SUPPLEMENTARY NOTES

(1891)

1. [II. p. 29.]

It was v. Helmholtz, in his paper *Ueber die Erhaltung der Kraft*, who first stated (in 1847) that the discharge of a Leyden jar is oscillatory. He arrived at this conclusion from its varying and opposite magnetic effects, and from the fact that when one endeavours to decompose water by electric discharges, both gases are developed at both electrodes. Sir William Thomson arrived independently at the same result from theoretical considerations. The mathematical treatment of the problem given by him in the year 1853 (*Phil. Mag.* (4) **5**, p. 393) still holds good to-day. We may further mention the following among the more important early papers on the subject:—

On the discharge of Leyden jars—

Feddersen, *Pogg. Ann.* **103**, p. 69, 1858; **108**, p. 497, 1859; **112**, p. 452, 1861; **113**, p. 437, 1861; **115**, p. 336, 1862; **116**, p. 132, 1862.

Paalzow, *Pogg. Ann.* **112**, pp. 567, 1861; **118**, pp. 178, 357, 1863.

v. Oettingen, *Pogg. Ann.* **115**, p. 513, 1862; *Jubelbd.* p. 269, 1874.

G. Kirchhoff, *Pogg. Ann.* **121**, p. 551, 1864; *Ges. Abhandl.* p. 168.

L. Lorenz, *Wied. Ann.* **7**, p. 161, 1879.

On the oscillations of open induction-circuits—

Helmholtz, *Pogg. Ann.* **83**, p. 505, 1851; *Ges. Abhandl.* **1**, 429. The theory is implicitly contained in this, but is not explicitly applied to the special case of oscillations.

v. Helmholtz, *Ges. Abhandl.* **1**, p. 531 (1869).

Bernstein, *Pogg. Ann.* **142**, p. 54, 1871.

Schiller, *Pogg. Ann.* **152**, p. 535, 1872.

2. [II. p. 34.]

At first I insulated carefully with sealing-wax, etc. But I always found that, for all such experiments as are here considered, the insulation afforded by dry wood is amply sufficient. In the subsequent experiments no other means of insulation was used.

3. [II. p. 39.]

I expect that the action of the induction-coil partly depends upon the fact that directly before the discharge it allows the potential to rise very rapidly. Several accessory phenomena lead me to believe that when this rapid rise takes place, the difference of potential is forced beyond the point at which sparking occurs when the difference of potential increases slowly; and that for this reason the discharge takes place more suddenly and energetically than when a statical charge is discharged.

4. [II. p. 45.]

These curves should be compared with the corresponding resonance-curves which Herr V. Bjerknes has obtained by more accurate experimental measurements (*Wied. Ann.* **44**, p. 74, 1891).

5. [II. p. 49.]

This remark in my first paper shows clearly that I never conceived the oscillations of my primary conductor as perfectly regular and long-continued sine-oscillations. The value of the damping has recently been carefully determined by Herr V. Bjerknes (*Wied.*

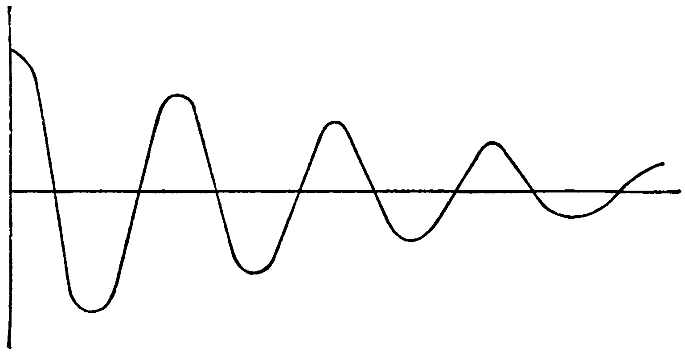

Fig. 40.

Ann. **44**, p. 74, 1891). Fig. 40 shows, in accordance with the results of his experiments, the kind of oscillation given by a conductor similar to our primary conductor.

6. [II. p. 50.]

Just at this point there has crept into the calculation a fatal mistake, the unfortunate effects of which extend even to some of the subsequent papers.

