# THE PRINCIPLES OF ELECTRICITY

NORMAN R. CAMPBELL M.A

THE PEOPLE'S BOOKS

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By NORMAN R. CAMPBELL, M.A.

AUTHOR OF "MODERN ELECTRICAL THEORY"

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# PREFACE

This little book is an attempt to illustrate, by means of the fundamental laws and theories of the science of electricity, some of the chief principles involved in all scientific investigation. It is intended for the reader of general interests; it demands of him no previous knowledge whatever of the facts concerned, but it does demand close attention and careful thought; it does not aim at providing a light half-hour's reading, but at satisfying some of the needs of those who really want to know.

In a volume of so small a size and so large a scope little attention can be paid to details. It is hoped that none of the statements made are positively misleading in important matters, but no effort has been made to attain the minute accuracy necessary in a textbook. On the other hand, the greatest care has been taken to avoid all ambiguities of statement or confusions of thought.

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# The Principles of Electricity

## CHAPTER I

### THE LAWS AND THEORY OF ELECTROSTATICS

**1. "What is Electricity?"**—Every one who has devoted any considerable time to the study of physics is probably familiar with a question put to him by his unscientific friends. "What is electricity?" they ask, and when he is obviously at a loss to provide them with the perfectly direct and straightforward answer which they expect, they endeavour with more or less politeness to conceal their opinion that after all he knows very little more about science than they do. As a matter of fact, however, it is the asking of the question and not the failure to answer it which displays ignorance of science; the more a man knows about science, the more impossible he would find it to answer that question, for the more clearly he would realise that the question, put in that form, is unanswerable. Nevertheless, the inquirer is probably asking for information which he can receive without undergoing a course of profound study. This little book is an attempt to give him such an answer. But since his original question showed that he is not merely ignorant of science, but has definite preconceived notions about it which are positively wrong, there is some fear that, if I proceeded to set forth, as in a textbook, the ideas which men of science have developed in order to enable them to deal with electrical phenomena, he would misunderstand a great many of the statements made and be confirmed in a great many erroneous notions. It is desirable, therefore, to begin with a brief explanation as to what science in general means, what kind of inquiries it makes, and what kind of answers it gives. It is only after such an introduction that any special branch of science can be expounded with any usefulness to our imaginary inquirer.

**2. The Foundations of Science.**—A textbook of electricity might possibly open with some such words as these: "It was observed by the Greeks that a piece of amber rubbed by the hand acquired the property of attracting light bodies in its neighbourhood." And this statement of the peculiar properties of amber will serve excellently as an example of one of the great classes of scientific propositions which we shall have to consider.

It will be noticed that the statement says that something has been observed, and in this instance the observations which are recorded may appear so simple that no further inquiry into their nature can be made. But a very little consideration will show that the matter is not quite so simple as it appears at first sight. Let us imagine ourselves encountered by a sceptic who says that the statement does not to him appear perfectly simple and that he wants to know what it means, and that his ignorance is not merely based on the fact that he speaks a different language. He will probably ask us first what we mean by "amber." It is not difficult to make some reply; "amber," we may say, "is a yellow, brittle, hard substance found near the sea, and so on." He now inquires what we mean by "yellow," "brittle," and "hard." We can easily tell him what we mean by "brittle"; we mean that if we hit the substance with a hammer it will not flatten out, but will break in pieces. With a little more difficulty we can explain the meaning of "hard"; we mean that if we place our fingers on the amber we cannot bring them into contact. But when he asks what we mean by "yellow," we can give him no answer whatsoever; we feel that if he does not know what yellow means, even when we have translated the word into all the tongues of the earth, we can do nothing to enlighten him. If, however, he is satisfied with "yellow," the conversation may proceed, and he may usefully push his questions further. He may, for instance, ask us what we mean by a "hammer" and by "breaks." We shall again have to consider whether it is possible to explain these ideas further, or whether they, like "yellow," are so simple that no further analysis of them is possible. And so we may imagine the dialogue to proceed, until he has extracted from us all the explanations which we feel it is possible to give; he will have forced us to analyse our original statement into a considerable number of other statements, and these again into yet others, until each process of analysis leads to ideas which are so perfectly simple that they can be explained no further.

3. **Laws.**—Any one who will try to carry out such a process of analysis completely will be soon disabused of the notion that the original statement was perfectly simple; he will find that the ideas contained in it are extraordinarily complex. And this is one of the conclusions which the example was taken to illustrate. The other conclusion is not so easily attained; it could not be established completely without effecting the entire analysis, a task of gigantic difficulty. It concerns the nature of the ultimate statements and the ultimate ideas which would be reached by the analysis, the statements and ideas which, like those concerned with "yellow," are so simple that they cannot be explained further. It is generally believed to-day that, if the analysis could be effected, all the ultimate statements would be, like the statement "this is yellow," statements concerning sensations—statements, that is to say, that something is known immediately through the organs of sense, that a colour is seen, a sound heard, or a muscular effort felt. It is clear, at least, that statements of this nature are ultimate and incapable of further explanation; it is impossible to make any one

understand what we mean by saying that we hear a high note, or that we try to move our arm, as it is to make them understand what we mean by saying that something is seen to be yellow.

Such a scientific statement, then, as that which has been taken as an example, consists of a complex collection of simple statements about the occurrence of sensations; it can be analysed into a series of such statements, and by the truth or falsity of them the truth or falsity of the complete statement is to be judged. This conclusion will be perfectly familiar to any one who knows the trend of recent thought concerning the principles of scientific knowledge; our example is what is usually termed a scientific "law," and a "law" has often been defined as a description of the sequences of sensations. The use of the word sequences in that definition draws attention to a feature of the law, which is of only secondary importance for our present purpose, but still should not be completely overlooked.

**4. The Nature of Laws.**—The proposition about the rubbed amber is a collection of simple statements about the occurrence of sensations, but it is not a mere collection; the statements are arranged in a definite order, and connections between them are asserted. Thus the statement that "amber has been rubbed" represents part of the collection, and the statement that "amber attracted light bodies" another part; the whole statement does not merely affirm both of these partial statements, but asserts also a connection between them, in that the first sensations represented by the first partial statement occurred before those represented by the other; the amber attracted *after* it was rubbed. This kind of connection between the ultimate statements, in which one set of sensations is affirmed to occur before another, is the one which has attracted far the largest share of the attention of writers on the philosophy of science, and the term "law" is usually confined to propositions in which this connection is very obvious.

But it is by no means the only kind of connection which is asserted in scientific statements. Thus the statement that amber is rubbed, implies the statement that there is such a thing as amber, and this statement in its turn, as we have seen, means that there is a thing which is yellow, hard, brittle, and found by the sea. This statement may be again resolved into partial statements, such as there is a thing which is hard, that there is a thing which is brittle, and so on, each of which is a collection of statements of the occurrence of sensations. But here the connection between the partial statements affirmed by the complete statement, is not that one series of sensations occurs after the other; a substance is called amber if it is first observed to be hard and then observed to be brittle, or if the order of these observations is reversed. If, however, we proceed yet further and analyse the statement that this is brittle, we find that it means that after it is hit with a hammer it breaks into pieces; the connection of statements concerning the occurrence of sensations by dividing them into two groups, of which one is subsequent to the other, reappears.

To attempt to analyse and describe all the various kinds of connections between statements affirming the occurrence of sensations which are made in such a proposition as we have taken as our example, would lead us much too far afield and would not serve our immediate purpose. But it is important to notice that the connection of invariable sequence is by no means the only one possible, and that it is very seldom, if ever, the only one occurring even in those statements which are universally recognised as laws. In these circumstances, the attempt to confine the term "law" to those propositions in which this connection is especially obvious, appears to me artificial and misleading; and in the sequel that term will be applied indifferently to any proposition asserting any kind of general connection between the occurrence of sensations. We shall term "laws" not only such propositions as that, when amber is rubbed, it attracts light bodies, but also such statements as that there is such a substance as amber, which is at the same time yellow, brittle, and the rest.

We are now ready to leave, for the present, these general considerations of the nature of scientific propositions, and to consider more closely the special class of those propositions which are indicated by the title of this little volume. The bearing of all the preceding exposition will not be immediately obvious; it has been necessary, not so much in order to render comprehensible the electrical laws which are about to be described, but in order to enable us at a later stage to contrast these laws with a different kind of scientific proposition with which they are only too often confused. In the next few paragraphs we shall be concerned with laws only; but since it should be clear from what has been said that, in order to determine whether a proposition is or is not a law, a complete analysis of it must be made, considerations of space forbid me to attempt to prove that all the propositions actually given are laws. For the truth of that statement all who are not thoroughly familiar with the science will have to rely upon the judgment of the author.

**5. The Laws of Electrostatics.**—The facts which we are about to notice are familiar to every one who has ever opened a textbook of electricity or heard a popular lecture; but nevertheless it is important to state them rather carefully with a view to our further discussion. They may be summed up in the following propositions, which represent the chief laws on which the science of electricity is based.

If several pieces of glass are rubbed with a corresponding number of pieces of silk under suitable conditions, the glass and the silk will be found to have acquired the following properties :—

(1) A piece of glass and a piece of silk attract one another.

(2) Two pieces of silk or two pieces of glass repel one another.

