0·9 mm. When two insulated metal spheres of 8 cm. diameter were placed in contact with the two poles of the circuit, the spark-length increased, and could be made as large as 2·5 mm. by suitably diminishing the distance between the two spheres. On the other hand, if two conductors of very large surface were placed in contact with the two poles, the spark-length was reduced to a small fraction of a millimetre. Exactly similar results followed when the poles of the secondary circuit were connected with the plates of a Kohlrausch condenser. When the plates were far apart the spark-length was increased by increasing the capacity, but when they were brought closer together the spark-length again fell to a very small value. The easiest way of adjusting the capacity of the secondary circuit was by hanging over its two ends two parallel bits of wire and altering the length of these and their distance apart. By careful adjustment the sparking distance was increased to 3 mm., after which it diminished, not only when the wires were lengthened, but also when they were shortened. That an increase of the capacity should diminish the spark-length appeared only natural; but that it should have the effect of increasing it can scarcely be explained excepting by the principle of resonance.

If our interpretation of the above experiment is correct, the secondary circuit, before its capacity was increased, had a somewhat shorter period than the primary. Resonance should therefore have occurred when the rapidity of the primary oscillations was increased. And, in fact, when I reduced the length of the primary circuit in the manner above indicated, the sparking distance increased, again reached a maximum of 3 mm. when the centres of the terminal spheres were 1·5 metres apart, and again diminished when the spheres were brought still closer together. It might be supposed that the spark-length would now increase still further if the capacity of the secondary circuit were again, as before, increased. But this is not the case; on attaching the same wires, which before had the effect of increasing the spark-length, this latter falls to about 1 mm. This is in accordance with our conception of the phenomenon; that which at first brought about an equality between the periods of oscillation now upsets an equality which has been attained in another way. The experiment was most convincing when carried out as follows:—The spark-micrometer was adjusted for a fixed sparking distance of 2 mm. If the secondary circuit was in its original condition, and the primary circuit 1·5 metres long, sparks passed regularly. If a small capacity is added to the secondary circuit in the way already described, the sparks are completely extinguished; if the primary circuit is now lengthened to 2·6 metres they reappear; they are extinguished a second time if the capacity added to the secondary circuit is doubled; and by continuously increasing the capacity of the already lengthened primary circuit they can be made to appear and disappear again and again. The experiment shows us quite plainly that effective action is determined, not by the condition of either of the circuits, but by a proper relation (or harmony) between the two.

The length of the induced sparks increased considerably beyond the values given above when the two circuits were brought closer together. When the two circuits were at a distance of 7 cm. from one another and were adjusted to exact resonance, it was possible to obtain induced sparks 7 mm. long; in this case the electromotive forces induced in the secondary circuit were almost as great as those in the primary.

In the above experiments resonance was secured by altering the coefficient of self-induction and the capacity of the primary circuit, as well as the capacity of the secondary circuit. The following experiments show that an alteration of the coefficient of self-induction of the secondary circuit can also be used for this purpose. A series of rectangles *a b c d* (Fig. 9) were prepared in which the sides *a b* and *c d* were kept of the same length, but the sides *a c* and *b d* were made of wires varying in length from 10 cm. to 250 cm. A marked maximum of the sparking distance was apparent when the length of the rectangle was 1·8 metres. In order to get an idea of the quantitative relations I measured the longest sparks which appeared with various lengths of the secondary circuit. Fig. 10*a* shows the results.[1] Abscissæ represent the total length of the induced circuit and ordinates the maximum spark-length. The points indicate the observed values. Measurements of sparking distances are always very uncertain, but this uncertainty cannot be such as to vitiate the general nature of the result. In another set of experiments not only the lengths of the sides *a b* and *c d*, but also their distance apart (30 cm.), and their position were kept constant; but the sides *a c* and *b d* were formed of wires of gradually increasing length coiled into loose spirals. Fig. 10*b* shows the results obtained. The maximum here corresponds with a somewhat greater length of wire than before. Probably this is because the lengthening of the wire in this case increases only the coefficient of self-induction, whereas in the former case it increased the capacity as well.