The capacity C in the formula $T = \pi\sqrt{PC}/A$ denotes the amount of electricity which exists at one end of an oscillating conductor when the difference of potential between the two ends is equal to unity. Now if these two ends consist of two spheres which are far apart from each other, and if their difference of potential is equal to unity, then the difference of potential between each of them and the surrounding space is equal to $\pm {}^1/_2$. Therefore the charge upon each of the spheres, measured in absolute units, is found by dividing its capacity, *i.e.* its radius measured in centimetres, by 2. Hence we should here put $C = \frac{15}{2}$ cm., and not $C = 15$ cm. The period of oscillation, T, now becomes smaller in the proportion of $1 : \sqrt{2}$, so that T is now equal to 1·26 hundred-millionths of a second.

M. H. Poincaré, as already stated in the introduction, first drew attention to this error (*Comptes Rendus*, **111**, p. 322, 1891).

7. [II. p. 52.]

The result is about right, but the way in which it is deduced is not sound. We have just referred [6] to an error in the calculation which would have to be corrected; and beside this no account is taken of damping through radiation. Indeed, I had not thought of this when writing the paper.

8. [IV. p. 67.]

The complications here mentioned, and the starting of long sparks by other much shorter ones, refer to the following phenomena:—Let the primary coils of two induction-coils be placed in the same circuit, and let their spark-gaps be so adjusted as to be just on the point of sparking. Any cause which starts sparking in one of them will now make the other begin to spark as well; and this quite independently of the mutual action of the light emitted by the two sparks—which, indeed, can easily be excluded. Sparking begins either in both, or else in neither of them. Again, let a Töpler-Holtz induction-machine, with a disc 40 cm. in diameter, be turned rapidly so as to give sparks having a maximum length of about 15 cm. Now draw the poles 20-25 cm. apart, so that the sparking entirely stops; it will now be found that a long crackling spark can again be regularly obtained every time a small spark is drawn from the negative conductor, either with the knuckle of the hand or with the knob of a Leyden jar; or the negative pole may be connected to a long conductor, and sparks may be drawn from this with the same result. The "releasing" spark may be quite short and weak; if it is drawn with the knob of a Leyden jar, the jar only appears slightly charged. The same effect cannot be obtained by drawing sparks from the positive pole. The phenomenon must have been often observed before; but I have not found any mention of it in the literature on the subject.

I can give no explanation of these phenomena. They clearly have the same origin as the phenomena which Herr G. Jaumann has described in his paper entitled "Einfluss rascher Potentialveränderungen auf den Entladungsvorgang" (*Sitzungsberichte d. Akad. d. Wissensch. zu Wien.*, Bd. **97**, Abth. II*a*. July 1888). Herr Jaumann arrives at the conclusion that "not only the form, condition, and potential difference of the discharge-field," but also "the manner in which the potential difference alters, and probably its rate of alteration, materially influence the discharge." It is to be hoped that these phenomena will be further explained.

9. [IV. p. 73.]

Soon afterwards Herren E. Wiedemann and H. Ebert showed that the action of the light only affects the negative pole, and only the surface of it (*Wied. Ann.* **33**, p. 241, 1888).

10. [IV. p. 76.]

Somewhat later I succeeded in this. I had hoped to observe an influence of the state of polarisation of the light upon the action, but was not able to detect anything of the kind.

11. [IV. p. 79.]

By this I did not mean to say that I had not succeeded in observing the action of light upon discharges other than those of induction-coils; but only that I had not succeeded in replacing spark-discharges—the nature of which is so little understood—by simpler means. This was first done by Herr Hallwachs (*Wied. Ann.* **33**, p. 301, 1888). The simplest effect that I obtained was with the glow-discharge from 1000 small Planté accumulators between brass knobs in free air; by the action of light I was able to make the glow-discharge pass when the knobs were so far apart that it could not spring across without the aid of the light.

12. [VII. p. 109.]

The 12 metres are supposed to be measured in the direction of the base-line. The space on each side of the base-line was clear up to a distance of 3-4 metres, with the exception of an iron stove which came within 1·5 metres of it. I did not think at the time that at this distance it could interfere at all.

