X

ON THE PROPAGATION OF ELECTRIC WAVES BY MEANS OF WIRES

(*Wiedemann's Ann.* 37, p. 395, 1889.)

When a constant electric current flows along a cylindrical wire, its strength is the same at every part of the section of the wire. But if the current is variable, self-induction produces a deviation from this most simple distribution. For the central parts of the wire are, on the whole, less distant from all the rest than are the outer parts; hence induction opposes variations of the current in the centre of the wire more strongly than at the circumference, and consequently the current by preference flows along the outer portion of the wire. When the current changes its direction a few hundred times per second, the deviation from the normal distribution can no longer be imperceptible. This deviation increases rapidly with the rate of alternation; and when the current alternates many million times per second, almost the whole of the interior of the wire must, according to theory, appear free from current, and the flow must confine itself to the very skin of the wire. Now in such extreme cases the above conception of what takes place is obviously not free from physical difficulties; and preference must be given to another conception of the matter which was first presented by Messrs. O. Heaviside[1] and J. H. Poynting,[2] as the correct interpretation of Maxwell's equations as applied to this case. According to this view, the electric force which determines the current is not propagated

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[1] O. Heaviside, Electrician, January 1885, *Phil. Mag.* 25, p. 153, 1888.

[2] J. H. Poynting, *Phil. Trans.* 2, p. 277, 1885.

in the wire itself, but under all circumstances penetrates from without into the wire, and spreads into the metal with comparative slowness and according to laws similar to those which govern changes of temperature in a conducting body. Thus when the forces around the wire continually alter their direction, the action of these forces only extends to a very slight depth within the metal; the slower the fluctuations, the more deeply will the action penetrate; and lastly, when the changes succeed each other with infinite slowness, the force has time to penetrate into the interior of the wire and to act with uniform strength throughout it.[1]

Whatever conception we may form from the theoretical results, it is important to find out whether these latter agree with the actual facts. Inasmuch as I made use of electric waves in wires of exceedingly short period in my experiments on the propagation of electric force, it was natural to test by means of these the correctness of the conclusions deduced. As a matter of fact the theory was found to be confirmed by the experiments which are now to be described; and it will be seen that these few experiments are amply sufficient to support the conception introduced by Messrs. Heaviside and Poynting. Similar experiments, with similar results, have been carried out by Dr. O. J. Lodge,[2] who has, however, used quite different experimental methods, and mainly with the object of elucidating the theory of lightning-conductors. To what extent the conclusions are true which were deduced by Dr. Lodge in the latter respect from his experiments must, in the first place, depend upon the actual rapidity of succession of the changes of electrical conditions which accompany lightning.

The apparatus and methods which are here mentioned are those which have been fully described in my previous papers. The waves used were such as had in wires nodes about 3 metres apart.

1. When a primary conductor acts through air upon a secondary conductor, there can be no doubt that the action penetrates from without into the latter. For it may be regarded as an established fact that in air the action is propagated from point to point, and it must therefore first meet

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[1] [See Note 24 at end of book.]

[2] O. J. Lodge, *Journ. of Soc. of Arts*, May 1888; *Phil. Mag.* **26**, p. 217, 1888.

the outer boundary of the conductor before it can act upon the inside. Now it can be shown that a closed metallic envelope does not allow the action to pass through it at all. If we place the secondary conductor in a favourable position with reference to the primary so that sparks 5-6 mm. long are obtained, and then surround it with a closed box of sheet zinc, not the slightest amount of sparking can be detected. Similarly the sparks disappear when the primary conductor is completely surrounded by a metal box. It is known that a metal screen does not interfere with the integral force of induction when the fluctuations of current are relatively slow. At first sight this appears to contradict the above experimental results. But the contradiction is only apparent and disappears when the time-relations are considered. In a similar way a badly-conducting envelope protects its interior completely against rapid fluctuations of external temperature, less completely against slow fluctuations, and not at all against a permanent rise or fall in temperature. The thinner the envelope the more rapid are the fluctuations which can act through it upon the interior. And so in our case also, the electric action should clearly penetrate into the interior if we only reduced sufficiently the thickness of the metal. Yet I did not find it easy to secure the requisite thinness. A box covered with tinfoil acted as a perfect screen; and so too did a box of gilt paper when care was taken to make good contact between the edges of the separate pieces of paper. In this case the thickness of the conducting metal could scarcely be estimated as high as $\frac{1}{20}$ mm. I next fitted the protecting envelope as closely as possible around the secondary conductor. For this purpose its spark-gap was drawn out to about 20 mm.; and, in order to be still able to detect electric disturbances in it, an auxiliary spark-gap was introduced just opposite the usual one. The sparks in this were not so long as in the proper spark-gap because the resonance-effect was now absent, but they still were quite vigorous. After being thus prepared the conductor was completely surrounded with a tube-shaped conducting envelope made as thin as possible; this did not touch the conductor, but was brought as close as possible to it, and in the neighbourhood of the auxiliary spark-gap—in order to be able to make use of the latter—was made of wire-gauze.