(3) A piece of glass or a piece of silk attracts any other body with which it has not been in contact. In all these cases the attraction or repulsion is less, the greater the distance between the attracting or repelling bodies.

(4) A third body which has been in contact with a piece of glass or a piece of silk acquires to some extent the properties, enumerated in (1), (2), (3), of the glass or silk with which it has been in contact. And the glass or silk with which the third body has been in contact loses to some extent those properties—that is to say, it attracts or repels with less force than before.

(5) These properties may be acquired by a third body, not only by direct contact with the glass or silk, but also by being connected to them by a rod composed of certain substances. Substances may be divided, roughly, into two classes with respect to this action. One class (A), of which the metals are the most prominent members, are such that they transmit the properties of the glass to a third body, when they are in contact with both; while the other class (B), which includes most other solid substances, cannot transmit the properties under such conditions.

(6) Bodies of class (A), but not bodies of class (B), can acquire the properties of the glass or silk by yet another method which requires no contact with the glass or silk. If the glass be brought near one end of a body of class (A), while the other end is touched momentarily with the hand, it is found on the removal of the glass that the body has acquired to some extent the properties of the rubbed silk. If in that statement the words glass and silk are interchanged the statement remains true.

Such are the facts, stated in such a way as to avoid all irrelevant complications, from the observation of which the science of electricity sprang. It will be convenient, merely for brevity, to introduce a few technical terms, all of which are probably familiar to the reader. The glass or silk when showing the properties described in (1), (2), (3), in consequence of being rubbed, is said to be "charged"; bodies of classes (A) and (B) are called "conductors" and "non-conductors" respectively; the method of charging a body described in (6) is called "charging by induction."

Now, of course, these are not nearly all the laws which have been discovered by the study of such phenomena. There are not only many other laws known of the same nature as those which have just been stated, describing similar properties when bodies other than glass and silk are rubbed together, but also many laws of a wholly different nature have been discovered. For instance, the reader probably knows that measurement plays an important part in electrical science, and that laws can be stated which, unlike those just given, are quantitative and not merely qualitative; we know not only that the attractive force between two charged bodies decreases as the distance between them increases, but also that there is a definite numerical relation between the distance and the attractive force. Such new laws will engage our attention presently; those which have just been stated differ from all the rest in their origin as well as in their nature, and it is of the utmost importance that this distinction should be made clear before we proceed any further. The laws which were given in the previous paragraph are the only laws which were discovered purely by the process of experiment and observation, which is usually supposed to be the source of all laws; the other laws were discovered only after the study of these phenomena had undergone a new development, of which no mention has been made hitherto.

6. **A New Development.**—This new development can be viewed under two entirely different aspects. From one point of view it consists in establishing relations between the laws which have already been discovered. We have seen that a law is a description of the sensations which go to make up observations; the process of establishing a law consists in finding one brief and concise statement which sums up all the ulti-

mate statements of the occurrences of sensations; and, once the law has been established, there can be deduced from it all those simpler statements of the occurrences of sensations. Thus from the statement that rubbed glass and silk attract one another, we can deduce the statement that any given piece of glass will attract any given piece of silk with which it has been rubbed; and further, as was shown before, we can deduce that there is a substance which is hard, transparent, and the rest, because if there were no such thing as glass the statement of what happened when it was rubbed would clearly be meaningless. In establishing laws, then, we are concerned to find relations between the ultimate statements of the occurrence about observations, and the relations which we find are such that, given the law which expresses the relations, we can deduce the observations.

And now we may ask how far will this process of establishing laws carry us; can we ultimately attain one law which will sum up *all* the observations which we have made and from which all those observations can be deduced, or shall we reach a stage when several laws are needed to describe all the observations and find that no further progress in the simplification of the description can be made? To answer this question thoroughly would need much more space and much more searching inquiry than is suitable in this place, but I think that there can be no doubt that the complete simplification, resulting in the description of all the observations by one single statement, cannot be effected if that statement is to be a law. We can reduce the number of laws somewhat further; we can, for instance, combine (5) and (6) into one law by saying that only those bodies which can transmit the properties of a charged body can be themselves charged by induction; but when all possible combinations of this kind have been effected, we shall be still left with several laws between which no connection can be established.

But it is yet possible that, though the connection of all the laws into one statement which shall be itself a law is not possible, yet they can be connected into one statement which is not a law, but some other kind of proposition. And such a further development is possible, and is of the utmost importance in scientific investigation; it is the development which we now propose to consider. But as yet we have no clue whatsoever to the kind of proposition which will be suitable for this purpose; the general problem of finding a proposition which will sum up a number of other propositions is quite indeterminate, until we know more of the nature of the proposition which is desired. We shall get a hint as to this nature if we view the proposed development under its other aspect.

**7. The Aim of Science.**—Why do people study science at all? What is the object of all these attempts to simplify the description of sensations? Why, indeed, do we want to describe our observations and sensations at all? We may leave out of account the answer that science is studied for utilitarian reasons, that is, because the study of science helps us to control in some measure the sensations which we experience, and thus to promote our bodily comfort. Of course there are many people who do study some branches of science for that reason, but it is not the reason which has led to any of the developments of pure science; and it is with the purest and most abstract science that we are here concerned. The student of pure science pursues his investigations simply because he wants to know, because the result of his work, when it is successful, gives him a certain indefinable intellectual pleasure similar to that which other men gain by the reading of great literature or the sight of great paintings; he seeks not the pleasures of the body, but the pleasures of the mind.

Now, such a student is forced to a new development of science beyond the formulation of laws, because the laws, even when he has got them, does not give him the intellectual satisfaction he seeks; he cannot accept them willingly as the end of his labours. It is quite impossible to say why he is not content with laws, just as it is impossible in the last resort to give any reason for an artistic preference, and fortunately there is no need to make the attempt. For my reader is supposed to be the plain man, and nobody feels more strongly than he the unsatisfying character of laws as an ultimate result of science; I can appeal to his own experience. If this little treatise were brought to a conclusion now, and the reader offered only the laws of p. 5 as the whole pronouncement of science on a great field of investigation, I think he would feel not only that the results were extraordinarily meagre, but that they were of the wrong kind. His natural instinct, unless it had been perverted by the mistaken admonitions of some people who ought to know better, would make him inquire "why?" "These laws are all very well," he would say, "but I have been expecting to be told why, for instance, charged bodies are able to attract uncharged, or only bodies which are conductors can be charged by induction." That request voices just the need which leads men of science to their greatest discoveries.

The question, however, put in the form "why?" does not enlighten us much as to the nature of the answer desired. For a little consideration will show that all the forms of answer which are given in ordinary discourse to a question beginning thus are clearly inapplicable to any question about the laws which have been discovered by observation. But the request for further information is sometimes put in another way; the inquirer says that he wants the laws "explained." Here, again, there is some vagueness, for "explanations" can be of many various kinds, of which most are inapplicable in this instance; but all explanations have one feature in common, they replace that which is explained by some ideas or some words which are more familiar. And it is an explanation in this sense of the word, a replacement of unfamiliar ideas by familiar, which is desired and which is offered by the new development of science which we are considering.

The fresh step, then, which we are going to take consists in the substitution for the laws which we have discovered of some other proposition or propositions which shall not be laws. And these new propositions have to fulfil two purposes: first, they have to be such that the laws can be deduced from them, and such that they sum up the laws as the laws sum up the individual observations; second, they have to be such that they give the intellectual satisfaction which cannot be obtained from the laws; for this latter purpose they will have to contain ideas which are more familiar than those of the laws.

Even with this specification of the problem to be solved it is clearly impossible to solve it off-hand; there is no sort of method by which we may set about the solution as we might about the solution of a problem in arithmetic. There is no use in spending space and time in considering various possibilities; we will proceed at once to consider the explanation which has been offered. There may be other explanations which would fulfil both purposes equally well, but if there are, it is not our business to discuss them. We must not lose sight of the question from which we started—"What is electricity?"; the explanation which is now offered is the only one which tells us anything about electricity.

**8. The "Fluid" Theory.**—This explanation is based on an analogy. We fix our attention on the facts described in (4), that an uncharged body acquires some of the properties of a charged body by simple contact with it. There are many obvious instances of such an action in other departments of observation. Thus, if we bring our hands into contact with a sponge saturated with water, they acquire some of the properties of the sponge; they become wet. In this instance further investigation shows that the communication of the properties of the sponge to our hands is due to the transfer of a "substance" water, from the sponge to our hands. Analogy suggests then—I put the suggestion at present in the form in which it would usually be made—that the communication of the properties of the charged to the uncharged body on contact is due to the transfer of some substance from the uncharged to the charged body, and that the properties which we imply by saying that the body is charged represent the presence in it of some of this supposed substance, to which the name "electricity" is given. If this suggestion is accepted, an examination of the properties which distinguish a charged from an uncharged body will give us information as to the "properties of electricity." Since there are two kinds of charged bodies, charged glass and charged silk, we must suppose that there are two different substances concerned, "glass-electricity" and "silk-electricity." Laws (1), (2), (3) show that two portions of glass-electricity or two portions of silk-electricity repel each other, while a portion of glass-electricity and a portion of silk-electricity attract each other. In order to represent on our analogy the fact that a charged body attracts one that is uncharged, we must make some supposition as to the state of an uncharged body with regard to electricity. The most natural supposition to make is that an uncharged body contains no electricity; but then our analogy will furnish no representation of the fact that by rubbing together two bodies which contain no electricity we cause electricity to appear in each of them. Another supposition is that an uncharged body contains both glass-electricity and silk-electricity in such proportions that each neutralises the effect of the other, and that the effect of rubbing is to separate the two kinds, causing one to adhere to the glass and the other to the silk; such an action might possibly be observed if the sponge were charged with two different liquids. On this supposition two uncharged bodies would not attract each other, because the attraction of the glass-electricity in one for the silk-electricity in the other would be counteracted by the repulsion of the silk-electricity in the one for the silk-electricity in the other. But if a body charged with glass-electricity is brought near an uncharged body, it will attract the silk-electricity and repel the glass-electricity. If now we add the further supposition that the electricities can move to some extent within the body, the silk-electricity will collect in the parts near the charged body and the glass-electricity in the parts remote from it; the former will be attracted more strongly, the latter repelled less strongly, so that on the whole there will be an attraction.