13. [VII. p. 109.]

In this calculation as well the capacity is assumed to be that of an end-plate, supposed to be hanging free in air; this capacity was experimentally determined by comparison with the sphere previously used. For the reasons stated in Note 6, only the half of this capacity should have been taken. Hence the period of oscillation, as correctly calculated, is smaller than the value given in the proportion of $1 : \sqrt{2}$. Thus the correct value of the period of oscillation is almost exactly one hundred-millionth of a second.

14. [VII. p. 112.]

Here, as well as in all that follows, it is to be understood that, in order to produce stationary waves in wires, not only must the primary and secondary conductors be brought into resonance, but the straight stretched wires must also be tuned to unison with both of these. Only in this case does the whole length of the wire divide itself clearly into half wave-lengths, and only in this case is this beautiful phenomenon exhibited in its full development. This condition seems to have escaped the attention of some observers who have repeated the experiments on waves in wires.

15. [VII. p. 113.]

This has not turned out to be true. In tubes of about 2 cm. diameter, filled with dilute sulphuric acid, the waves travel quite well and with the same velocity as in wires. Herr E. Cohn has, moreover, shown that the inertia of the electrolytes cannot come into play when the period of oscillation is of the order here employed (*Wied. Ann.* **38,** p. 217). The fact that these oscillations are transmitted through electrolytes has been used by J. J. Thomson for the purpose of determining their resistance (*Proc. Roy. Soc.* **45,** p. 269).

16. [VII. p. 114.]

The correctly calculated period of oscillation is one hundred-millionth of a second. This, with a wave-length of 2·8 metres, gives a velocity of 280,000 km. per second, or approximately the velocity of light.

This is the final form,—although, of course, with much more careful data,—which Messrs. E. Lecher (*Wied. Ann.* **41,** p. 850) and Blondlot (*C. R.* **113,** p. 628) have adopted for showing that the velocity of waves in wires is the same as the velocity of light. As a matter of fact, however, this final form only shows the accordance of theory and observation in the following respect: that in a simple straight wire 2·8 metres long, and in a conductor of the form of our primary conductor, the periods of oscillation are equal. But the absolute value of the period of oscillation, and hence the velocity, might on that account differ by the same amount in both cases from the theoretical value; and it must differ by the same amount if the same causes produce equal retardations in both conductors.

Hence this final form cannot be employed for the purpose of removing doubts as to the existence of such a retardation.

The velocity assumed in the text depends much more upon the experiments of Fizeau and Gounelle and Siemens, than upon the calculation.

17. [VII. p. 118.]

It is not without interest to inquire how the interferences should have taken place if the experiments had led to the conclusion that the velocity in wires is equal to the velocity in air. This can easily be deduced from the correct theory given in No. IX., together with the aid of Fig. 31, and comes out as follows:—

	0		1		2		3		4		5		6		7		8
100	+	+	O	O	−	−	−	−	−	−	−	−	−	−	−	−	−
250	O	−	−	−	O	O	O	O	O	O	O	O	O	O	O	O	O
400	−	−	O	O	+	+	+	+	+	+	+	+	+	+	+	+	+

If the velocities were equal, there should still have been *one* change of sign; but the further changes which the experiments gave can only be explained by a difference in the velocities, or by illusions due to reflections or disturbances in the neighbourhood.

18. [VII. p. 131.]

It should be observed that we are here only able to determine the position of the magnetic force by the aid of theory. From the experiments we cannot conclude that a second kind of force is present together with the electric force. If we confine ourselves to the experiments, we can only regard the expression "magnetic force" as a short name for a certain mode of distribution of the electric force. That this magnetic force produces effects which cannot be explained by the electric force, is first verified by experiments in No. XII.; and, of course, only for waves in wires.

19. [VIII. p. 133.]

The wave-length measured depends, therefore, very much upon the distance of B and C; and hence upon the assumption that C is quite accurately measured. If we assume that the position of C is altered by general conditions of the surrounding space, the first node should be placed nearer to the wall and we might obtain much smaller values for the wave-length. But the experiments give no reason for believing that the position of C is uncertain.

20. [VIII. p. 136.]