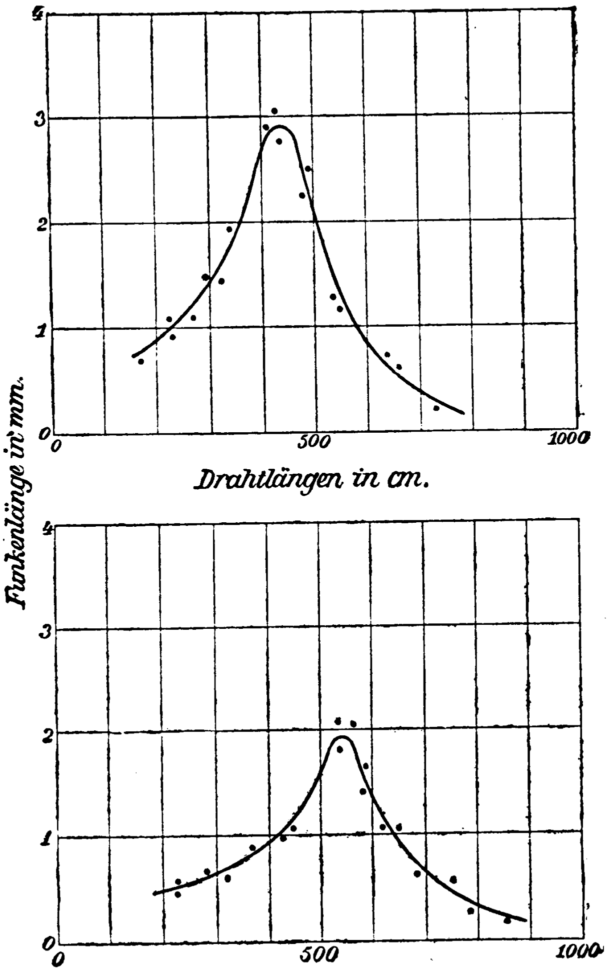

Fig. 10, *a* and *b*.

Some further experiments were made in order to determine whether any different result would be obtained by altering the resistance of the secondary circuit. With this intention the wire *c d* of the rectangle was replaced by various thin copper and German silver wires, so that the resistance of the secondary circuit was made about a hundred times as large. This change had very little effect on the sparking distance, and none at

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[1] [See Note 4 at end of book.]

all on the resonance; or, in other words, on the period of oscillation.

The effect of the presence of iron was also examined. The wire *c d* was in some experiments surrounded by an iron tube, in others replaced by an iron wire. Neither of these changes produced a perceptible effect in any sense. This again confirms the supposition that the magnetism of iron cannot follow such exceedingly rapid oscillations, and that its behaviour towards them is neutral. Unfortunately we possess no experimental knowledge as to how the oscillatory discharge of Leyden jars is affected by the presence of iron.

Nodes

The oscillations which we excited in the secondary circuit, and which were measured by the sparks in the micrometer, are not the only ones, but are the simplest possible in that circuit. While the potential at the ends oscillates backwards and forwards continually between two limits, it always retains the same mean value in the middle of the circuit. This middle point is therefore a node of the electric oscillation, and the oscillation has only this one node. Its existence can also be shown experimentally, and that in two ways. In the first place, it can be done by bringing a small insulated sphere near the wire. The mean value of the potential of the small sphere cannot differ appreciably from that of the neighbouring bit of wire. Sparking between the knob and the wire can therefore only arise through the potential of the neighbouring point of the system experiencing sufficiently large oscillations about the mean value. Hence there should be vigorous sparking at the ends of the system and none at all near the node. And this in fact is so, excepting, indeed, that when the nodal point is touched the sparks do not entirely disappear, but are only reduced to a minimum. A second way of showing the nodal point is clearer. Adjust the secondary circuit for resonance and draw the knobs of the micrometer so far apart that sparks can only pass by the assistance of the action of resonance. If any point of the system is now touched with a conductor of some capacity, we should in general expect that the resonance would be disturbed, and that the sparks would disappear; only at the node would there be no interference with the period of oscillation. Experiment confirms this. The middle of the wire can be touched with an insulated sphere, or with the hand, or can even be placed in metallic connection with the gas-pipes without affecting the sparks; similar interference at the side-branches or the poles causes the sparks to disappear.