We have now "explained" laws (1), (2), (3), (4); let us consider (5) and (6). The analogy again offers a suggestion. If we put one end of a solid rod in contact with the sponge, we shall not be able to wet our hands by touching the other end; but if we replace the rod by a tube we shall be able to do so. Accordingly, we suggest that conductors correspond to tubes; they are bodies along which electricity can flow freely; while non-conductors are bodies in which it cannot move freely.[1]

And this suggestion immediately gives us the clue to (6). For the presence of the charged glass at one end of an uncharged body causes, as we have seen, silk-electricity to collect at that end and glass-electricity at the other. If the body is a conductor and we touch the far end of it, the glass-electricity, being able to move freely, flows away still further from the charged body into our hand, leaving only silk-electricity in the body. If it is a non-conductor, the glass-electricity cannot flow away, so that when the charged glass is removed the body still possesses its original proportions of glass- and silk-electricity; it still appears uncharged.

**9. Theories.**—Such is the new development of our

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[1] The careful reader will note that there is a little difficulty here, for in order to explain the attraction of uncharged bodies, conductors or non-conductors, by charged bodies, we have supposed that the electricity can move within any body; while now we suppose that it can only move in conductors. The discrepancy cannot be cleared up without recourse to the ideas—quite modern ideas—which are beyond our scope.

science, the explanation which is offered. I think the reader will agree that it is of the kind for which he hoped, and that it does throw a new and valuable light upon the phenomena under consideration. Indeed, it is doubtless perfectly familiar to him, and appears so obvious that care was needed in stating the laws to avoid any implicit reference to it; but it should be clear that it does represent a new development, and that it is perfectly possible to state the laws of the phenomena without any reference whatsoever to it. The help which it affords is doubtless due to the fact that it reduces the quite unfamiliar actions observed with the charged glass and silk to the quite familiar action of the transference of a substance from one body to another.

An explanation of the kind sketched in § 8 I propose to term a "theory." That word is generally used as loosely in science as in ordinary discourse, and, as usual, the looseness in terminology is due to a looseness of thought. The true nature of the explanations of laws which science offers has not been recognised, and much confusion has been introduced by a failure to perceive clearly the essential difference between such explanations and the laws which they explain. My use of the term "theory" will not always coincide with that adopted in many scientific treatises, but it appears to me that most writers do not attach any definite meaning to the word, but use it to denote many wholly different kinds of scientific proposition.

And now we must proceed to consider rather more closely the exact nature of a theory and its relation to the laws which it explains. We have noted that if the theory is to be satisfactory the laws must be deducible from the theory; let us see how the deduction can be effected. In ordinary language the theory which has been given might be stated in some such terms as these: "The peculiar properties of charged bodies are due to the presence in them of an excess of one of two fluids, which we call electricity. These fluids are such that each attracts a portion of the other and repels any other portion of the same kind; the fluids can move freely through substances of class (A), but not through substances of class (B)." These two sentences have a rather different significance. The second sentence states the "properties of electricity"; it tells us exactly what is the nature of the new ideas which are introduced by the theory. The first sentence does not tell us anything about electricity; it only tells us what is the connection between electricity and the phenomena which we observe; it is a statement which enables us to connect the new idea, electricity, with the ideas involved in the laws which the theory was meant to explain.

Now every theory consists in the same way of two parts, one of which describes the new ideas introduced by the theory, and the other of which enables us to translate, as it were, statements in terms of these ideas of the theory into statements in terms of the ideas of laws. This second part is a kind of dictionary; in the case we have taken it consists of the statement that when we say that a body contains an excess of one of the two electrical fluids we mean exactly the same thing as when we say that the body shows the peculiar properties of charged glass or silk which have been described; and this statement is of exactly the same nature as that which might be contained in a French dictionary to the effect that, when we say "Cela est jaune," we mean the same thing as when we say "That is yellow." And it is only by the use of this dictionary that laws can be deduced from theories. Thus the assertion of our theory that opposite fluids attract each other, and that they can move freely through metals, leads, of course, directly to the conclusion that if we join by a metal two bodies containing opposite fluids those fluids will mingle and so neutralise each other's action. But this conclusion does not mean anything which can be stated in a law, till the dictionary is used to translate "containing opposite fluids" into "charged like glass and silk"; after that translation is effected, we get part of the law stated in (5).

In one aspect, then, a theory, consisting of an assertion about some new ideas and a dictionary enabling statements in terms of those ideas to be translated into statements about observations, is, as we designed from the start, simply equivalent to the laws which it explains; it merely states those laws over again in a different form of words which, for certain reasons, we find more convenient and satisfying. And in so far as the theory states anything about observations at all, it states simply the observations contained in those laws; no other observations have been considered in framing the theory. Again, it must be noted (and this is the point on which great emphasis must be placed) it is only by the use of the dictionary that statements about observations can be deduced from the theory at all; statements so deduced which cannot be translated by the dictionary do not assert anything about observations. Thus our theory states that "electricity is a fluid." Turning to our dictionary we find no entry which enables us to translate that statement; we can translate the statement that a body contains the fluid electricity, but not the statement that electricity is a fluid. Consequently that statement means nothing at all which can be stated in terms of observation; if it means anything at all, what it means is not that certain observations have been made. And if, as we shall, we attribute any meaning whatsoever to that statement, we shall have to admit that something can be significantly said about electricity which cannot be stated in terms of observation, and, consequently, that the idea "electricity" is not completely describable in terms of observation. The neglect of this obvious conclusion has led to endless trouble.

**10. The Value of Theories.**—And now we must view theories in the aspect which makes them at the same time valuable and dangerous. It would not have been necessary to expound at such length what a theory does not state, if there had not been a risk arising from the fact that it appears to assert more than it actually does;

a theory suggests a great deal more than it actually asserts. When we say that "electricity is a fluid having certain properties," we cannot help suggesting, both to ourselves and to our hearers, that electricity is something which has not only the properties which we have definitely assigned to it, but also all those properties which are common to the other things which we sometimes call fluids. We have called "electricity" a fluid because we have supposed that it can move with great ease through some bodies but not through others; but once we have so called it, we cannot help wondering whether, like other fluids, it has a definite weight and volume, whether it will evaporate if we heat it, or solidify if we cool it, and so on.

Now, it is not immediately obvious what these suggestions really mean; for, according to what has just been said, it is not clear that there is any significance in the statement that "electricity has weight," unless that it can be interpreted by the dictionary to express a law. As a matter of fact, of course, what is suggested is a new law; it is suggested that a body should weigh more when it contains more electricity. But it must be remembered that the new statements about electricity which are suggested should express laws, and new terms must be added to the dictionary; the need of such new terms is not very obvious in the case of some of the suggestions, but it is perfectly clear in others. Thus, a statement that "electricity has a definite volume" cannot be immediately interpreted to mean that the volume of a body is the greater the more electricity it contains; for, returning to the original analogy, the volume of the sponge is not necessarily greater when it is wet than when it is dry. We must guard carefully against concluding that such a statement has any significance until we are sure that there are terms in the dictionary by which it may be translated into the assertion of a law.

It does not follow that, because the laws summed up in the original theory are true, the laws which are further suggested by it will also be true. Whether these new laws are or are not true can only be ascertained by experiment and observation; we must see that observations can be made which are accurately described by these new laws. And it will often be found that the suggestions of the theory are false; for instance, all the suggestions which have been just made on the basis of the "fluid theory" of electricity are false. There are no laws known which are accurately stated by saying that electricity has weight or volume, that it solidifies when cooled or evaporates when heated. Now, a theory which suggests laws which are false is clearly less satisfactory than one which suggests laws which are true; the difference between actual theories in this respect is one of degree rather than kind, for almost all serious theories have suggested some true laws, and all have suggested some false ones. And it is therefore desirable that we should ascertain, if possible, how we arrive at theories which do in fact suggest true laws.