Lloyd's experiment is the optical analogue of the experiments in which the primary conductor is gradually moved away from the wall. The experiments of the first kind, in which we removed the secondary conductor from a reflecting wall, have also found an optical analogue in the beautiful experiments which Herr O. Wiener has published in his paper on "Stationary Light-Waves and the Direction of Vibration of Polarised Light" (*Wied. Ann.* **40**, p. 303).

As to the acoustic analogues, I find that the phenomenon which forms the analogue to the experiments of the first kind was discovered by N. Savart many years ago (see *Pogg. Ann.* **46**, p. 458, 1839; also a number of Seebeck's paper in the subsequent volumes). If a steady source of sound is placed at a distance of 15-20 metres in front of a plane wall, and if we listen near the wall (best with the aid of a resonator), we find that the sound swells out at certain points—the antinodes,—and becomes weak at other points—the nodes. A correct analogue to the experiments of the second kind—in which the primary conductor is moved—has been already given in the text. Another analogue—in itself interesting—is the following. Take a glass tube about 60 cm. long and 2 cm. in diameter and lower it gradually over a Bunsen burner, of which the flame is not too large. At a given depth the Bunsen flame will begin, but not without some difficulty, to make the tube sing loudly. Now bring the system near to a wall. Quite near the wall the sound disappears; it reappears at a distance of a quarter wave-length, and again vanishes at a distance of half a wave-length. By very careful adjustment, which up to the present I have not been able to secure at will, I have been able to observe two further positions of sound and silence at distances of half a wave-length. I do not know of any complete explanation of this phenomenon. Probably it has some connection with the fact that such a tube becomes silent if a resonator, tuned to the same note, is brought near its end. This last experiment is due—as far as I am aware—to Professor A. Christiani (*Verhandl. d. phys. Gesellsch. zu Berlin*, Dec. 15, 1882, at end of the *Fortschritte der Physik*, **36**).

21. [VIII. p. 136.]

This remark refers to the experiments with wires, which I was arranging at the time when this paper was written. It has already been stated in the introduction that the hope here expressed has not been fulfilled.

22. [IX. p. 141.]

An error in sign in the original paper, to which M. L. de la Rive drew my attention, has here been corrected.

23. [IX. p. 150.]

This calculation is based upon the observed wave-length of 480 cm. If this is not correct, the calculation must be altered accordingly. With regard to the real value of the damping see Note 5.

24. [X. p. 161.]

By the experiments in the following paper it is pretty plainly proved that in the case of rapid variations of current the changes penetrate from without into the wire. It is thereby made probable that in the case of a steady current as well, the disturbance in the wire itself is not, as has hitherto been assumed, the cause of the phenomena in its neighbourhood; but that, on the contrary, the disturbances in the neighbourhood of the wire are the cause of the phenomena inside it.

That the disturbances in the wire are connected with a regular circulation of materal particles, or of a fluid assumed *ad hoc*, is a hypothesis which is neither proved nor disproved by our experiments; they simply have nothing to do with it. We have neither any right to oppose this hypothesis, nor have we any intention of doing so, on the ground of the experiments here described.

25. [XI. p. 177.]

In connection with these phenomena we may refer to the observation which Herren Hagenbach and Zehnder have brought forward as an objection to my interpretation of the experiment (*Wied. Ann.* **43**, p. 611). My meaning is that light behaves just as the electric waves here behave; but we must imagine the dimensions of everything concerned in the experiment to be reduced in the same proportion, not only the length of the waves.

26. [XI. p. 181.]

Herr W. König first pointed out that the analogy between the reflection of electric waves from our grating and the reflection of monochromatic light from the surface of dichroic crystals is much more complete than the analogy which is drawn in the text. He has also drawn attention to the relation between the action of our grating and certain polarising effects of optical gratings (*Wied. Ann.* **37**, p. 651, at the end).

27. [XI. p. 182.]

Messrs. Oliver Lodge and Howard have actually succeeded in showing the refraction and concentration of electric rays by means of large lenses (*Phil. Mag.* **27**, p. 48, 1889).

28. [XI. p. 185.]

