I

INTRODUCTION

A. *Experimental*

I HAVE often been asked how I was first led to carry out the experiments which are described in the following pages. The general inducement was this. In the year 1879 the Berlin Academy of Science had offered a prize for a research on the following problem:—To establish experimentally any relation between electromagnetic forces and the dielectric polarisation of insulators—that is to say, either an electromagnetic force exerted by polarisations in non-conductors, or the polarisation of a non-conductor as an effect of electromagnetic induction. As I was at that time engaged upon electromagnetic researches at the Physical Institute in Berlin, Herr von Helmholtz drew my attention to this problem, and promised that I should have the assistance of the Institute in case I decided to take up the work. I reflected on the problem, and considered what results might be expected under favourable conditions by using the oscillations of Leyden jars or of open induction-coils. The conclusion at which I arrived was certainly not what I had wished for; it appeared that any decided effect could scarcely be hoped for, but only an action lying just within the limits of observation. I therefore gave up the idea of working at the problem; nor am I aware that it has been attacked by anybody else. But in spite of having abandoned the solution at that time, I still felt ambitious to discover it by some other method; and my interest in everything connected with electric oscillations had become keener. It was scarcely possible that I should overlook any new form of such oscillations, in case a happy chance should bring such within my notice.

Such a chance occurred to me in the spring of 1886, and brought with it the special inducement to take up the following researches. In the collection of physical instruments at the Technical High School at Karlsruhe (where these researches were carried out), I had found and used for lecture purposes a pair of so-called Riess or Knochenhauer spirals. I had been surprised to find that it was not necessary to discharge large batteries through one of these spirals in order to obtain sparks in the other; that small Leyden jars amply sufficed for this purpose, and that even the discharge of a small induction-coil would do, provided it had to spring across a spark-gap. In altering the conditions I came upon the phenomenon of side-sparks which formed the starting-point of the following research. At first I thought the electrical disturbances would be too turbulent and irregular to be of any further use; but when I had discovered the existence of a neutral point in the middle of a side-conductor, and therefore of a clear and orderly phenomenon, I felt convinced that the problem of the Berlin Academy was now capable of solution. My ambition at the time did not go further than this. My conviction was naturally strengthened by finding that the oscillations with which I had to deal were regular. The first of the papers here republished ("On very Rapid Electric Oscillations") gives, generally in the actual order of time, the course of the investigation as far as it was carried out up to the end of the year 1886 and the beginning of 1887.

While this paper was in the press I learned that its contents were not as new as I had believed them to be. The Geographical Congress of April 1887 brought Herr W. von Bezold to Karlsruhe and into my laboratory. I spoke to him about my experiments; he replied that years ago he had observed similar phenomena, and he drew my attention to his "Researches on the Electric Discharge," in vol. cxl. of Poggendorff's *Annalen.* This paper had entirely escaped me, inasmuch as its external appearance seemed to indicate that it related to matters quite other than electric oscillations, namely, Lichtenberg figures; indeed, it does not appear to have attracted such attention as the importance of its contents merited. In an appendix to my paper I acknowledged Herr von Bezold's prior claim to a whole series of observations. In place of this appendix, I here, with Herr von Bezold's kind consent, include as the second of these papers that part of his communication which is of the most immediate interest in the present connection. It may now well be asked with surprise how it was possible that results so important and so definitely stated should have exercised no greater influence upon the progress of science? Perhaps the fact that Herr von Bezold described his communication as a preliminary one may have something to do with this.

I may here be permitted to record the good work done by two English colleagues who at the same time as myself were striving towards the same end. In the same year in which I carried out the above research, Professor Oliver Lodge, in Liverpool, investigated the theory of the lightning-conductor, and in connection with this carried out a series of experiments on the discharge of small condensers which led him on to the observation of oscillations and waves in wires. Inasmuch as he entirely accepted Maxwell's views, and eagerly strove to verify them, there can scarcely be any doubt that if I had not anticipated him he would also have succeeded in observing waves in air, and thus also in proving the propagation with time of electric force. Professor Fitzgerald, in Dublin, had some years before endeavoured to predict, with the aid of theory, the possibility of such waves, and to discover the conditions for producing them. My own experiments were not influenced by the researches of these physicists, for I only knew of them subsequently. Nor, indeed, do I believe that it would have been possible to arrive at a knowledge of these phenomena by the aid of theory alone. For their appearance upon the scene of our experiments depends not only upon their theoretical possibility, but also upon a special and surprising property of the electric spark which could not be foreseen by any theory.

By means of the experiments already mentioned I had succeeded in obtaining a method of exciting more rapid electric disturbances than were hitherto at the disposal of physicists. But before I could proceed to apply this method to the examination of the behaviour of insulators, I had to finish with another investigation. Soon after starting the experiments I had been struck by a noteworthy reciprocal action between simultaneous electrical sparks. I had no intention of allowing this phenomenon to distract my attention from the main object which I had in view; but it occurred in such a definite and perplexing way that I could not altogether neglect it. For some time, indeed, I was in doubt whether I had not before me an altogether new form of electrical action-at-a-distance. The supposition that the action was due to light seemed to be excluded by the fact that glass plates cut it off; and naturally it was some time before I came to experiment with plates of rock-crystal. As soon as I knew for certain that I was only dealing with an effect of ultra-violet light, I put aside this investigation so as to direct my attention once more to the main question. Inasmuch as a certain acquaintance with the phenomenon is required in investigating the oscillations, I have reprinted the communication relating to it ("On an Effect of Ultra-Violet Light upon the Electric Discharge") as the fourth of these papers. A number of investigators, more especially Herren Righi, Hallwachs, and Elster and Geitel, have helped to make our knowledge of the phenomenon more accurate; nevertheless, the mechanics of it have not yet been completely disclosed to our understanding.