Nobody ever has suggested, and I do not think that any one ever will suggest, any sort of formal rule by which true theories—theories, that is, which suggest true laws—may be devised rather than false. Attempts have been made, as the reader is doubtless aware, to formulate rules for the invention of laws, though all those attempts appear to me completely unsatisfactory; but there has not even been an attempt to solve the same problem for theories. But some light may be thrown on the subject by reverting once more to the question as to why we form theories at all. We form them, as was said, because laws appear to us unsatisfying, because they do not satisfy the æsthetic desires of our intellects. The remarkable fact that any true theories have been attained, so that, starting out only with the object of finding a new form of verbal expression for certain laws, it has been found possible to obtain a form which expresses not only those laws but others which were not originally contemplated in the vaguest way, appears to me to show that the instinct which forces men to devise theories at all, also leads them to devise true theories. If they found by experience that they could only satisfy the intellectual desires which make them want to go behind laws by introducing notions which usually or always turned out to be misleading, they would cease to try to satisfy them; they would cease to study science. Science, as a branch of pure learning, is possible only because observations turn out to be in accordance with the needs of reason—a remark which is almost a truism.

**11. The Art of Science.**—I have spoken of "men's" intellectual needs; but men differ in respect of those needs; some prefer to study literature, some mathematics, some science. Who are the men whose intellectual needs seem thus to guide the course of natural events? The answer is obvious; they are the truly great men of science. The many philosophers who, especially during the last century, attempted to analyse and describe the "Method of Science" as an almost mechanical method of deducing results from observations, never faced the problem why it is that the great advances in science have not been made by those who were (or claimed to be) the best acquainted with the "method," but by men like Newton or Faraday whose philosophical attainments were contemptible. In their attempt to render the results of science convincing to the plain man, such writers try to conceal the fact that those results are attained by flights of imagination of which the plain man is quite incapable. There can be no true philosophy of science which does not recognise that the attainment of any valuable theory requires the presence of an intellectual element which is as personal, as incommunicable and as indescribable as that which distinguishes the work of great artists. Science in its highest form is not opposed to art; it is a form of art.

And this peculiar intellectual power is necessary to understand a theory just as much as to invent one. A theory may suggest things which are false as well as things which are true, but it is to the true and not to the false suggestions that those who have the true scientific instinct are susceptible. The false suggestions recorded on a previous page have not hindered the progress of science, for all of them (except that electricity has weight, which may not be wholly false) appear at once ridiculous to any one who has any feeling for science; it would never occur to him to take them seriously. But these false suggestions are very dangerous to those who have not that feeling, and they have given rise to a vast amount of meaningless discussion. When the plain man, and still more the philosopher, hears that men of science say that electricity is a substance, they rush at once to the conclusion that "electricity" has all the properties of other things which they call substances. The confusion is partly verbal; it is a little difficult to realise that the statement that "electricity is a substance" has not even the same kind of meaning as the statement that "amber is a substance," though we have seen that the latter is simply a description of certain observations, while the former deals with ideas which are not completely definable in terms of observations. Unscientific people are apt to complain of the technical terms which science employs; they would make far fewer errors if science employed more and not fewer such terms; there is no source of error more abundant than the use of a familiar term in a new sense. But the confusion also lies deeper; it arises out of a susceptibility to the false rather than to the true suggestions of the theory. Scientific theories are designed by and meant for scientific people; it is they alone who properly understand them; those who have not the instinct for science had better avoid them as far as possible. Science is *not* "organised common sense"; it is the most esoteric of all studies.

It is on these grounds that exception was taken at the beginning of our discussion to the form of the question "What is electricity?" for that question inevitably suggests an answer which begins, "Electricity is a substance." We shall see later that there are theories of electrical phenomena which do not admit of an answer of that kind at all; but even if the theories are accepted which lead to such an answer, the answer, if given without a great deal of explanation, is almost sure to be misleading.

Now our long introduction is ended, and we can proceed with greater confidence in avoiding gross error to consider a few of the detailed results of the study of electrical phenomena. In the succeeding paragraphs we shall again be considering laws, but now they will be laws which did not arise from mere observation, but were definitely suggested by the theory that has been propounded and were subsequently proved to be true. The most important of these laws concern the measurement of electrical magnitudes.

# CHAPTER II

## ELECTROSTATIC MEASUREMENTS

**12. What is Measurement?**—The idea of measurement is one with which every one is familiar—so familiar, indeed, that very few people have tried to analyse it. Every one recognises that there are some qualities which are measurable and some which are not, but by no means every one could give any account whatever of the essential difference between measurable and unmeasurable qualities. Thus we may have before us a large number of jugs, differing in size, weight, colour, hardness, and artistic design; of these qualities in which the jugs differ the first two are immediately recognised as measurable; every one attaches a meaning to the statement that one jug is three or five times as large or as heavy as another. The idea of measuring the colour of the jugs—that is, of expressing the difference between the colour of two jugs by the difference between numbers—would occur only to one familiar with fairly recent developments of optical science; measurements of hardness are as yet possible even to the most scientific in only a rather unsatisfactory way, while measurements of artistic design are quite impossible; nobody would attach any accurate meaning to a statement that one jug was twice as beautiful as another.

The application of numbers to distinguish objects in respect of a certain quality can have two uses. First, it may merely be convenient as a method of detailed description. The series of numbers may be regarded merely as a set of words having the very useful property that we can readily make as many of them as we please.

If a new jug were brought to our notice we might want to compare it in respect of size with one of the other jugs, and we might then say that its size was somewhere near that of the jug with the broken handle, and also near that of the jug with the blue spots. But if we had a very large number of jugs, it might be rather difficult to find a sufficient number of descriptions of this kind distinguishing the various jugs; we should find it convenient to label each jug with a numeral, and to call the first jug with the broken handle No. 1, that with the blue spots No. 2, and so on; however many jugs we had we could never run short of descriptions of this kind.

This use of numbers is familiar in ordinary life in the description of houses; sensible people in a town do not call their house "Chatsworth" or "Seaview," but No. 231 Clarendon Road. This use of numbers might be applied to distinguish bodies in respect of any property whatsoever, artistic beauty as well as size; and numbers, though the most satisfactory means of distinction, are by no means the only means; if we know beforehand that the number of objects is limited, we might use letters of the alphabet. But this use of numbers, although it is the logical foundation of measurement, is not measurement proper. When we use numbers to measure and not merely to describe qualities, we mean to imply that there is some relation between the qualities which we call "2" and "3" and the quality which we call "5," which does not hold if we substitute for the quality "3" the quality "4"; this relation we express by saying that the quality "5" is the sum of the qualities "2" and "3." It is this relation which we have to consider in greater detail.

**13. The Logic of Measurement.**—What we mean when we say that the size of one jug is five times that of another is that, if we fill the first jug with water and empty it into the second carefully, repeating the process five times, we shall just fill the second jug. When we say that the weight of one jug is five times that of another, we mean that if we place on one scale of a balance five jugs exactly similar to the first, they will just balance the second. In each case the numeral five represents the number of times a certain operation has to be performed on the first jug, in order to produce the same effect as is attained by performing that operation once on the other jug. But the nature of this operation differs in different cases, and it is important to notice that the nature of the operation is not arbitrary, but must be closely defined; unless we know what is the appropriate operation, the statement that one body is in respect of some quality five times that of another conveys no information whatsoever. The question which we have to ask is why we chose one operation for the measurement of a certain quality rather than another, and how we are to determine what operation is "appropriate."

The answer to this question is to be found by considering that measurements have to be consistent with themselves; it must not be possible to perform the measurements in such a way as to arrive at two different estimates of the number representing the quality of the same object. Let us consider only size. Suppose that we have four jugs, A, B, C, D, one of which (A) is known, for some reason, to have the size represented by 1. We fill A and empty its contents carefully into B, C, and D successively till they are full; we find that it requires two fillings and emptyings to fill B, four to fill C, and six to fill D. Then, from what we have said, the sizes of B, C, and D are known to be 2, 4, 6. Now, it follows also from what has been said as to the meaning of measurement of size that, if we fill B and C and empty them both into D, D must become just full, for $2+4=6$. We try the experiment and we find that it turns out rightly; D just does become full. But this result could not possibly have been foretold; it is not a logical consequence of the definition of measurement of size which has been given and the arithmetical proposition; it could only have been foretold by observation; it is a law. If we had defined the operation appropriate to measurement differently, we should not have arrived at the same result. We might have said that the statement that the size of one jug was five times that of another meant that if we filled the second and threw it at the first, repeating the operation five times (instead of transferring the contents carefully), then the first would be just filled. But in this case we should not have found the relation between the sizes of B, C, and D which we found above; our measurements would not have been self-consistent. And there is nothing whatsoever which can be known apart from experiment to tell us which of the two operations, careful transference or random throwing, will lead to consistent results. That one operation seems to us reasonable and the other absurd is simply the result of long continued experience.

The conclusion which I am concerned to draw is that, in order that measurement shall be possible at all, detailed knowledge of certain laws must be possessed; we must find some operation which is appropriate to the measurement, in the sense that its use will always lead to consistent results. Whether any proposed operation will or will not so lead can only be known by experiment. The reason why we can measure some qualities and not others is that in the case of some and not in the case of others such an appropriate operation has been found; it has been found in the case of size and weight; it has been found, though it is far more complex, in the case of colour; it has not been found satisfactory for hardness, and it has not been found at all for artistic design.