Between the poles of this envelope the sparking was as vigorous as it had previously been in the secondary conductor itself; but in the enclosed conductor not the slightest electrical disturbance could be perceived. It does not interfere with the result if the envelope touches the conductor at a few points; it is not necessary to insulate the two from one another in order to make the experiment succeed, but only in order to give it its demonstrative force. In imagination we can clearly draw the envelope around the conductor more closely than is possible in practice; indeed, we can imagine it to coincide with the outer skin of the conductor. Thus although the electrical disturbances at the surface of our conductor are so powerful as to produce sparks of 5-6 mm. length, yet at a depth of only $\frac{1}{20}$ mm. below its surface there is such complete calm that not the slightest sparking is produced. We are thus led to suspect that what we call an induced current in the secondary conductor is a process which takes place for the most part in the surrounding space and in which the inside of the conductor scarcely plays any part.

2. We might admit that this is so when an electrical disturbance passes through a dielectric, but yet maintain that it is otherwise when the disturbance, as we usually say, has been propagated in a conductor. Near one of the end plates of our primary conductor let us place a conducting plate and fasten to it a long straight wire; in our earlier experiments we have already shown how the action of the primary oscillation can be conveyed to great distances with the aid of such a wire. The usual view of this is that the wave proceeds through the wire. We will, however, try to show that all the changes are confined to the surface and the space outside, and that the interior of the wire knows nothing of the passing waves. I first arranged experiments in the following manner. From the conducting wire a piece 4 metres long was removed and replaced by two strips of zinc sheet 4 metres long and 10 cm. broad, which were laid flat one upon the other with their ends touching and firmly connected. Along the whole length of the middle line between the strips, and hence almost completely surrounded by metal, was placed a copper wire 4 metres long and covered with gutta-percha. It made no difference in the experiments whether the outer ends of this wire were in metallic connection with

the strips, or insulated from them; but generally the ends were soldered to the zinc strips. The copper wire was cut in the middle, and its ends were twisted round each other and led out between the strips to a small spark-gap by which any electrical disturbance in the wire could be perceived. Not the slightest action could be detected at the spark-gap, even when the strongest possible waves were led through the whole arrangement. But if any part of the copper wire, a few decimetres long, was pulled out of its place so as to project but a little beyond the strips, sparking immediately began. The longer the projecting part and the further it extended beyond the edge of the strips, the more vigorous became the sparking. The absence of sparks in the first instance cannot be attributed to any unfavourable conditions of resistance; no change has taken place in these conditions; only the wire at first was inside a conducting mass and beyond the reach of outside influences. Indeed, it is only necessary to enclose the projecting part of the wire with a little tinfoil in metallic connection with the zinc strips, in order to stop the sparking at once. By so doing we virtually bring the copper wire back inside the conductor. In like manner the sparks become weaker if we carry another wire, in a somewhat larger arc, around the part of the gutta-percha wire which projects beyond the strips; the second wire cuts off from the first part of the external effect. Indeed, we may say that the edge of the zinc strip itself in a similar way cuts off from the middle of the strip some of the induction. For if we now remove one of the two zinc strips and simply let the gutta-percha wire rest upon the other, we always perceive sparks in the wire; but these are extremely feeble in the middle of the strip, and much more powerful towards the edge. Just as electricity when distributed by electrostatic induction would tend to accumulate on the sharp edge of the strip, so here the current seems by preference to move along the edge. In both cases we may say that the outer parts protect the inner from an influence exerted from the outside.