The summer of 1887 was spent in fruitless endeavours to establish the electromagnetic influence of insulators by the aid of the new class of oscillations. The simplest method consisted in determining the effect of dielectrics upon the position of the neutral point of a side-circuit. But in that case I should have had to include the electrostatic forces in the bargain, whereas the problem consisted precisely in investigating the electromagnetic induction alone. The plan which I adopted was the following:—The primary conductor[1] had the form shown in Fig. 1; between the plates A and A' at its ends was introduced a block BB of sulphur or paraffin, and this was then quickly removed. I placed the secondary conductor C in the same position, with respect to the primary, as before (the only position which I had taken

[1] The reader is assumed to be already acquainted with the papers referred to.

into consideration), and expected that when the block was in place very strong sparks would appear in the secondary, and that when the block was removed there would only be feeble sparks. This latter expectation was based upon the supposition that the electrostatic forces could in no case induce a spark in the almost closed circuit *C*, for since these forces have a potential, it follows that their integral over a nearly closed circuit is vanishingly small. Thus in the absence of the insulator we should only have to consider the inductive effect of the more distant wire *a b*. The experiment was frustrated by the invariable occurrence of strong sparking in the secondary conductor, so that the moderate strengthening or weakening effect which the insulator must exert did not make itself felt. It only gradually became clear to me that the law which I had assumed as the basis of my experiment did not apply here; that on account of the rapidity of the motion even forces which possess a potential are able to induce sparks in the nearly closed conductor; and, in general, that the greatest care has to be observed in applying here the general ideas and laws which form the basis of the usual electrical theories. These laws all relate to statical or stationary states; whereas here I had truly before me a variable state. I perceived that I had in a sense attacked the problem too directly. There was yet an infinite number of other positions of the secondary with respect to the primary conductor, and among these there might well be some more favourable for my purpose. These various positions had first to be examined. Thus I came to discover the phenomena which are described in the fifth paper ("On the Action of a Rectilinear Electric Oscillation upon a Neighbouring Circuit"), and which surprised me by their variety and regularity. The finding out and unravelling of these extremely orderly phenomena gave me peculiar pleasure. The paper certainly does not include all the discoverable details; whoever may extend

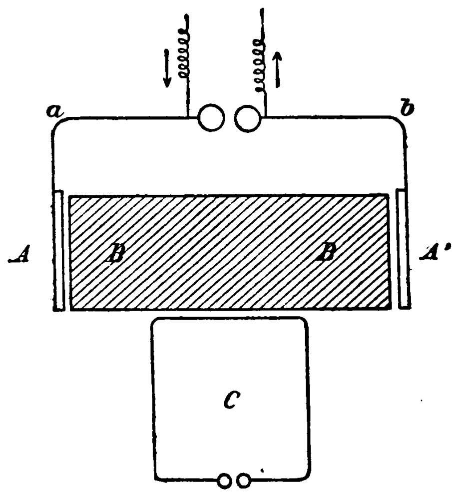

Fig. 1.

the experiments to various other forms of conductor will find that the task is not an ungrateful one. The observations at greater distances are also probably very inaccurate, for they are affected by the disturbing influence of reflections which were not at that time suspected. What especially surprised me was the continual increase of the distance up to which I could perceive the action; up to that time the common view was that electric forces decreased according to the Newtonian law, and therefore rapidly tended to zero as the distance increased.

Now during the course of this investigation I had made sure of other positions of the secondary conductor in which it was possible, by bringing an insulator near, to cause the appearance or disappearance of sparks, instead of simply altering their size. The problem which I was investigating was now solved directly in the manner described in the sixth paper ("On Electromagnetic Effects produced by Electrical Disturbances in Insulators"). On 10th November 1887 I was able to report the successful issue of the work to the Berlin Academy.

The particular problem of the Academy which had been my guide thus far was evidently propounded at the time by Herr von Helmholtz in the following connection:—If we start from the electromagnetic laws which in 1879 enjoyed universal recognition, and make certain further assumptions, we arrive at the equations of Maxwell's theory which at that time (in Germany) were by no means universally recognised. These assumptions are: first, that changes of dielectric polarisation in non-conductors produce the same electromagnetic forces as do the currents which are equivalent to them; secondly, that electromagnetic forces as well as electrostatic are able to produce dielectric polarisations; thirdly, that in all these respects air and empty space behave like all other dielectrics. In the latter part of his paper ("On the Equations of Motion of Electricity for conducting Bodies at Rest"),[1] von Helmholtz has deduced Maxwell's equations from the older views and from hypotheses which are equivalent to those just stated. The problem of proving all three hypotheses, and thereby establishing the correctness of the whole of

[1] v. Helmholtz, *Ges. Abhandl.* 1, p. 545.

Maxwell's theory, appeared to be an unreasonable demand; the Academy, therefore, contented itself with requiring a confirmation of one of the first two.