We have not yet decided how we are to know that the size of some jug is 1. Well, we do not know it at all; we simply assume it. If we have found an "appropriate" operation, we can call the size of any jug we like 1, and our measurements will be consistent; though, of course, the numeral to be affixed to any particular jug will depend upon the choice of the jug which we choose to call 1.

To expound the matter formally, then, we have to make three statements in order to be able to measure a quality at all. (1) We say that the quantity which we are measuring is the same for two objects which are alike in some quality. Thus the size of two jugs is the same when a portion of water which fills one also fills the other. (2) We say that this quantity is 1 for some specially designated object. Thus the size of the jug with the blue spots is 1. (3) We say that when we state that the quantity for one object is the sum of the quantities for two other objects, we mean that the performance of a specified operation on the last two objects produces the same result as the performance of that operation on the first object. Thus we say that the statement that the size of one jug is the sum of the sizes of two others, means that if we pour into any other vessel the contents of the last two we shall fill the vessel to the same level as if we pour into it the contents of the first jug.

All these three statements are definitions—(1) is purely verbal, the kind of definition you find in a dictionary; (2) is purely arbitrary, the kind of definition you make when you call a dog "Spot"; (3) is essential; it must be very carefully chosen, and, in so far as it implies that measurement is possible at all which will lead to consistent results, it implies an important law which may be very complicated.

Now we have found out what measurement means, we can proceed to discuss the measurement of electrical quantities.

**14. Quantity of Electricity.**—The problem before us may be stated thus. We are given several pieces of charged glass, that is, of glass which, in the circumstances narrated before, have acquired the property of attracting light objects. The pieces do not attract a given light object in the same way; it is required to describe this difference in the properties of the pieces of glass by a difference of numbers, in such a way that the number assigned to any piece of glass is the same in whatever way, consistent with our method, the assignment is made.

We must first note that we are not starting science from the beginning. We assume that some science has been developed and some measurable quantities have already been defined. Among these quantities are lengths and forces; we cannot stop to pursue the very interesting inquiry as to how these quantities are measured. Now we can describe the differences in respect of attraction between different pieces of glass in terms of these measurable quantities which are known already; we find that, for the same piece of glass, the attraction exerted on the same light object is different, according to the distance between the glass and the object. Accordingly, some part of the differences which we are investigating can be described in this way by investigating the relation which holds between the number representing the force exerted by a given charged body on a given light object and the distance of the charged body from the object, and then noting the distances of the various charged bodies from the attracted object. But after this process has been carried out, we shall find that there are still outstanding differences; after we have allowed for the difference in distance by means of the relation discovered, or after we have ruled out differences in distance by placing all the charged bodies at the same distance from the attracted object, we still find that the attraction [1] exerted by different charged bodies is different. We have now exhausted the measurable quantities known previously, and have to invent a new one in order to measure these differences.

It is now that the theory of electricity helps us. We do not need its help to invent two of the three definitions necessary for defining a measurable quantity. The first two definitions, according to the scheme of p. 27, may be stated thus:—(1) "Two bodies are the same in respect of the quantity which we are going to measure when they exert the same attraction on the

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[1] For brevity, I shall use the word "attraction" henceforth as including "repulsion," when the two are not expressly contrasted.

same object when placed at the same distance from it."[1] (2) "This quantity is 1 for a body when, if it is placed at the distance 1 from a body for which the quantity is the same, it attracts it with a force 1." But now what is the operation which is to be made the basis of (3)? The answer which has been given was doubtless originally suggested by the theory.

We have traced an analogy between electricity and a fluid. The measurable quantity which represents the difference between two jugs in respect of the water which they contain is called the "quantity of the water" in them, and the operation appropriate for measuring it is, as we have seen, that of emptying one vessel into another. Now, in the case of electrical phenomena, there is a process which seems at first to correspond exactly to emptying one vessel into another. There are circumstances such that, if a charged body A is brought into contact with another charged body B and subsequently withdrawn, A is found to be uncharged, while the attracting properties of B are changed; B no longer attracts other bodies at a given distance with the same force as before. Analogy suggests that this operation may be "appropriate"; accordingly, we call the quantity which we are trying to define the "quantity of electricity" in the body, and we propose tentatively the following definition: (3) "The quantity of electricity on A is added to the quantity of electricity on B, when A is brought into contact with B under the given conditions." (It would serve no useful purpose to describe to the reader accurately what the conditions are.)

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[1] It may be noted in this case that (1) is not a purely verbal definition. If we are going to attain consistent results, it is clear that, if two bodies attract equally an object A, they must also attract equally another object B; for if this were not so, we should arrive at different conclusions as to the magnitude of the quantity, according as we used A or B as a test. But we can only know that it is indifferent whether we use A or B by trying the experiment. Accordingly, in this case, (1) implies a law; it would be more logical, but far more complicated for purposes of exposition, to alter the definitions so as to include this law under (3) and not under (1).

We must now find out whether this definition leads to consistent results. In order to do this we must perform the following experiments. We take three bodies, A, B, C, all uncharged, and a considerable number of bodies, each of which bears the quantity of electricity 1 according to (2). We bring $n$ of these bodies into contact with A, and $m$ of them into contact with B, under the given conditions; we then bring A and B successively into contact with C; C now bears, according to our definition, the quantity of electricity $n+m$. We note the force with which C attracts some other body D, and we then render C uncharged again. We now bring $n+m$ of the unit bodies into contact with C; C again bears a quantity of electricity $n+m$; we note again the force with which C attracts D at the same distance from it. If the force is the same, however often we try the experiment, taking different bodies A, B, C, and different numbers $n$ and $m$, then the definition leads to a consistent measurement, and our object is achieved.

This series of experiments has often been made—though, of course, the actual details are far more complicated than are described now. I do not know that it has ever been made with the immediate object of testing the consistency of the measurements, for men of science, in early days at least, did not recognise the logic of measurement; but it is tried every day incidentally and no inconsistency has ever been found. The quantity which we have called "quantity of electricity" is firmly established in science, and is the basis of most other electrical measurements, but many students would be hard put to it to explain clearly the arguments and observations which are necessary to its measurement.

**15. Positive and Negative Electricity.**—It will be noted that I have only spoken of one "quantity of electricity," whereas the theory required us to recognise two kinds of electricity—glass-electricity and silk-electricity. Do we not need two quantities to describe completely all electrical phenomena? It is a remarkable consequence of the definitions that we have adopted, and one of their chief advantages, that we do not.

We have noted that if a body charged with glass-electricity attracts another charged body, then a body charged with silk-electricity repels it. If the two are brought into contact so that the electricities mingle, their effects counteract each other and the attraction is less than it would be if either acted alone. It might seem likely at first, since either kind of charged body attracts a neutral body, that the mingled electricities would attract the object more strongly than either acting alone. But this presumption would be false; the attraction for an uncharged body is diminished in the same ratio as that for a charged object—a fact of which the theory given will be found to afford an explanation readily. Suppose, then, that A is charged with glass-electricity and B with silk-electricity, and that, according to the definition, the quantities of electricity on A and B are both added to C; then it will sometimes happen that C is found to have no charge at all. But if, after adding A or B, we had not gone on to add B or A, but had subtracted A or B, the result would have been precisely the same; C would have been found uncharged. We see that the effect of adding one kind of electricity is precisely the same as subtracting the other kind. But in arithmetic, when the effect of adding one number is the same as subtracting another, the second number is called the negative of the first; the effect of adding $+2$ is the same as that of subtracting $-2$. Accordingly, in the case which has just been described, if we call the quantity of electricity on A or B $+a$, and that on B or A $-a$, no further definitions need be introduced in order to measure quantities of both kinds of electricity. We shall have again to inquire whether we get consistent results by this method, which will involve experiments of the same nature as those described before; as a matter of fact, the measurements are found to be consistent.

But, it may yet be inquired, shall we not have to make definition (2) more precise; shall we not get different estimates of the quantity of electricity on a body according as the unit body, chosen to define the quantity of electricity 1, is charged with glass-electricity or silk-electricity. Well, it is found by experiment that the only effect of changing the charge on the unit body from glass-electricity to silk-electricity is to change the *sign* of the quantity of electricity found by means of the definitions on any body from + to − or *vice versa;* the *number* representing the quantity will be unchanged; a quantity +2 will become a quantity −2, and not −3 or −1. This discovery is an experimental law, and a most convenient one. Stated in other terms it is that, if a body A, charged with glass-electricity, attracts or repels another body C with the same force as a body B, charged with silk-electricity, repels or attracts C, then if the quantities of electricity on A and B are added in the manner described to D, the charge on D will be found unaltered.

Since, then, a quantity of glass-electricity is precisely equivalent to a quantity of silk-electricity with the opposite sign, quantities of the one are represented by positive, and quantities of the other by negative, numbers. It is quite immaterial which is associated with either sign; by a convention, which was dictated originally by mere chance, glass-electricity is associated with positive, and silk-electricity with negative, numbers, and for the names "glass-electricity" and "silk-electricity" the names "positive" and "negative" electricity are now universally substituted.