After the possibility of fixing a nodal point was thus proved, it appeared to me to be worth while experimenting on the production of a vibration with two nodes. I proceeded as follows:—The straight primary conductor *C C′* and the rectilinear secondary *a b c d* were set up as in the earlier experiments and brought to resonance. An exactly similar rectangle *efgh* was then placed opposite to *a b c d* as shown in Fig. 11, and the neighbouring poles of both were joined (1 with 3 and 2 with 4). The whole system forms a closed metallic circuit, and the lowest or fundamental vibration possible in it has two nodes. Since the period of this vibration must very nearly agree with the period of either half, and therefore with the period of the primary conductor, it was supposed that vibrations would develop having two antinodes at the junctions 1-3 and 2-4, and two nodes at the middle points of *c d* and *g h*. These vibrations were always measured by the sparking distance between the knobs of the micrometer which formed the poles 1 and 2. The results of the experiment were as follows:—Contrary to what was expected, it was found that the sparking distance between 1 and 2 was considerably diminished by the addition of the rectangle *e f g h*. From about 3 mm. it fell to 1 mm. Never-

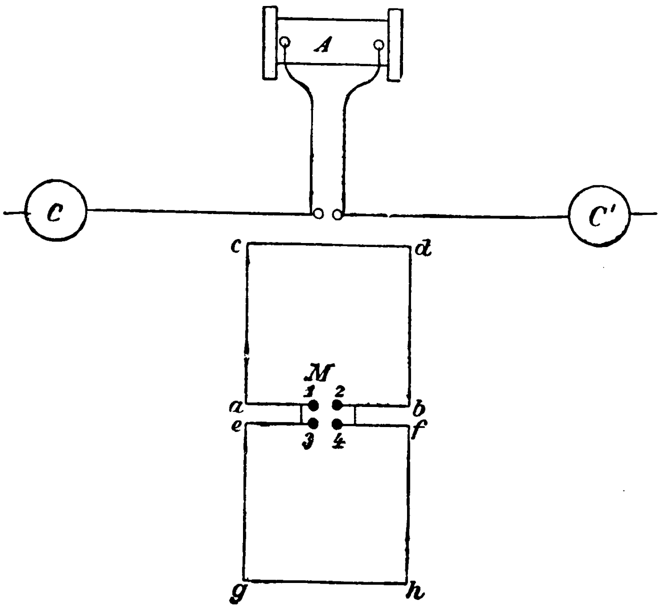

Fig. 11.

theless there was still resonance between the primary circuit and the secondary. For every alteration of *e f g h* reduced the sparking distance still further, and this whether the alteration was in the direction of lengthening or shortening the rectangle. Further, it was found that the two nodes which were expected were actually present. By holding a sphere near *c d* and *g h* only very weak sparks could be obtained as compared with those from *a e* and *b f*. And it could also be shown that these nodes belonged to the same vibration which, when strengthened by resonance, produced the sparks 1-2. For the sparking distance between 1 and 2 was not diminished by touching along *c d* or *g h,* but it was by touching at every other place.

The experiment may be modified by breaking one of the connections 1-3 or 2-4, say the latter. As the current-strength of the induced oscillation is always zero at these points, this cannot interfere much with the oscillation. And, in fact, after the connection has been broken, it can be shown as before that resonance takes place, and that the vibrations corresponding to this resonance have two nodes at the same places. Of course there was this difference, that the vibration with two nodes was no longer the deepest possible vibration; the vibration of longest period would be one with a single node between *a* and *e*, and having the highest potentials at the poles 2 and 4. And if we bring the knobs at these poles nearer together we find that there is feeble sparking between them. We may attribute these sparks to an excitation, even if only feeble, of the fundamental vibration; and this supposition is made almost a certainty by the following extension of the experiment:—We stop the sparks between 1 and 2 and direct our attention to the length of the sparks between 2 and 4, which measures the intensity of the fundamental vibration. We now increase the period of oscillation of the primary circuit by extending it to the full length and adding to its capacity. We observe that the sparks thus increase to a maximum length of several millimetres and then again become shorter. Clearly they are longest when the oscillation of the primary current agrees with the fundamental oscillation. And while the sparks between 2 and 4 are longest it can be easily shown that at this time only a single nodal point corresponds to these sparks. For only between *a* and *e* can the conductor be touched without interfering with the sparks, whereas touching the previous nodal points interrupts the stream of sparks. Hence it is in this way possible, in any given conductor, to make either the fundamental vibration or the first overtone preponderate.