The first assumption was now shown to be correct. I thought for some time of attacking the second. To test it appeared by no means impossible; and for this purpose I cast closed rings of paraffin. But while I was at work it struck me that the centre of interest in the new theory did not lie in the consequences of the first two hypotheses. If it were shown that these were correct for any given insulator, it would follow that waves of the kind expected by Maxwell could be propagated in this insulator, with a finite velocity which might perhaps differ widely from that of light. I felt that the third hypothesis contained the gist and special significance of Faraday's, and therefore of Maxwell's, view, and that it would thus be a more worthy goal for me to aim at. I saw no way of testing separately the first and the second hypotheses for air;[1] but both hypotheses would be proved simultaneously if one could succeed in demonstrating in air a finite rate of propagation and waves. Certainly some of the first experiments in this direction failed; these are described in the paper referred to, and they were carried out at short distances. But in the meantime I had succeeded in detecting the inductive action at distances up to 12 metres. Within this distance the phase of the motion must have been reversed more than once; and now it only remained to detect and prove this reversal. Thus the scheme was conceived which was carried out as described in the research "On the Finite Velocity of Propagation of Electromagnetic Actions." The first step that had to be taken was easy. In straight stretched wires surprisingly distinct stationary waves were produced with nodes and antinodes, and by means of these it was possible to determine the wave-length and the change of phase along the wire. Nor was there any greater difficulty in producing interference between the action which had travelled along the wire and that which had travelled through the air, and thus in comparing their phases. Now if both actions

[1] The expressions air (*Luftraum*) and empty space (*leerer Raum*) are here used as synonymous, inasmuch as the influence of the air itself in these experiments is negligible.

were propagated, as I expected, with one and the same finite velocity, they must at all distances interfere with the same phase. A simple qualitative experiment which, with the experience I had now gained, could be finished within an hour, must decide this question and lead at once to the goal. But when I had carefully set up the apparatus and carried out the experiment, I found that the phase of the interference was obviously different at different distances, and that the alternation was such as would correspond to an infinite rate of propagation in air. Disheartened, I gave up experimenting. Some weeks passed before I began again. I reflected that it would be quite as important to find out that electric force was propagated with an infinite velocity, and that Maxwell's theory was false, as it would be, on the other hand, to prove that this theory was correct, provided only that the result arrived at should be definite and certain. I therefore confirmed with the greatest care, and without heeding what the outcome might be, the phenomena observed: the conclusions arrived at are given in the paper. When I then proceeded to consider more closely these results, I saw that the sequence of the interferences could not be harmonised with the assumption of an infinite rate of propagation; that it was necessary to assume that the velocity was finite, but greater than that in the wire. As shown in the paper, I endeavoured to bring into harmony the various possibilities; and although the difference in the velocities appeared to me to be somewhat improbable, I could see no reason for mistrusting the experiments. And it was not by any means impossible that the motion in the wire might be retarded by some unknown causes, as, for example, by an essential inertia of the free electricity.

I have entered into these details here in order that the reader may be convinced that my desire has not been simply to establish a preconceived idea in the most convenient way by a suitable interpretation of the experiments. On the contrary, I have carried out with the greatest possible care these experiments (by no means easy ones), although they were in opposition to my preconceived views. And yet, although I may have been lucky elsewhere, in this research I have been decidedly unlucky. For instead of reaching the right goal with little effort, as a properly devised plan might have enabled me to do, I seem to have taken great pains, and to have fallen into error after all.

In the first place, the research is disfigured by an error of calculation. The time of oscillation is overestimated in the ratio of $\sqrt{2}:1$. M. Poincaré first drew attention to this error.[1] As a matter of fact, this error affects the form of the research more than the substance of it. My reliance on the correctness of the calculation was mainly due to its supposed accordance with the experiments of Siemens and Fizeau and with my own.[2] If I had used the correct value for the capacity, and so found out the discrepancy between calculation and experiment, I would have placed less reliance on the calculation; the investigation would have been somewhat altered in form, but the subject-matter would have remained unaltered.

In the second place (and this is the more important point), one of the principal conclusions of the investigation can scarcely be regarded as correct—namely, that the velocities in air and in the wire are different. Such further knowledge as has been gained respecting waves in wires, instead of confirming this result, tends to make it more and more improbable. It now seems fairly certain that if the experiment had been carried out quite correctly, and without any disturbing causes, it would have given almost exactly the result which I expected at the start. There is no doubt that the phase of the interference must have changed sign once (and this I had not expected beforehand); but the interference should have exhibited no second change of sign; and yet the experiments without exception pointed to this. It is not easy to point to any disturbing cause which could imitate in such a deceptive way the effect of a difference in velocity; but there is no reason why we should not admit the possibility of such a deception. While performing the experiments, I never in the least suspected that they might be affected by the neighbouring walls. I remember that the wire along which the waves travelled was carried past an iron stove, and only 1·5 metres from it. A disturbance caused in this way, and always acting at the same point, might have given rise to the second change

[1] H. Poincaré, *Comptes Rendus*, **111**, p. 322.

[2] See the remark at the end of the second part of the paper (p. 114).

of phase of the interference. However this may be, I should like to express a hope that these experiments may be repeated by some other observer under the most favourable conditions possible, *i.e.* in a room as large as possible. If the plan of the experiment is correct, as I think it is, then it must, whenever properly carried out, give the result which it should at first have given; it would then prove without measurement the finite velocity of propagation of the waves in air, and at the same time the equality between this and the velocity of the waves in the wire.

I might also mention here some further considerations which at that time strengthened my conviction that the waves in the wire suffered a retardation. If the waves in the wire run along at the same speed as waves in air, then the lines of electric forces must be perpendicular to the wire. Thus a straight wire traversed by waves cannot exert any inductive action upon a neighbouring parallel wire. But I found that there was such an action, even though it was only a weak one. I concluded that the lines of force were not parallel to the wire, and that the velocity of the waves was not the same as that of light. Further, if the lines of force are perpendicular to the wire, it can be shown by a simple calculation that the energy propagated by a wave in a single wire becomes logarithmically infinite. I therefore concluded that such a wave was *à priori* impossible. Lastly, it seemed to me that it could have no effect upon the rate of propagation in a straight conductor, whether that conductor was a smooth wire or a wire with side projections, or a crooked wire, or a spiral wire with small convolutions, provided always that these deviations from the straight line were small compared with the wave-length, and that their resistance did not come into consideration. But now I found that all these alterations produced a very noticeable effect upon the velocity. Hence I concluded that here again there was some obscure cause at work which caused a retardation, and which would also make itself felt in simple smooth wires. At the present moment these and other reasons do not appear to me to be of decisive weight; but at that time they so far satisfied me that I asserted without any reserve that there was a difference between the velocities, and regarded this decision as one of the most interesting of my experimental results. Soon I was to discover what appeared to be a confirmation of my opinion; and at that time it was very welcome.