16. **The Application of Measurement.**—When we have succeeded in defining measurable quantities in any department of science, a new field of investigation is opened up; we can try to find relations which always hold between the measurable quantities. The statements of these relations when they are found are laws; they are, like the laws from which we started, descriptions of observations. But it is important to notice that they are very special kinds of descriptions; the

actual observations which they describe might be made over and over again before it could possibly occur to any one who had not certain preconceived notions to describe them in that particular way. To represent laws as attainable simply by experiment and observations is quite misleading. It is true that there is no logical impossibility in discovering any law by mere observation, but as a matter of fact the form given to the law—and it is the form which is important—is determined in the case of the most important laws by ideas which are not derived from observation, usually by ideas which are suggested by some theory. Though laws are logically prior to theories, the most important laws which have been discovered in physics are historically subsequent to theories; these important laws are almost all laws stating relations between measurable quantities, and I think it is safe to assert that no new measurable quantity has ever been introduced into physics except as the result of the suggestions of some theory. The view, which has been put forward by some very eminent men of science, that their study would really get on better without theories, is only tenable if the term "theory" is used in a sense when it is utterly different from that employed here.

The most important laws in the branch of electricity which we are now considering, stating relations between measurable quantities, are :—

(1) The force of attraction exerted by any charged body on another charged body placed at a fixed distance from it is proportional to the quantity of electricity borne by the charged body.[1]

(2) In any isolated system of bodies the sum of the

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[1] In the modern mathematical theory of electricity (1) does not appear as a law, but as a definition of "quantity of electricity"; the first two definitions of p. 37 are retained, while for the third is substituted the statement that, when we say that the quantity of electricity on one body is twice that of another, we mean that it exerts twice the force on a body with regard to which both charged bodies are similarly situated. The definition (3) then becomes a

quantities of electricity on each of the bodies remains constant whatever changes the bodies may undergo. As a matter of fact, I think this apparent law is in strict logic only a definition of what we mean by "isolated," but it does describe very important facts which it would be cumbrous and difficult to express in a form more logically unobjectionable.

With the help of these laws we can define new measurable quantities without any further investigation; for, if we find that certain quantities are always related in the same way to other quantities, we can give a name to the whole combination. Thus, when we have defined the quantities, "weight of a given portion of water" and "volume of a given portion of water," and have found that these quantities are proportional to each other, we can without further investigation define the "density of water" to mean the ratio of the weight to the volume. From a logical point of view, such definitions are purely verbal, but they are important in drawing attention to the fact that certain combinations of quantities have a special interest.

Of the electrical quantities which are thus defined only one needs special mention, "electric potential."

true law in the form that contact under the given conditions does result in one body receiving the whole of the charge of the other.

I have preferred to adopt the definitions of the text for two reasons. Firstly, they seem better in accordance with the historical development of the subject; and, secondly, they offer an excellent opportunity of explaining the logic of measurement. The modern definition does not depend on that logic at all, so far as electrical science is concerned; it takes the notion of force as a measurable quantity from dynamics, which has proved experimentally that force is a consistently measurable quantity. And, since there are special difficulties, unconnected with the logic of measurement in general, which arise in defining the measurement of force, it has seemed better to avoid these by taking a system of measurement which does not employ that notion. It will be seen that in developing our measurements, we have never spoken of one force being twice another, only of one force being equal to another; we have only employed the first two definitions of the three required to define force; the difficulties are, as usual, connected with the third.

If two bodies charged with electricity of the same kind are connected by a wire, one of them will generally lose and the other gain electricity; but nothing which has been said so far enables us to determine which will gain and which will lose; the one which gains is not necessarily that which contains the smaller quantity of electricity. The electric potential is a quantity such that when two charged bodies are connected by a wire the body with the higher electric potential loses electricity; it turns out to be determined both by the charge carried by the body, its shape and its position with regard to other bodies. This quantity might have been made the primary measurable quantity of the science of electricity in place of that which we have called "quantity of electricity," for the method of its definition is also suggested by the theory. If two vessels containing water are connected by a tube, water will flow from that in which the water is at the higher pressure; accordingly, electric potential bears an analogy to the pressure of a fluid which we shall find useful to remember.

**17. The Science of Electrostatics.**—For our present purpose there is no need to pursue further the science of "electrostatics," as that branch of electricity which deals with the laws and theories which we have been considering is termed. Yet we have only arrived at the point from which a treatise intended for professed students of the science would start. Such a treatise consists of two parts. The first deals with the logical consequences of the laws we have enunciated; the general problem considered consists in determining from a knowledge of the values of some of the electrical quantities for a certain system of bodies of known shape and position the values of the other electrical quantities. The problem may give rise to discussions of considerable mathematical interest, and is important for the second part of the treatise, which consists in a description of the various experimental devices, based on the results of that discussion, which may be employed for the measurement of the quantities. But neither of these developments is likely to interest the general reader, and so we pass to a completely new set of phenomena, which lead to the formulation of new laws and the development of new theories.

# CHAPTER III

## ELECTRO-MAGNETISM

**18. The Fundamental Laws.**—If a plate of pure zinc and a plate of pure copper are placed, without touching each other, in a vessel containing dilute sulphuric acid, no change takes place. But if the plates touch, or if they are joined by a metal wire, chemical changes take place; the zinc is gradually dissolved in the acid, while gaseous hydrogen, derived from the acid, is evolved at the surface of the copper. At the same time changes are noted in the wire which joins the plates; the wire is heated, and if a magnetic needle, suspended as in an ordinary compass, is brought near it, the needle which formerly pointed north and south now shows at the same time a tendency to set itself at right angles to the direction of the wire. The chemical changes go on and the deflection of the needle continues, so long as the wire is unbroken, until all the zinc is dissolved; but they cease immediately the wire is broken.

Every one who has ever dabbled in science knows these facts, and knows further that the phenomena are regarded as "electrical"; but perhaps he would be troubled if he were asked to explain what he meant by saying that the phenomena were electrical, or to produce evidence for the truth of his assertion. Let us inquire more precisely what the statement means, and what kind of evidence is needed to establish it.

**19. "Frictional" and "Voltaic" Electricity.**—The assertion clearly means that the phenomena have something to do with those described and analysed in the previous chapter, but they are not merely particular cases of them. The new laws, which have just been stated, are not deducible in any way from the former laws, for they contain ideas, such as that of a magnet, of which no previous mention has been made. We saw, moreover, that so long as we confined ourselves to stating laws, there was no need to introduce such terms as "electricity" and "electrical"; the need arose only when we developed a theory to explain the laws. The assertion that the new phenomena are electrical must have some reference to the theory of the last chapter, and a little consideration will show that it means that the new laws can be explained by that theory as it stands, or by some simple development of it, so that both sets of laws can be embraced by one theory of electricity.

But the assertion would not have been made unless there appeared to be some common element in the laws themselves, that is, unless there were some important action described by both sets of laws. The action which, as a matter of historical fact, first gave rise to the idea of a connection between the laws is one with which we are not concerned, but if only the facts described were known, a connection might be suggested by the distinction which they both make between metallic and non-metallic solid bodies; if the wire joining the plates were replaced by one composed of one of the substances which our study of electrostatics led us to call non-conductors, the non-conducting wire would show none of the peculiar properties mentioned. The division of substances into two classes by means of the two sets of phenomena is the same in its general features.

This fact gives us our first suggestion that the theory of electricity may possibly throw some light on the new phenomena. That theory drew an analogy between the difference of the two classes of substances, and the difference of hollow and solid bodies in respect of the passage of fluid through them. Now, the properties of a hollow tube differ somewhat according as a current of fluid is or is not passing through it, and the suggestion is obvious that the difference between the properties of the bodies which are likened to tubes, according as they are or are not joining the plates of zinc and copper, is to be explained by the passage through them of a current of the "fluid" electricity in the former and not in the latter state. This suggestion was made almost immediately after the discovery of the peculiar properties of the wire joining the plates of a "voltaic cell" (as the combination of zinc, copper, and dilute acid is called), and for many years the question of the sufficiency of the explanation was debated in the form, "Are Voltaic and Frictional Electricity the same?"

The analysis of the theory which was undertaken before shows that the form of the question was undesirable; it suggests much too forcibly that "voltaic electricity" and "frictional electricity" are substances in the sense that water is a substance. And, in fact, the form in which the question was asked did lead to much confusion, and obscured the nature of the evidence which was required to answer it. We see now that, though nobody doubted that the answer was in the affirmative after about 1830, the experiments which were really necessary to give the answer were not made till some fifty years after that date, and that, when they were made, they were designed to give an answer to a perfectly different question, which, if men of science had been logical, they would never have asked when they imagined the previous question to have been answered. The history of electrical theory in the early part of the nineteenth century provides an admirable illustration of the danger of using forms of words without a due examination of their meaning—but it is easy to be wise after the event.

**20. Proof of the Identity.**—If voltaic electricity is the same thing as frictional electricity, when we know the state of a system completely in respect of frictional electricity we must also know it completely in respect of voltaic electricity. Now, to know the state of a system completely in respect of frictional electricity, means to know for each part of the system the values of the electrical quantities which were described in the previous chapter, the quantity of electricity contained in each part at every time and the positions of the parts with respect to each other. On the other hand, since by "voltaic electricity" we mean the agent concerned in the production of the properties of the wire when it joins the plates, to know completely the state of the system with respect to voltaic electricity means to know completely all the quantities determined by the properties of the wire, the amount of heat developed in the wire, the magnitude and direction of the forces acting on the magnetic needle in a given position with regard to the wire, the amount of zinc dissolved, and the amount of hydrogen generated. If, then, voltaic and frictional electricity are the same thing, one set of quantities must be completely determined by the other; a knowledge of one set of quantities, and of that set only, must be sufficient to determine the other, just as the knowledge of the volume, temperature, and density of a gas is sufficient to determine its pressure.