Equally convincing, and somewhat neater, are the following experiments:—I inserted in the conductor, along which the waves were transmitted, a very thick copper wire 1·5 metre long, whose ends carried two circular metallic disks 15 cm.

in diameter. The wire passed through the centres of the disks whose planes were perpendicular to it. Around the edge of each disk were twenty-four equidistant holes. A spark-gap was inserted in the wire. When the waves traversed the wire they produced sparks up to 6 mm. in length. A thin copper wire was now stretched across between two corresponding holes in the disks. The sparking distance thereupon sank to 3·2 mm. No other change was produced when the thin wire was replaced by a thick one, or when twenty-four wires were used instead of the single one, provided they were drawn all together through the same pair of holes. But it was otherwise when the wires were distributed around the edge of the disks. When a second wire was added opposite the first one the spark-length sank to 1·2 mm. When two more wires were put in midway between the others, the spark-length went as low as 0·5 mm. The insertion of four more wires in intermediate positions left sparks barely 0·1 mm. long; and after all twenty-four wires were inserted at equal distances no sparking whatever could be perceived inside. Yet the resistance of the inner wire was much smaller than the joint resistance of all the outer wires; and furthermore, we have given a special proof that the resistance is of no consequence. At the side of the tube of wires which has been built up let us place as a shunt a conductor precisely similar to the one inside the tube; we see vigorous sparking in the former, but none whatever in the latter. The former is not screened, the latter is screened by the tube of wires. We have here an electromagnetic analogue to the electrostatic experiment known under the name of the electric bird-cage.

Again I altered the experiment in the way indicated in Fig. 33. The two disks were moved nearer together so that,

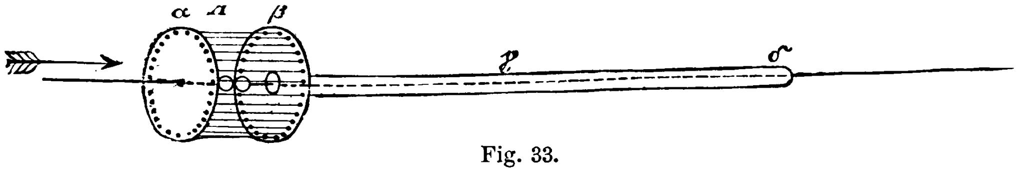

Fig. 33.

with the wires stretched between them, they formed a wire-cage *A* just big enough to contain the spark-micrometer. One of the disks (α) remained in metallic connection with the central wire; the other (β) was insulated from it by cutting out a round hole, and was instead connected with a conducting tube γ which, without touching the central wire, completely surrounded it for a distance of 1·5 metre. The free end δ of the tube was then placed in metallic connection with the central wire. The wire with its spark-gap still lies in a space surrounded by a metallic screen; and it seems to follow naturally after what has already been stated that, whether the waves be led through the arrangement in the one direction or the other, not the slightest electrical disturbance would be detected in the wire. So far, then, this arrangement offers nothing new; but it has this advantage over the preceding one, that we can replace the protecting metal tube γ by others with thinner and thinner walls, and so find out what thickness of wall is just sufficient to stop off the outside action. Very thin brass tubes—tubes of tinfoil and tubes of Dutch metal—acted as perfect screens. I next took glass tubes which had been chemically silvered, and now found it quite easy to prepare tubes so thin that, in spite of their protection, there was vigorous sparking in the central wire. But the sparks only appeared when the film of silver was so thin that it was no longer quite opaque to light, and was certainly thinner than $\frac{1}{1000}$ mm. In imagination, though not in practice, we may draw the protecting envelope more and more closely around the wire, until at last it coincides with its surface; and we may feel certain that nothing would practically be altered thereby. So, however vigorously the waves may really play about the wire, inside it is perfectly calm; the action of the waves scarcely penetrates further into the wire than does the light which is reflected from its surface. Hence we should expect to find the real seat of these waves in the neighbourhood of the wire, and not in the wire itself; and instead of saying that our waves are propagated in the wire, we should rather say that they glide towards and along the wire.