While investigating the action of my primary oscillation at great distances, I came across something like a formation of shadows behind conducting masses, and this did not strike me as being very surprising. Somewhat later on I thought that I noticed a peculiar reinforcement of the action in front of such shadow-forming masses, and of the walls of the room. At first it occurred to me that this reinforcement might arise from a kind of reflection of the electric force from the conducting masses; but although I was familiar with the conceptions of Maxwell's theory, this idea appeared to me to be almost inadmissible—so utterly was it at variance with the conceptions then current as to the nature of an electric force. But when I had established with certainty the existence of actual waves, I returned to the mode of explanation which I had at first abandoned, and so arrived at the phenomena which are described in the paper "On Electromagnetic Waves in Air, and their Reflection." No objection can be urged against the qualitative part of this research—the experiments have been frequently repeated and confirmed. But the part of the research which relates to the measurements is doubtful, inasmuch as it also leads to the very unlikely result that the velocity in air is considerably greater than that of waves in wires. Assuming that this result is incorrect, how are we to explain the error which has crept in? Certainly it is not due to simple inaccuracy of observation. The error of observation may perhaps be about a decimetre, but certainly not a metre. I can only here attribute the mistake in a general sense to the special conditions of resonance of the room used. The vibrations natural to it may possibly have been aroused, and I may have observed the nodes of such a vibration when I thought that I was observing the nodes of the waves of the primary conductor. There was certainly a substantial difference between the distances of the nodes in air which I measured and the wave-lengths in the wire. I specially directed my attention to the question whether or not such a difference existed. As far as any exact accordance with the first series of experiments is concerned, I freely allow that in the interpretation of the experimental results I may have allowed myself to be influenced by a desire to establish an accordance between the two sets of measurements. I put back the first node a certain distance behind the wall, and an exact control of the amount of this cannot be deduced from the experiments. If I had wished to combine the experiments otherwise, I might indeed have been able to calculate a ratio of the velocities which would come out nearer to unity; but I certainly could not infer from them that the velocities were equal.

Now, if the experiments which I made at that time all agree in pointing to a difference between the velocities, it will naturally be asked what reasons now induce me to allow that there may have been unknown sources of error in the experiments, rather than to abide by the statement made as to the difference of velocities. Is it the objection which has been raised in several quarters as to the want of accord between the results and the theory? Certainly not. The theory was known to me at the time; and furthermore, it must be subordinated to the experiments. Is it the experiment in this connection made by Herr Lecher?[1] This, too, I must deny, although I fully recognise the value of the work which Herr Lecher has done in this direction. In working out his results Herr Lecher assumes that the calculation is correct, and therefore in a certain sense that the theory itself is correct.[2] Is it then the results of MM. Sarasin and de la Rive,[3] who carefully repeated the experiments and arrived at conclusions which were completely in accord with the theory? In a certain sense, yes; in another sense, no. The Genevan physicists worked in a much smaller room than my own; the greatest distance of which they could avail themselves was only 10 metres, and the waves could not develop quite freely even up to this distance. Their mirror was only 2·8 metres high. Care in carrying out the observations cannot compensate for the unfavourable nature of the room. In my experiments, on the other hand, the waves had perfectly free play up to 15

[1] E. Lecher, "Eine Studie über elektrische Resonanzerscheinungen," *Wied. Ann.* **41**, p. 850.

[2] The same remark holds good for the work recently published by M. Blondlot, *C. R.* **113**, p. 628 (cp. Note 15 at end of book).

[3] E. Sarasin and L. de la Rive, *Comptes Rendus*, **112**, p. 658.

metres. My mirror was 4 metres high. If the decision rested simply and solely with the experiments, I could not attribute greater weight to those of MM. Sarasin and de la Rive than to my own.[1] So far, then, I again say no. But certainly the Genevan experiments show that my experiments are subject to local variations; they show that the phenomena are different if the reflecting walls and the rooms are different, and also that under certain conditions the wave-lengths have the values required by theory. But if the experiments furnish information which is ambiguous and contradictory, they obviously contain sources of error which are not understood; and hence they cannot be brought forward as arguments against a theory which is supported by so many reasons based on probability. Thus the Genevan experiments deprive my own of their force, and so far they restore the balance of probability to the theoretical side.

Still, I must acknowledge that the reasons which decided me were of a more indirect kind. When I first thought that I had found a retardation of waves in the wires, I hoped soon to discover the cause of this retardation, and to find some gradual change in its value. This hope has not been realised. I found no such change, and, as my experience increased, instead of coming across an explanation, I met with increasing discrepancies, until these at last appeared to me to be insoluble, and I had to give up all hope of proving the correctness of my first observation. My own discovery, that for short waves the difference between the velocities very nearly disappears, tended in the same direction. Before one of my scientific colleagues had attacked this question, I had stated my opinion in the following words:[2]—"Thus I found that for long waves the wave-length is greater in air than in wires, whereas for short waves both appear to be practically equal. This result is so surprising that we cannot regard it as certain. The decision must be reserved until further experiments are made." The only experiments of the kind referred to that have hitherto been made are those of MM. Sarasin and de la Rive; and inasmuch as these were carried

[1] Mr. Trouton, in a room of which the dimensions are not exactly given, found, like myself, that the wave-length of my primary conductor in air was about 10 metres.—*Nature*, **39**, p. 391.