The experiments which are necessary to answer the question consist then in determinations, for a great many different voltaic cells, of the electrostatic quantities for each part of the cell on the one hand, and such quantities as the magnitude and direction of the forces on the magnetic needle on the other; when the determinations have been made a scrutiny must be conducted to discover whether the two sets of quantities are constantly related. Such a scrutiny would not be likely to lead to a positive result, unless it were conducted with some preconceived ideas as to the nature of the relations likely to be found, for the relations might be excessively complicated and quite undiscoverable by simple trial. But in this task we are again helped by the theory and the analogies it offers. If we were investigating the relation between the properties of a hollow tube which were due to the passage of a current of water through it and the state of the flowing water we should find that the most important of the quantities, defined by those properties were simply connected with such quantities as the rate of the flow of the water, or the pressure under which it was flowing. It is natural, therefore, to seek in the first place relations between the quantities, such as the amount of heat developed in the wire, defined by the properties of the wire, and such quantities as the rate of flow of electricity through the wire—that is, the quantity of electricity which is subtracted from one plate and given to the other in a given time. This rate of flow will depend clearly upon the electric potentials of the two plates which the wire joins, and measurements of this quantity are likely to be of the greatest importance in the investigation.[1]

The observations are extremely difficult to carry out, because the values of the electrostatic quantities in

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[1] Nothing has been said hitherto as to the electric potentials of the plates, but it is obvious that, if the view is correct that the peculiar properties of the wire are due to the "passage of a current of electricity through it," there must be a difference in the potentials, for electricity will pass from one body to another only when the two have different electric potentials. The discovery that there was a difference of electric potential between the two plates was regarded in the early days of electricity as a conclusive proof of the identity of "voltaic" and "frictional" electricity, but the proof is not conclusive until it is shown that the voltaic quantities are completely determined by the electrostatic quantities. It is not the mere fact that there is a difference of electric potential, but the fact that, when the difference of electric potential and all the other electrostatic quantities are the same, the heat developed, the forces on the needle, and so on, are also the same which justified an affirmative answer.

such systems as show most markedly the properties of the voltaic cell are either very much larger or very much smaller than the values of the same quantities for a system which shows most markedly the electrostatic phenomena described in the last chapter, so that it is hard to measure them in both cases by the same method, just as it is hard to measure accurately by the same method the diameter of a drop of water and the diameter of the earth. But the difficulties have been overcome, and the results are such as to establish completely the "identity of voltaic and frictional electricity." It is found that the amount of heat developed in the wire is determined completely by the product of two electrostatic quantities, the quantity of electricity which has passed through the wire from one plate to the other, and the difference of electric potential between the ends of the wire; the magnitude of the forces on the needle is determined wholly by the rate at which electricity is passing through the wire; the quantity of zinc dissolved or the quantity of hydrogen generated is determined wholly by the total quantity of electricity which has passed through the wire. When these discoveries had been made, but not before, it was justifiable to use the properties of the voltaic cell to measure electrostatic quantities. Nowadays it is permissible to determine, for instance, the rate at which electricity is passing through the wire (or the "current through the wire," as it is termed) by observations on the forces on the magnetic needle; and in practice to-day such methods are employed universally, but they were not justifiable until the observations mentioned had been carried out.

**21. Consequences of the Identification.**—Two special conclusions which are implied by the theory of the voltaic cell which identifies frictional and voltaic electricity require separate notice. Since the total quantity of electricity in an isolated system, such as that of the plates in the acid, is constant, the electricity flowing along the wire from one plate cannot disappear when it reaches the other; it must either remain in the other plate or return to the starting-point by some other path. Experiment shows that the latter alternative is correct; the electricity returns through the liquid in which the plates are immersed, and that liquid, on closer examination, shows all the peculiar properties of the wire.

Again, it has been said that the forces on the needle are determined solely by the rate at which electricity flows through the wire from one plate to the other, and by the shape of the wire and the position of the needle. But it is possible to transfer electricity from one plate to the other without connecting the plates by a wire; if a series of bodies be brought into contact with one plate and then carried across into contact with the other, they will also effect a transfer of electricity. If these bodies are small and the path in which they travel between the plates lies everywhere in the position previously occupied by the wire, and if the quantity of electricity which the bodies receive from one plate and give to the other in a certain time is the same as that which flows through the wire in the same time, then the moving bodies are exactly similar to the wire in respect of the flow of electricity along a given path. If our theory of the identity of frictional and voltaic electricity is correct, the moving bodies must exert on the magnetic needle precisely the same forces as the wire which they replace. If they did not, the forces on the needle would be determined by quantities other than electrostatic quantities, and voltaic and frictional electricity would not "be the same thing." As a matter of fact, delicate experiments have shown that the two methods of transferring electricity are equivalent, a conclusion of the greatest importance for modern developments of electrical theory.

One question which has been left untouched may have troubled the reader; the identity of frictional and voltaic electricity has been discussed without any reference to the fact that our theory of frictional electricity led us to recognise two kinds of "fluid." With which is voltaic electricity identical? The phenomena which have been considered hitherto can give no answer to this question. It is meaningless at present to inquire whether, if a positively charged body A is joined by a wire to a negatively charged body B, the flow of electricity consists of the passage of positive electricity from A to B, or of the passage of negative electricity from B to A, or of both. For the expression "the flow of electricity" only has a meaning if the theory of electricity is accepted, and that theory, as developed so far, asserts definitely that the two kinds of stream are indistinguishable, that there is no phenomenon which can be attributed to a loss of positive electricity rather than to a gain of negative. It is true that the theory suggests that such phenomena may possibly be discovered, and that suggestion has turned out to be true; but the consideration of such phenomena on which the modern theory of electricity is based lies beyond our scope.

**22. Ampère's Theory.**—The laws given on p. 37 do not state all that is known about the effects due to the passage of a stream of electricity. Each of them has been subjected to much further study, the results of which do not now concern us; only one of them shall we consider in any detail. The heating of the wire is of immense technical importance, but the study of it did not lead to results of any theoretical interest until the development of quite modern ideas. The study of the chemical changes, the solution of the zinc and the evolution of the hydrogen, led to the formulation of a new science which has influenced chemistry as much as physics, but such matters, again, are beyond our scope. However, we cannot proceed further without more investigation of the effect of the current upon a magnetic needle.

Such a needle is, of course, deflected, not only by a current passing through a wire, but also by another magnet brought into its neighbourhood. An investigation of the relation between the deflection of the needle and the shape of wire circuit, together with the magnitude of the current flowing in it, enabled Ampère to set forth rules whereby the form of the magnet to which a given circuit is equivalent in respect of its action on the needle might be determined; he found that the form of the magnet was determined wholly by the shape of the circuit and the magnitude of the current flowing in it. Now, if two circuits are equivalent to two magnets in respect of their action on a magnetic needle, analogy suggests that they should be equivalent to the same two magnets in respect of their action on each other, and that they should, like two magnets, exert forces on each other. And it is found, in fact, that in respect of all actions any current circuit can be completely replaced by an appropriate magnet, or a magnet by an appropriate current circuit. A great part of the textbooks designed for students of physics consists in a discussion of Ampêre's rules, and in an application of them to determine in various cases of technical or theoretical importance the forces exerted between magnets and current circuits, or between two current circuits.

23. **The Doctrine of Energy.**—Now let us look at the matter from a rather different point of view. When the ends of the wire are joined to the plates of the cell and a current started in it, a magnet in the neighbourhood will tend to move. The question arises whether there is any "converse" action, consisting of a current started in the wire when a magnet in the neighbourhood is moved: and to understand what is meant by a "converse" action, and why there should be any expectation of its occurrence, we must notice very briefly and imperfectly some features of the doctrine of energy.

When the changes in any set of bodies is investigated, it is usually found that a change in any one body is accompanied by a change in some other body. In the case which we have been considering, the change in the position of the magnet does not take place unless there is a change in the zinc of the cell, nor is there a change in the zinc unless there is a change in the position of the magnet. Now, each of such related changes can be used, after due investigation, to define various measurable magnitudes, and these magnitudes will be such that the value of one cannot change unless the value of the other changes. It will be easy to frame the definitions so that, as the magnitude of the value defined by one of the related changes increases, the value of that defined by the other decreases. Further—and this is the important point—we can usually find among such related magnitudes one pair such that the increase in the magnitude defined by one change is equal to the decrease of the magnitude defined by the other change. Such quantities, if they can be found, are all termed "energy"; and from the way in which "energy" is defined it follows at once that the sum of the values of the energy for all the bodies of a system is always the same, for an increase in the energy of one body is necessarily accompanied by an equal decrease in the energy of another. To say, as is often said, that "energy is conserved" is to state a truism; the important statement is that there are such quantities as "energy."