Since then the experiments have been exhibited objectively in many ways. Herr R. Ritter has employed successfully a frog's leg (*Wied. Ann.* **40**, p. 53). Mr. Dragoumis has used Geissler tubes (*Nature*, **39**, p. 548). Herr Boltzmann has given a very convenient method in which a gold-leaf electroscope is used (*Wied. Ann.* **40**, p. 399). Herr Klemenčič has used a thermo-element (*Wied. Ann.* **42**, p. 416). The method which is most elegant and best adapted for demonstration, although it is far from being an easy one, is the bolometer method which Herren H. Rubens and R. Ritter have employed for exhibiting the experiments and for further useful researches (*Wied. Ann.* **40**, p. 55, and subsequent volumes).

29. [XIII. p. 198.]

And by more than one independent variable. The "force" and "polarisation" in this paper are not to be regarded as two variables in this sense; for they are connected by a fixed linear relation. If this relation is allowed to drop, by regarding it as a special case of a general relation, then "force" and polarisation" may serve as two variables. But it would be more convenient to introduce the polarisation of the ether as the one variable, and the polarisation of the ponderable matter as the other.

30. [XIII. p. 214.]

According to this usual system of nomenclature it is undoubtedly true that the amount of "electricity" on an insulated sphere remains unchanged when the sphere is immersed in an insulating fluid, or, speaking generally, when it is moved in any way through insulating media. Hence we have denoted as "true" electricity the magnitude which remains unchanged during such motion. The distance-action of the sphere, and therefore the "free" electricity does change during the motion.

31. [XIII. p. 220.]

Consider a steam-engine which drives a dynamo by means of a strap running to the dynamo and back, and which in turn works an arc lamp by means of a wire reaching to the lamp and back again. In ordinary language we say—and no exception need be taken to such a mode of expression—that the energy is transferred from the steam-engine by means of the strap to the dynamo, and from this again to the lamp by the wire. But is there any clear physical meaning in asserting that the energy travels from point to point along the stretched strap in a direction opposite to that in which the strap itself moves? And if not, can there be any more clear meaning in saying that the energy travels from point to point along the wires, or—as Poynting says—in the space between the wires? There are difficulties here which badly need clearing up.

32. [XIII. p. 221.]

In order to deduce the mechanical forces from the changes of energy, we must impart virtual displacements to the bodies. Hence we should have to use the equations for bodies in motion and not for bodies at rest, and at present the former are not at our disposal. By the aid of the experimental fact here assumed we are able to fill up this gap for the statical and steady states satisfactorily.

33. [XIV. p. 244.]

This proof that the statements here made embrace the observed facts, is also a proof of the statements themselves. They are therefore logically stated as facts derived from experience; not as results of any particular experiment, but as results of all the general experience which we possess respecting such matters.

34. [XIV. p. 246.]

The meaning of the equations is exceedingly simple; but their external appearance is somewhat complicated. This led me to expect that skilful mathematicians might be able to replace them by more elegant forms. And in fact Signor Vito Volterra has succeeded in representing by a single system of equations the phenomena for bodies both at rest and in motion (*Il nuovo Cimento* (3), **29**, p. 53; see also p. 147 *ibid.*)

35. [XIV. p. 255.]

A similar theory has also been developed recently by J. J. Thomson (*Phil. Mag.* (5), **31**, p. 149). In so far as this theory and Poynting's lead to Maxwell's equations, I would regard them as special forms of "Maxwell's theory," although their conceptions are undoubtedly not Maxwell's.

36. [XIV. p. 267.]

This does not necessarily imply an error in the theory, though it does necessarily imply a lack of completeness in it. Moreover it seems to be at the very root of our view, for it can be understood without using the equations. Let us suppose a magnetised steel sphere to rotate in free space about an axis which does not coincide with the direction of magnetisation. It continually sends out electromagnetic waves; it therefore gives out energy and must gradually come to rest. Now let us take an iron sphere at rest and excite in it a rotary magnetisation by varying electric forces; it will easily be seen that the iron sphere must, conversely, begin to rotate. Such conclusions scarcely seem probable. But in connection with these matters we have scarcely any right to speak of probability,—so complete is our ignorance as to possible motions of the ether.