[2] *Archives de Genève* (3), **21**, p. 302.

out in small rooms, they may more properly be regarded as a confirmation of the second part of my statement than as a refutation of the first part. Decisive experiments for long waves seem to me to be still wanting.[1] I have little doubt that they will decide in favour of equal velocities in all cases.

The reader may, perhaps, ask why I have not endeavoured to settle the doubtful point myself by repeating the experiments. I have indeed repeated the experiments, but have only found, as might be expected, that a simple repetition under the same conditions cannot remove the doubt, but rather increases it. A definite decision can only be arrived at by experiments carried out under more favourable conditions. More favourable conditions here mean larger rooms, and such were not at my disposal. I again emphasise the statement that care in making the observations cannot make up for want of space. If the long waves cannot develop, they clearly cannot be observed.

The experiments hitherto described on the reflection of waves were finished in March 1888. In the same month I attempted, by means of reflection at a curved surface, to prevent the dispersion of the action. For my large oscillator I built a concave parabolic mirror of 2 metres aperture and 4 metres high. Contrary to my expectation I found that the action was considerably weakened. The large mirror acted like a protecting screen surrounding the oscillator. I concluded that the wave-length of the oscillation was too large in comparison with the focal length of the mirror. A moderate reduction in the size of the primary conductor did not improve the result. I therefore tried to work with a conductor which was geometrically similar to the larger one, but smaller in the proportion of 10 : 1. Perhaps I did not persevere sufficiently in this attempt; at any rate I entirely failed at that time to pro-

[1] Since the above was written, the wish expressed has been amply satisfied by the experiments which MM. Sarasin and de la Rive have carried out in the great hall of the Rhone waterworks at Geneva (see *Archives de Genève*, **29**, pp. 358 and 441). These experiments have proved the equality of the velocity in air and in wires, and have thus established the full agreement between experiment and theory. I consider these experiments to be conclusive, and submit to them now with as much readiness as I then felt hesitation in submitting to experiments which were not superior to my own. I gladly avail myself of the opportunity of thanking MM. Sarasin and de la Rive for the great kindness and goodwill which they invariably exhibited in the whole controversy—a controversy which has now been decided entirely in their own favour.

duce and observe such short oscillations, and I abandoned these experiments in order to turn my attention to other questions.

In the first place, it was important to devise a clearer theoretical treatment of the experiments. In the researches to which I have hitherto referred, the experiments were interpreted from the standpoint which I took up through studying von Helmholtz's papers.[1] In these papers Herr v. Helmholtz distinguishes between two forms of electric force—the electromagnetic and the electrostatic—to which, until the contrary is proved by experience, two different velocities are attributed. An interpretation of the experiments from this point of view could certainly not be incorrect, but it might perhaps be unnecessarily complicated. In a special limiting case Helmholtz's theory becomes considerably simplified, and its equations in this case become the same as those of Maxwell's theory; only one form of the force remains, and this is propagated with the velocity of light. I had to try whether the experiments would not agree with these much simpler assumptions of Maxwell's theory. The attempt was successful. The results of the calculation are given in the paper on "The Forces of Electric Oscillations, treated according to Maxwell's Theory." That part of the research which relates to interference between waves in air and in wires could clearly be adapted without difficulty to any other form of such interference which might result from more complete experiments.

Side by side with the theoretical discussions I continued the experimental work, directing the latter again more to waves in wires. In doing so, my primary object was to find out the cause of the supposed retardation of these waves. Secondly, I wished to test the correctness of the view according to which the seat and field of action of the waves is not in the interior of the conductor, but rather in the surrounding space. I now made the waves travel in the interspace between two wires, between two plates, and in tubular spaces, instead of along a single wire; in various interposed insulators instead of in different metals. The research on "The Propagation of Electric Waves by Means of Wires" was, for the most part, carried out in the summer of 1888, although it was only completed and published later on.

[1] v. Helmholtz, *Ges. Abhandl.* **1**, p. 545.

For in the autumn a singular phenomenon attracted my attention away from the experiments with wires. For the investigation of waves in the narrow interspace between two wires I was using resonators of small external dimensions, and was engaged in tuning these. I found that I obtained distinct nodes at the end of the wires even when I used resonators which were much too small. Even when I diminished the size of the circles to a few centimetres diameter, I still obtained nodes; these were situated at a small distance from the end of the wires, and I could observe half wave-lengths as small as 12 cm. Thus chance brought me on to the track, hitherto undiscovered, of the short waves. I at once followed up this track, and soon succeeded in finding a form of the primary conductor which could be used with the small resonators.

I paid no special attention to the phenomenon which led me back to the observation of short waves; and, as no suitable occasion arose for doing so, I have not mentioned it in my papers. Clearly it was a special case of the same phenomenon which was later on discovered by MM. Sarasin and de la Rive,[1] called by the name of "Multiple Resonance," and explained by saying that the primary conductor did not possess any definite period of oscillation, but that it performed simultaneously all possible oscillations lying within wide limits. If I paid little attention myself to this phenomenon, it was partly because I was soon led on to other researches. It arose no less from the fact that I had from the start conceived an interpretation of the phenomenon which lent much less interest to it than the interpretation given by MM. Sarasin and de la Rive. I regarded the phenomenon as a consequence of the rapid damping of the primary oscillation—a necessary consequence, and one which could be foreseen. M. Sarasin was good enough to communicate at once to me the results of his research, and I told him my doubts as to his explanation of the phenomenon, and gave him my own explanation of it; but although he received my explanation with the readiest goodwill, we did not succeed in coming to a common understanding as to the interpretation of the experiment. With M. H. Poincaré such an understanding was secured at once; he had formed a conception of the

[1] E. Sarasin and L. de la Rive, *Arch. de Genève* (3), **23**, p. 113, 1890.