Meanwhile, there are several further problems which I have not solved; amongst others, whether it is possible to establish the existence of oscillations with several nodes. The results already described were only obtained by careful attention to insignificant details; and so it appeared probable that the answers to further questions would turn out to be more or less ambiguous. The difficulties which present themselves arise partly from the nature of the methods of observation, and partly from the nature of the electric disturbances observed. Although these latter manifest themselves as undoubted oscillations, they do not exhibit the characteristics of perfectly regular oscillations. Their intensity varies considerably from one discharge to another, and from the comparative unimportance of the resonance-effects we conclude that the damping must be rapid; many secondary phenomena point to the superposition of irregular disturbances upon the regular oscillations, as, indeed, was to be expected from the complex nature of the system of conductors. If we wish to compare, in respect of their mathematical relations, our oscillations with any particular kind of acoustic oscillations, we must not choose the long-continued harmonic oscillations of uniform strength which are characteristic of tuning-forks and strings, but rather such as are produced by striking a wooden rod with a hammer, —oscillations which rapidly die away, and with which are mingled irregular disturbances.[1] And when we are dealing with oscillations of the latter class we are obliged, even in acoustics, to content ourselves with mere indications of resonance, formation of nodes, and similar phenomena.

For the sake of those who may wish to repeat the experiments and obtain the same results I must add one remark, the exact significance of which may not be clear at first. In all the experiments described the apparatus was set up in such a way that the spark of the induction-coil was visible from the place where the spark in the micrometer took place. When

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[1] [See Note 5 at end of book.]

this is not the case the phenomena are qualitatively the same, but the spark-lengths appear to be diminished. I have undertaken a special investigation of this phenomenon, and intend to publish the results in a separate paper.[1]

Theoretical

It is highly desirable that quantitative data respecting the oscillations should be obtained by experiment. But as there is at present no obvious way of doing this, we are obliged to have recourse to theory, in order to obtain at any rate some indication of the data. The theory of electric oscillations which has been developed by Sir W. Thomson, v. Helmholtz, and Kirchhoff has been verified as far as the oscillations of open induction-coils and oscillatory Leyden jar discharges are concerned;[2] we may therefore feel certain that the application of this theory to the present phenomena will give results which are correct, at least as far as the order of magnitude is concerned.

To begin with, the period of oscillation is the most important element. As an example to which calculation can be applied, let us determine the (simple or half) period of oscillation T of the primary conductor which we used in the resonance-experiments. Let P denote the coefficient of self-induction of this conductor in magnetic measure, expressed in centimetres; C the capacity of either of its ends in electrostatic measure (and therefore expressed also in centimetres); and finally A the velocity of light in centimetre/seconds. Then, assuming that the resistance is small, $\mathrm{T} = \pi\sqrt{\mathrm{PC}}/\mathrm{A}$. In our experiments the capacity of the ends of the conductor consisted mainly of the spheres attached to them. We shall therefore not be far wrong if we take C as being the radius of either of these spheres, or put $\mathrm{C} = 15$ cm.[3] As regards the coefficient of self-induction P, it was that of a straight wire, of diameter $d = 0{\cdot}5$ cm., and of which the length L was 150 cm. when resonance occurred. Calculated by Neumann's formula $\mathrm{P} = \iint \cos\epsilon/r\, ds\, ds'$, the value of P for such a wire is

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[1] [See IV., p. 63.]

[2] Lorenz, *Wied. Ann.* 7, p. 161, 1879.

[3] [See Note 6 at end of book.]

$2L\{\log \text{nat}\,(4L/d) - 0{\cdot}75\}$ and therefore in our experiments $P = 1902$ cm.

At the same time we know that it is not certain whether Neumann's formula is applicable to open circuits. The most general formula, as given by v. Helmholtz, contains an undetermined constant k, and this formula is in accordance with the known experimental data. Calculated according to the general formula, we get for a straight cylindrical wire of length L and diameter d the value $P = 2L\{\log \text{nat}\,(4L/d) - 0{\cdot}75 + \frac{1}{2}(1-k)\}$. If in this we put $k = 1$, we arrive at Neumann's value. If we put $k = 0$, or $k = -1$, we obtain values which correspond to Maxwell's theory or Weber's theory. If we assume that one at any rate of these values is the correct one, and therefore exclude the assumption that it may have a very large negative or positive value, then the true value of k is not of much moment. For the coefficients calculated with these various values of k differ from each other by less than one-sixth of their value; and so if the coefficient 1902 does not exactly correspond to a length of wire of 150 cm., it does correspond to a length of our primary conductor not differing greatly therefrom. From the values of P and C it follows that the length $\pi\sqrt{CP}$ is 531 cm. This is the distance through which light travels in the time of an oscillation, and is at the same time the wave-length of the electromagnetic waves which, according to Maxwell's view, are supposed to be the external effect of the oscillations. From this length it follows that the period of oscillation itself (T) is 1·77 hundred-millionths of a second; thus the statement which we made in the beginning as to the order of magnitude of the period is justified.