Instead of inserting the arrangement last described in the conducting wire in which waves were indirectly produced, we can insert it in one of the branches of the primary conductor itself. In such experiments I obtained much the same results as in the previous ones. Hence it must be equally true of our primary oscillation, that its seat is not to be found in the interior of the conductor; only the outer skin of the conductor, about which it plays, takes part in it.[1]

One further item of information may be added to what we have already learned about waves in wires, and this relates to the method of carrying out the experiments. If our waves have their seat in the space surrounding the wire, then a wave gliding along a single wire will not be propagated through the air alone; but, inasmuch as its action extends to a considerable distance, it will be propagated in the neighbouring walls, the floor, etc., and so will develop into a complicated phenomenon. But if we set up in exactly the same way two auxiliary plates opposite the two poles of our primary conductor, connect a wire to each of them, and lead both wires straight and parallel to one another to the same distance, then the action of the waves makes itself felt only in the neighbourhood of the space between the two wires. Hence it is only in the space between the wires that the wave progresses. We can thus take measures to secure that the propagation occurs only through air or another insulator, and by so arranging matters can experiment more conveniently and with less fear of complications. The wave-lengths thus obtained are, however, approximately the same as those obtained with single wires; so that even with the latter the disturbing effects do not seem to be of much importance.

3. From what has already been stated, we may conclude that rapid electric oscillations are quite incapable of penetrating metallic layers of any thickness, and that it is therefore quite impossible to excite sparks by the aid of such oscillations inside closed metallic envelopes. Hence, if we see sparks induced by such oscillations inside metallic envelopes which are nearly, but not quite, closed, we must conclude that the electric disturbance has penetrated through the existing openings. And this mode of conception is the correct one; but in some cases it contradicts the usual view so completely that special experiments are required to induce us to forsake the usual view for the newer one. We shall select a striking case of this kind; and by making certain of the correctness of our

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[1] The calculation of the self-induction of such conductors on the assumption of uniform current-density in the interior must therefore lead to totally unreliable results. It is surprising that the results obtained under such erroneous assumptions should yet appear to agree approximately with the truth.

view in this case, we shall show its probability in all other cases. We return to the arrangement described in the previous section, and represented in Fig. 33, only we no longer connect the protecting tube at δ with the central wire. We now send the train of waves through the arrangement in the direction from A towards δ. We obtain brilliant sparks at A, and these are about as strong as if we had inserted the spark-gap in the conducting wire without any protection. Nor do the sparks become much smaller if, without altering anything else, we lengthen the tube γ considerably—up to about 4 metres. According to the usual view, it would be said that the wave on reaching A easily passes through the thin metal disk a, which is a good conductor, then springs across the spark-gap at A and proceeds along the central wire. According to our conception, on the other hand, we ought to describe what happens as follows :—The wave on reaching A is absolutely unable to pass through the metal disk, so it glides along the disk over the outside of the apparatus, travelling along thus until it reaches the point δ, 4 metres off. Here it divides—one part, which at present does not concern us, immediately proceeds straight along the wire; another part bends round into the inside of the tube and runs back the whole 4 metres in the air-space between the tube and the central wire, until it reaches the spark-gap A, where it now produces sparks. We shall show by the following experiments that our conception, although somewhat complicated, is yet the correct one. In the first place, every trace of sparking at A disappears as soon as we close the opening at δ, even if it be only with a tinfoil cap. Our waves have a wave-length of only 3 metres; before their action has reached the point δ, it has gone back to zero at A, and has changed sign. What influence then could the closing of the distant opening at δ exert upon the spark at A, if the latter really appears as soon as the wave passes through the metallic partition ? In the second place, the sparks disappear when we make the central wire end inside the tube γ, or at the open end δ of this tube; they reappear when the end of the wire is allowed to project beyond the opening, even if only for 20-30 cm. What influence could this insignificant lengthening of the wire have upon the spark at A, unless the projecting end of the wire were just the means by which a part of the wave is intercepted and brought through the opening δ into the interior? Let us, in the third place, introduce a second spark-gap B in the central wire between A and δ, and surround it with a wire cage just like A. When we place the poles in B so far apart that sparks can no longer pass, we find that it is no longer possible to obtain any perceptible sparks in A. But if, in like manner, we hinder the passage of the sparks in A, we find that this has scarcely any influence upon the sparks in B. Hence, for the passage of the sparks in A, it is requisite that they should first pass in B; but for the passage of sparks in B, it is not necessary that they should first pass in A. Hence the direction of propagation in the interior is from B towards A, not from A towards B.