phenomenon which was practically identical with my own, and had communicated it to me in a letter. This conception he has worked out mathematically, and published in his book *Electricité et Optique*.[1] Herr V. Bjerknes has worked out the mathematical developments simultaneously and independently.[2] That the explanation given by MM. Poincaré and Bjerknes is not only a possible one, but is the only possible one, appears to me to be proved by an investigation by Herr Bjerknes,[3] which has appeared recently, and which makes it certain that the vibration of the primary conductor is, at any rate to a first approximation, a uniformly damped sine-wave of determinate period. Hence the careful investigations of MM. Sarasin and de la Rive are of great value in completing our knowledge of this part of the work, but they in no way contradict any statement made by me. The authors themselves regard their experiments in this light. Nevertheless, these experiments gave occasion to an adverse criticism of my work from a distinguished French physicist who had not, however, repeated the experiments himself. I hope it will now be allowed that there was no cause for such a criticism.[4]

I may be permitted to take this opportunity of referring to the doubts which have recently been raised by Herrn Hagenbach and Zehnder as to what my experiments really prove.[5] Perhaps I ought not yet to consider their work as being completed. The authors reserve to themselves the right of returning to the explanation of resonance and the formation of nodes and antinodes in my experiments. But it is just precisely upon these phenomena that my experiments, and the whole interpretation of them, rest.

After I had succeeded (as already described) in observing very short waves, I chose waves about 30 cm. long, and repeated first of all the earlier experiments with these. I now found, contrary to my expectation, that these short waves travelled along wires with very nearly the same velocity as in air. As it was easy to procure free play for such short waves, no doubt could arise in this case as to the correctness of the

[1] H. Poincaré, *Electricité et Optique*, **2**, p. 249.
[2] V. Bjerknes, *Wied. Ann.* **44**, p. 92, 1891.
[3] *Ibid.* **45**, p. 513, 1891.
[4] Cornu, *Comptes Rendus*, **110**, p. 72, 1890.
[5] E. Hagenbach and L. Zehnder, *Wied. Ann.* **43**, p. 610, 1891.

results. After I had become quite used to managing these short waves, I returned to the experiment with the concave reflector. The large old reflector was no longer at my disposal, so I had a smaller one made, about 2 metres high and a little more than 1 metre in aperture. It worked so remarkably well that, directly after the first trial, I ordered not only a second concave reflector, but also a plane reflecting surface and a large prism. The experiments which are described in the paper "On Electric Radiation" now followed each other in rapid succession, and without difficulty; they had been considered and prepared long beforehand, with the exception of the polarisation-experiments, which only occurred to me during the progress of the work. These experiments with concave mirrors soon attracted attention; they have frequently been repeated and confirmed. The approval with which they have been received has far exceeded my expectation.[1] A considerable part of this approval was due to reasons of a philosophic nature. The old question as to the possibility and nature of forces acting at a distance was again raised. The preponderance of such forces in theory has long been sanctioned by science, but has always been accepted with reluctance by ordinary common sense; in the domain of electricity these forces now appeared to be dethroned from their position by simple and striking experiments.

Though in the last-mentioned experiments my research had, in a certain sense, come to its natural end, I still felt that there was one thing wanting. The experiments related only to the propagation of the electric force. It was desirable to show that the magnetic force was also propagated with a finite velocity. According to theory it was not necessary for this purpose to produce special magnetic waves; the electric waves should at the same time be waves of magnetic force; the only important thing was to really detect in these waves the magnetic force in the presence of the electric force. I hoped that it would be possible to do this by observing the mechanical forces which the waves exerted upon ring-shaped conductors. So experiments were planned which (for other reasons) were only

[1] These experiments gave occasion to the lecture "On the Relations between Light and Electricity," which I delivered to the *Naturforscherversammlung* at Heidelberg in 1889, and in which I gave a general account of my experiments in a popular form (published by E. Strauss, Bonn).

carried out later on, and then incompletely; these are described in the last experimental research "On the Mechanical Action of Electric Waves in Wires."

Casting now a glance backwards we see that by the experiments above sketched the propagation in time of a supposed action-at-a-distance is for the first time proved. This fact forms the philosophic result of the experiments; and, indeed, in a certain sense the most important result. The proof includes a recognition of the fact that the electric forces can disentangle themselves from material bodies, and can continue to subsist as conditions or changes in the state of space. The details of the experiments further prove that the particular manner in which the electric force is propagated exhibits the closest analogy [1] with the propagation of light; indeed, that it corresponds almost completely to it. The hypothesis that light is an electrical phenomenon is thus made highly probable. To give a strict proof of this hypothesis would logically require experiments upon light itself.

What we here indicate as having been accomplished by the experiments is accomplished independently of the correctness of particular theories. Nevertheless, there is an obvious connection between the experiments and the theory in connection with which they were really undertaken. Since the year 1861 science has been in possession of a theory which Maxwell constructed upon Faraday's views, and which we therefore call the Faraday-Maxwell theory. This theory affirms the possibility of the class of phenomena here discovered just as positively as the remaining electrical theories are compelled to deny it. From the outset Maxwell's theory excelled all others in elegance and in the abundance of the relations between the various phenomena which it included. The probability of this theory, and therefore the number of its adherents, increased from year to year. But as long as Maxwell's theory depended solely upon the probability of its results, and not on the certainty of its hypotheses, it could not completely displace the theories which were opposed to it. The fundamental hypotheses of Maxwell's theory contradicted the usual views,

[1] The analogy does not consist only in the agreement between the more or less accurately measured velocities. The approximately equal velocity is only one element among many others.

and did not rest upon the evidence of decisive experiments. In this connection we can best characterise the object and the result of our experiments by saying: The object of these experiments was to test the fundamental hypotheses of the Faraday-Maxwell theory, and the result of the experiments is to confirm the fundamental hypotheses of the theory.