Let us now turn our attention to what the theory can tell us as to the ratio of damping of the oscillations. In order that oscillations may be possible in the open circuit, its resistance must be less than $2A\sqrt{P/C}$. For our primary conductor $\sqrt{P/C} = 11{\cdot}25$: now since the velocity A is equal to 30 earth-quadrant/seconds, or to 30 ohms, it follows that the limit for r admissible in our experiment is 676 ohms. It is very probable that the true resistance of a powerful discharge lies below this limit, and thus from the theoretical point of view there is no contradiction of our assumption of oscillatory motion. If the actual value of the resistance lies somewhat below this limit, the amplitude of any one oscillation would bear to the amplitude of that immediately following the ratio of 1 to $e^{-(r\mathrm{T}/2\mathrm{P})}$. The number of oscillations required to reduce the amplitude in the ratio of 2·71 to 1 is therefore equal to $2\mathrm{P}/r\mathrm{T}$ or $2\mathrm{A}\sqrt{\mathrm{P}/\mathrm{C}}/\pi r$. It therefore bears to 1 the same ratio that $1/\pi$ of the calculated limiting value bears to the actual value of the resistance, or the same ratio as 215 ohms to r. Unfortunately we have no means of even approximately estimating the resistance of a spark-gap. Perhaps we may regard it as certain that this resistance amounts to at least a few ohms, for even the resistance of strong electric arcs does not fall below this. It would follow from this that the number of oscillations we have to consider should be counted by tens and not by hundreds or thousands.[1] This is in complete accordance with the character of the phenomena, as has already been pointed out at the end of the preceding section. It is also in accordance with the behaviour of the very similar oscillatory discharges of Leyden jars, in which case the oscillations of perceptible strength are similarly limited to a very small number.

In the case of purely metallic secondary circuits the conditions are quite different from those of the primary currents to which we have confined our attention. In the former a disturbance would, according to theory, only come to rest after thousands of oscillations. There is no good reason for doubting the correctness of this result; but a more complete theory would certainly have to take into consideration the reaction upon the primary conductor, and would thus probably arrive at higher values for the damping of the secondary conductor as well.

Finally, we may raise the question whether the induction-effects of the oscillations which we have observed were of the same order as those which theory would lead us to expect, or whether there is here any appearance of contradiction between the phenomena themselves and our interpretation of them. We may answer the question by the following considerations:—We observe, in the first place, that the maximum value of the electromotive force which the oscillation induces in its own

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[1] [See Note 7 at end of book.]

circuit must be very nearly equal to the maximum difference of potential at the ends, for if the oscillations were not damped, there would exist complete equality between the two magnitudes; inasmuch as the potential difference of the ends and the electromotive force of induction would in that case be in equilibrium at every instant. Now in our experiments the potential difference between the ends was of a magnitude corresponding to a sparking distance of 7-8 mm., and any such sparking distance fixes the value of the greatest inductive effect of the oscillation in its own path. We observe, in the second place, that at every instant the induced electromotive force in the secondary circuit bears to that induced in the primary circuit the same ratio as the coefficient of mutual induction p between the primary and secondary circuits bears to the coefficient of self-induction P of the primary circuit. There is no difficulty in calculating according to known formulæ the approximate value of p for our resonance-experiments. It was found to vary in the different experiments between one-ninth and one-twelfth of P. From this we may conclude that the maximum electromotive force which our oscillation excites in the secondary circuit should be of such strength as to give rise to sparks of $^1/_2$ to $^2/_3$ mm. in length. And accordingly the theory allows us, on the one hand, to expect visible sparks in the secondary circuit under all circumstances, and, on the other hand, we see that we can only explain sparks of several millimetres in length by assuming that several successive inductive effects strengthen each other. Thus from the theoretical side as well we are compelled to regard the phenomena which we have observed as being the results of resonance.

Further application of theory to these phenomena can only be of service when we shall have succeeded by some means in determining the period of oscillation directly. Such measurement would not only confirm the theory but would lead to an extension of it. The purpose of the present research is simply to show that even in short metallic conductors oscillations can be induced, and to indicate in what manner the oscillations which are natural to them can be excited.