Moreover, we can adduce other and more convincing proofs. By making the spark-gap either vanishingly small or very great, we may prevent the wave returning from δ towards A from expending its energy in the formation of sparks. In this case the wave will be reflected at A, and will again proceed from A towards δ. But in doing so it must combine with the direct waves to form stationary oscillations with nodes and antinodes. If we succeed in showing the presence of these, we can no longer have any doubt as to the correctness of our conception. For this purpose we must necessarily give our apparatus somewhat different dimensions, so as to be able to introduce electric resonators inside it. I therefore carried the central wire through the axis of a cylindrical tube 5 metres long and 30 cm. in diameter. This tube was not made of solid metal, but was built up of twenty-four copper wires; these were stretched parallel to one another along the generating surface over seven equidistant circular rings of stout wire, as indicated in Fig. 34. The resonator to be used I made as follows:—Copper wire 1 mm. thick was coiled tightly into a spiral of 1 cm. diameter. About 125 turns of this were taken, pulled out a little, and bent into a circle of 12 cm. diameter; between the free ends was inserted an

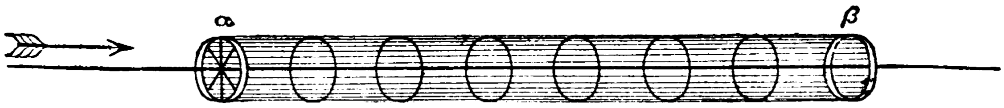

Fig. 34.

adjustable spark-gap. Special experiments had shown that this circle was in resonance with the waves of 3 metres length in the wire, and yet it was sufficiently small to be introduced between the central wire and the tube. Both ends of the tube were at first left open, and the resonator was held inside in such a way that its plane included the central wire, and the spark-gap was not turned exactly inwards or outwards, but faced towards one or other end of the tube; vigorous sparks, $\frac{1}{2}$-1 mm. long, appeared at the spark-gap. If now both ends of the tube were closed by four wires arranged crosswise and connected with the central conductor, not the slightest sparking could be discovered inside, which proves that the network of the tube is sufficiently close for our experiments. In the next place, the cross-wires on the β side of the tube (*i.e.* the side remote from the origin of the waves) were removed. No sparks could be detected when the resonator was in the immediate neighbourhood of the remaining cross-wires, *i.e.* in the position a, which corresponds to the spark-gap A of our earlier experiments. But when it was moved from this position towards β the sparks reappeared, became very vigorous at a distance of 1·5 metre from a, decreased again and almost completely disappeared at a distance of 3 metres, and again became stronger towards the end of the tube. Our supposition is therefore confirmed. It is right that there should be a node at the closed end, for at the metallic connection between the central wire and the tube the electric force between both must necessarily be zero. It is otherwise if we cut the central conductor at this point, quite near the cross-wires, leaving a gap a few centimetres long. In this case the wave is reflected with the opposite phase, and we should expect an antinode at a. And, in fact, we do now find vigorous sparks in the resonator; these, however, rapidly become smaller as we move from a towards β, disappear almost entirely at a distance of 1·5 metre, become vigorous again at a distance of 3 metres, and finally give plain indications of a second node at a distance of 4·5 metres, *i.e.* 0·5 metre from the open end of the tube. The nodes and antinodes which we have described lie at fixed distances from the closed end, and alter their position as this does; but otherwise they are quite independent of processes going on outside the tube, *e.g.* of the nodes and antinodes which we may produce there. The phenomena occur in precisely the same way when we allow the waves to traverse the tube in the opposite direction, *i.e.* from the open to the closed end; but this case is less interesting, because the mode of propagation of the waves here differs less from the usual conception than in the case which we have just discussed. If we leave the central wire uncut, and both ends of the tube open, and produce in the whole system stationary waves, with nodes and antinodes, we always find a node inside the tube corresponding to each node outside, which proves that the rate of propagation is approximately the same inside and outside.

On studying the experiments above described, the mode in which we have interpreted them, and the explanations of the investigators referred to in the introduction, one difference will be found especially striking between the conception here advocated and the usually accepted view. In the latter conductors appear as the only bodies which take part in the propagation of electrical disturbances—non-conductors as bodies which oppose this propagation. According to our conception, on the other hand, all propagation of electrical disturbances takes place through non-conductors; and conductors oppose this propagation with a resistance which, in the case of rapid alternations, is insuperable. We might almost feel inclined to agree to the statement that conductors and non-conductors should, according to this conception, have their names interchanged. Such a paradox, however, only arises because we omit to specify what conduction or non-conduction is under discussion. Undoubtedly metals are non-conductors for electric force, and for this very reason they, under certain conditions, restrain it from becoming dissipated, and compel it to remain concentrated; they thus become conductors of the apparent source of these forces—the electricity—to which the usual terminology has reference.