B. Theoretical

And now, to be more precise, what is it that we call the Faraday-Maxwell theory? Maxwell has left us as the result of his mature thought a great treatise on Electricity and Magnetism; it might therefore be said that Maxwell's theory is the one which is propounded in that work. But such an answer will scarcely be regarded as satisfactory by all scientific men who have considered the question closely. Many a man has thrown himself with zeal into the study of Maxwell's work, and, even when he has not stumbled upon unwonted mathematical difficulties, has nevertheless been compelled to abandon the hope of forming for himself an altogether consistent conception of Maxwell's ideas. I have fared no better myself. Notwithstanding the greatest admiration for Maxwell's mathematical conceptions, I have not always felt quite certain of having grasped the physical significance of his statements. Hence it was not possible for me to be guided in my experiments directly by Maxwell's book. I have rather been guided by Helmholtz's work, as indeed may plainly be seen from the manner in which the experiments are set forth. But unfortunately, in the special limiting case of Helmholtz's theory which leads to Maxwell's equations, and to which the experiments pointed, the physical basis of Helmholtz's theory disappears, as indeed it always does, as soon as action-at-a-distance is disregarded. I therefore endeavoured to form for myself in a consistent manner the necessary physical conceptions, starting from Maxwell's equations, but otherwise simplifying Maxwell's theory as far as possible by eliminating or simply leaving out of consideration those portions which could be dispensed with, inasmuch as they could not affect any possible phenomena. This explains how the two theoretical papers (forming the conclusion of this collection) came to be written. Thus the representation of the theory in Maxwell's own work, its representation as a limiting case of Helmholtz's theory, and its representation in the present dissertations—however different in form—have substantially the same inner significance. This common significance of the different modes of representation (and others can certainly be found) appears to me to be the undying part of Maxwell's work. This, and not Maxwell's peculiar conceptions or methods, would I designate as "Maxwell's Theory." To the question, "What is Maxwell's theory?" I know of no shorter or more definite answer than the following:—Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory; every theory which leads to different equations, and therefore to different possible phenomena, is a different theory. Hence in this sense, and in this sense only, may the two theoretical dissertations in the present volume be regarded as representations of Maxwell's theory. In no sense can they claim to be a precise rendering of Maxwell's ideas. On the contrary, it is doubtful whether Maxwell, were he alive, would acknowledge them as representing his own views in all respects.

The very fact that different modes of representation contain what is substantially the same thing, renders the proper understanding of any one of them all the more difficult. Ideas and conceptions which are akin and yet different may be symbolised in the same way in the different modes of representation. Hence for a proper comprehension of any one of these, the first essential is that we should endeavour to understand each representation by itself without introducing into it the ideas which belong to another. Perhaps it may be of service to many of my colleagues if I here briefly explain the fundamental conceptions of the three representations of Maxwell's theory to which I have already referred. I shall thus have an opportunity of stating wherein lies, in my opinion, the especial difficulty of Maxwell's own representation. I cannot agree with the oft-stated opinion that this difficulty is of a mathematical nature.

When we see bodies acting upon one another at a distance, we can form for ourselves various conceptions of the nature of this action. We may regard the effect as being that of a direct action-at-a-distance, springing across space, or we may regard it as the consequence of an action which is propagated from point to point in a hypothetical medium. Meanwhile, in applying these conceptions to electricity, we can make a series of finer distinctions. As we pass from the pure conception of direct attraction to the pure conception of indirect (*vermittelten*) attraction, we can distinguish between four standpoints.

From the first standpoint we regard the attraction of two bodies as a kind of spiritual affinity between them. The force which each of the two exerts is bound up with the presence of the other body. In order that force should be present at all, there must be at least two bodies present. In some way a magnet only obtains its force when another magnet is brought into its neighbourhood. This conception is the pure conception of action-at-a-distance, the conception of Coulomb's law. In the theory of electricity it has almost been abandoned, but it is still used in the theory of gravitation. Mathematical astronomy speaks of the attraction between the sun and a planet, but with attraction in empty space it has no concern.

From the second standpoint we still regard the attraction of the bodies as a kind of spiritual influence of each upon the other. But although we admit that we can only notice this action when we have at least two bodies, we further assume that each of the acting bodies continually strives to excite at all surrounding points attractions of definite magnitude and direction, even if no other similar bodies happen to be in the neighbourhood. With these strivings, varying always from point to point, we fill (according to this conception) the surrounding space. At the same time we do not assume that there is any change at the place where the action is exerted; the acting body is still both the seat and the source of the force. This is about the standpoint of the potential theory. It obviously is also the standpoint of certain chapters in Maxwell's work, although it is not the standpoint of Maxwell's theory. In order to compare these conceptions more easily with one another, we represent from this standpoint (as in Fig. 2) two oppositely electrified condenser-plates. The diagrammatic representation will be easily understood; upon the plates are seen the positive and negative electricities (as if they were material); the force between the plates is indicated by arrows. From this standpoint it is immaterial whether the space between the plates is full or empty. If we admit the existence of the light-ether, but suppose that it is removed from a part *B* of the space, the force will still remain unaltered in this space.

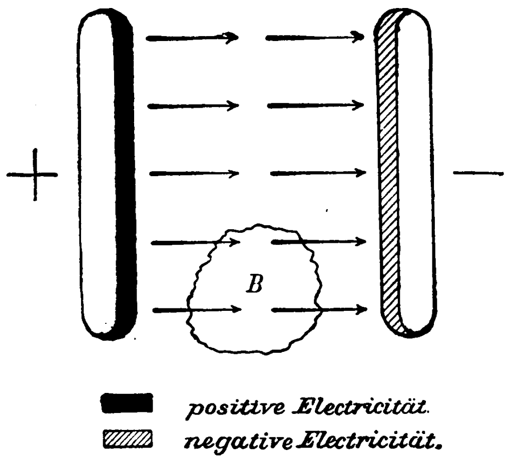

Fig. 2. [II.]

The third standpoint retains the conceptions of the second, but adds to them a further complication. It assumes that the action of the two separate bodies is not determined solely by forces acting directly at a distance. It rather assumes that the forces induce changes in the space (supposed to be nowhere empty), and that these again give rise to new distance-forces (*Fernkräften*). The attractions between the separate bodies depend, then, partly upon their direct action, and partly upon the influence of the changes in the medium. The change in the medium itself is regarded as an electric or magnetic polarisation of its smallest parts under the influence of the acting force. This view has been developed by Poisson with respect to statical phenomena in magnetism, and has been transferred by Mosotti to electrical phenomena. In its most general development, and in its extension over the whole domain of electromagnetism, it is represented by Helmholtz's theory.[1]

Fig. 3 illustrates this standpoint for the case in which the medium plays only a small part in the resultant action. Upon the plates are seen the free electricities, and in the parts

[1] At the end of the paper "On the Equations of Motion of Electricity for Conducting Bodies at Rest."—*Ges. Abh.* **1**, p. 545.

of the dielectric the electrical fluids which are separated, but which cannot be divorced from each other. Let us suppose that

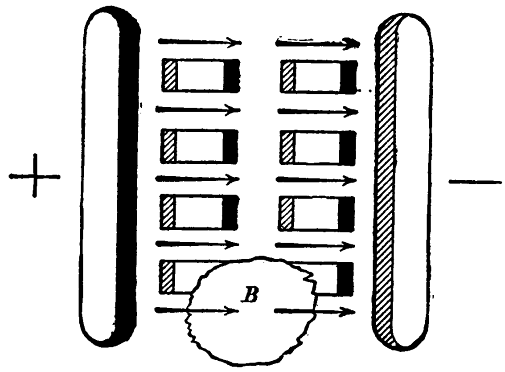

Fig. 3. [IIIa.]

the space between the plates contains only the light-ether, and let a space, such as B, be hollowed out of this; the forces will then remain in this space, but the polarisation will disappear.

One limiting case of this mode of conception is of especial importance. As closer examination shows, we can split up the resultant action (which alone can be observed) of material bodies upon one another into an influence due to direct action-at-a-distance, and an influence due to the intervening medium. We can increase that part of the total energy which has its seat in the electrified bodies at the expense of that part which we seek in the medium, and conversely. Now in the limiting case we seek the whole of the energy in the medium. Since no energy corresponds to the electricities which exist upon the conductors, the distance-forces must become infinitely small. But for this it is a necessary condition that no free electricity should be present. The electricity must therefore behave itself like an incompressible fluid. Hence we have only closed currents; and hence arises the possibility of extending the theory to all kinds of electrical disturbances in spite of our ignorance of the laws of unclosed currents.

The mathematical treatment of this limiting case leads us to Maxwell's equations. We therefore call this treatment a form of Maxwell's theory. The limiting case is so called also by v. Helmholtz. But in no sense must this be taken as meaning that the physical ideas on which it is based are Maxwell's ideas.

Fig. 4 indicates the state of the space between two electrified plates in accordance with the conceptions of this theory. The distance-forces have become merely nominal. The electricity on the conductors is still present, and is a necessary part of the conception, but its action-at-a-distance is completely neutralised by the opposite electricity of the medium which is displaced towards it. The pressure which this medium exerts, on account of the attraction of its internal electrifications, tends to draw the plates together. In the empty space B there are present only vanishingly small distance-forces.

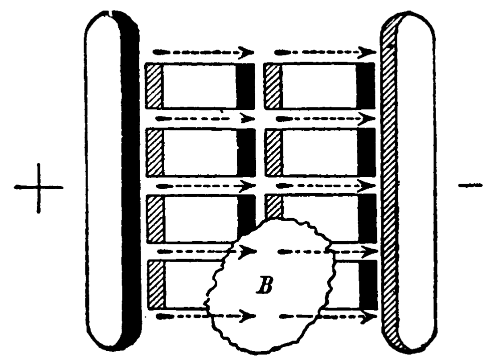

Fig. 4. [IIIb.]

The fourth standpoint belongs to the pure conception of action through a medium. From this standpoint we acknowledge that the changes in space assumed from the third standpoint are actually present, and that it is by means of them that material bodies act upon one another. But we do not admit that these polarisations are the result of distance-forces; indeed, we altogether deny the existence of these distance-forces; and we discard the electricities from which these forces are supposed to proceed. We now rather regard the polarisations as the only things which are really present; they are the cause of the movements of ponderable bodies, and of all the phenomena which allow of our perceiving changes in these bodies. The explanation of the nature of the polarisations, of their relations and effects, we defer, or else seek to find out by mechanical hypotheses; but we decline to recognise in the electricities and distance-forces which have hitherto passed current a satisfactory explanation of these relations and effects. The expressions electricity, magnetism, etc., have no further value for us beyond that of abbreviations.

Considered from the mathematical point of view, this fourth mode of treatment may be regarded as coinciding completely with the limiting case of the third. But from the physical point of view the two differ fundamentally. It is impossible to deny the existence of distance-forces, and at the same time to regard them as the cause of the polarisations. Whatever we may designate as "electricity" from this standpoint does not behave like an incompressible fluid. If we consider Fig. 5, which brings symbolically before us the view presented from this standpoint, we are struck by another distinction. The polarisation of the space is represented by the