But if there are such quantities as energy, it is immediately clear that, when one of the related changes is reversed in direction, the other must also be reversed; for if a change in one direction implies an increase in the quantity defined by it, a change in the opposite direction implies a decrease. And since it is one of the primary articles of the faith of a physicist that he will always and for all changes be able to define a quantity with the properties of energy—we shall not inquire here into the foundations of his faith—he must necessarily expect to find in each case a "converse" change. If a certain change in one body from A to A′ produces a change in another from B to B′, then he expects to find that if, under suitable conditions, he restores the second body from the state B′ to B, he will at the same time restore the first body from the state A′ to A.[1]

**24. The Induction of Currents.**—The suitable conditions are easy to find in the case under consideration, and the results are such as were anticipated from considerations of energy. Let us take a loop of wire and place near it at one point a magnetic needle freely suspended with its axis parallel to the wire at that point. Let us now cut the wire and join the ends to the two plates of the voltaic cell; a current flows through the wire and the needle sets itself with its axis perpendicular to the wire; let us then disconnect the ends of the wire from the plates and join them again to each other. Two changes have taken place: a certain current has passed through the wire and the needle has changed its position. The doctrine of energy leads us to suppose that if, under suitable conditions, we restore the needle to its original position, we shall make a current pass through the wire in the opposite direction to that in which it flowed before. The conditions are easily attained; we have only to turn the needle back by hand

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[1] The proviso "under suitable conditions" is of the utmost importance, for it may easily happen—indeed it usually does happen—that the reversal of the state of the second body from B′ to B turns out to be accompanied by a change in the state of some third body, which underwent no change when the changes from A to A′ and from B to B′ were originally effected. In such a case the doctrine of energy does not lead to the expectation that the reverse change B′ to B will be accompanied by the reverse change A′ to A, for there is the change in the energy associated with the third body to be taken into consideration. But if conditions can be found such that the change from B′ to B is accompanied by a change in the first body and by a change in no other body, then that change should be from A′ to A.

and we find that a current does indeed flow in the reverse direction through the wire. Such general conditions could not easily lead us to predict what would be the exact nature of the current flowing through the wire when the needle is moved, but a more precise investigation of the action of the current on the needle enables us to make the prediction with success. It is found that a given change in the position of the needle causes a given quantity of electricity to pass round the wire circuit; the magnitude of the current and the time for which it flows will depend upon the nature of the wire and other things, but so long as the positions between which the needle is moved are the same and the position of the wire is the same, the same quantity of electricity will flow round the circuit. If the magnet is moved simply from one position to another the current in the wire will soon stop, but a continuous current may be maintained by maintaining suitably the motion of the magnet.

This is the "converse" change which we expected, and it is of immense industrial importance, for dynamos consist simply of wire circuits near which magnets are moved so as to maintain a continuous current. But other aspects of this action interest us more nearly.

The question might be asked, whether the action depends upon the actual motion of the needle, or whether it is due to the fact that the motion of the needle alters the force which it exerts on the magnet represented by the current flowing in the wire? The question can be answered by replacing the magnetic needle by the current circuit to which it is equivalent; we can then produce a change of the force due to this magnet on the loop of wire either by moving the circuit or by changing the strength of the current flowing round it. We find, as might be expected, that either process causes a temporary current to flow round the loop of wire, and further investigation enables us to conclude that the quantity of electricity which is carried round the loop by the temporary current is determined only by the change in the magnetic forces exerted by the magnet on the loop, and that it is quite immaterial how that change is produced.

**25. Electric Inertia.**—We arrive, then, at the notable result that a current can be made to flow in one circuit by changing the current flowing in a neighbouring circuit, an action which is technically termed the "induction of currents." And now a further question immediately suggests itself; if a change in the current flowing in one circuit produces a change in the current in a neighbouring circuit, may it not also produce a change in the current in the original circuit. Such a suggestion, that a change in the current in a circuit should produce a current in the same circuit, seems difficult and almost meaningless, until we remember that the effect of a given change in the current may be expected to produce only a temporary current, consisting in the passage of a given quantity of electricity round the circuit; the phenomenon which is suggested is something affecting only the current in the circuit for a short time after the change is made. Further careful attention shows that, if a change in the current in any circuit induces in that circuit changes of the same nature as those which it induces in others, we shall find that any change in the condition of the circuit tending to produce a change in the current through it (as, for example, the cutting of the wire, which must lead ultimately to the cessation of the current) will not produce that change in the current immediately, but that there will be an interval, corresponding to that required for the passage of the induced quantity of electricity round the circuit, during which the current gradually changes from that appropriate to the old to that appropriate to the new condition. This effect is observed. If we suddenly disconnect the wire from the plates and join the ends, we shall find after a certain period that there is no longer any current flowing through the wire. But if we make the examination during a very short interval following the disconnecting, we shall find that the current does not stop suddenly, but that during an interval, the magnitude of which depends upon the shape and material of the wire and so on, the current decreases gradually from that flowing originally to nothing at all.

It may be noted that the existence of such phenomena is suggested by our theory of electricity without any special reference to magnetic phenomena or the doctrine of energy; they are suggested merely by the fact that we have used the analogy of a fluid to explain some laws, and the suggestion would have been the same whatever the nature of the laws so explained. For we have likened the actions which take place when a wire joins the plates of a voltaic cell to those of a stream of water driven round a closed channel by means of a pump (corresponding to the cell) placed at one point of it. Now, if the pump is stopped, the flow of the water will not stop suddenly; it will decrease gradually, until it dies away.

We describe this property of the water by saying that water possesses inertia, and we use it to define a most important quantity called the "mass" of the water. Analogy suggests that the flow of the electricity should also die away slowly and not decrease suddenly when the cell is removed, and we have seen that the suggestion turns out to be correct. We may express this result, remembering, however, all that has been said of the caution necessary in interpreting statements made about the ideas of a theory, by saying that we have found that electricity possesses inertia, and we may proceed to try to define a quantity called the "mass of electricity."

Unfortunately, our efforts in that direction will not be completely successful; we shall not be able to define such a quantity which, as in the case of water, depends only on the quantity of electricity flowing round the circuit and is independent of everything else; we can define a magnitude which resembles in some important respects the mass of the water, but it differs from it in depending on the shape of the circuit and on its relation to other circuits. Still the conception of inertia possessed by the electric current is of the greatest importance, and we shall find it of great help in further developments of electrical theory.

**26. Conclusion.**—With this conclusion our survey of the "fluid theory of electricity" may end. We have found that the laws which have been enunciated may be explained, at least in their general features, by the action of two fluids, to which two distinct sets of properties must be attributed. The first set is necessary to explain the laws of electrostatics, the second to explain the laws of "electro-magnetism," as the branch of the subject which we have just left is called. We have seen that, as we considered each fresh law, the suggestions of the analogy on which the theory is based have usually enabled us to imagine a new property to be attributed to the fluid in order that the law might be explained; but we have noted at the same time that all the suggestions of the analogy are not correct, and we have received a warning of the danger of following suggestions blindly without putting them to the test of experiment.

The theory is now complete; no further properties can be attributed usefully to the fluids in order to enable the theory to explain further laws, except such properties as are inconsistent with some which have been attributed already. The modern developments of electrical theory which retains some of the features of the fluid theory while rejecting others will not concern us in this volume, and we may now pass to the consideration of a theory totally different in its nature to the fluid theory—one that was originally proposed to replace the fluid theory, but has since been combined with it to form the theory which has led to such notable advances in the last few years. This theory is inseparably connected with the name of Faraday.

## CHAPTER IV

### FARADAY'S THEORY

**27. "Action at a Distance" and "Through a Medium."**—Faraday's theory is sometimes contrasted with the fluid theory by the statement that, while the latter is a theory of "action at a distance," the former is a theory of "action through a medium." The use of these two phrases has led to endless discussions; philosophers, amateur and professional, have attempted to maintain by purely transcendental arguments that one type of theory is preferable to the other. But our view of the meaning of theories leads us to reject as irrelevant to science any such arguments; if one type of theory is preferable to another it must be on the ground that it explains laws which are not explicable by the other; if one type can be rejected in favour of another it must be on experimental grounds. But what is the nature of experiments which can decide between the two types of theory in any particular case? Let us take an example.

It has always been admitted that gravitational action is such as to suggest most characteristically "action at a distance," while the action of a locomotive on a railway coach suggests "action through a medium." A little consideration will show that the distinction lies in the following facts. The gravitational action of a body A on a body B depends only on the properties of those two bodies and their relative position; it is independent of the properties of any third body, and, in particular, of bodies lying between A and B.

On the other hand, the action of the locomotive on the coach depends not only on the properties of the engine and the coach, but also on the properties of the bodies which lie between them; if we remove the couplings there is no longer any action. A theory of "action at a distance" suggests that the action between two bodies is independent of the properties of any third body, while a theory of "action through a medium" suggests a dependence on the properties of bodies lying between the acting bodies.

**28. Faraday's Discovery.**—Before the fluid theory of electricity was put forward definitely, the theory of gravitation was well developed. When it was found that the relation of the forces between two charged bodies to the distance between them was very similar to that of the gravitational forces between two bodies to the distance between them, there was a natural tendency to assimilate in every way electrical and gravitational action. In particular, it was assumed without trial that the electrical action between two charged bodies was independent of the nature of the medium separating them, so long, of course, as that medium was non-conducting and did not allow the charges to mingle and neutralise each other. Faraday appears to have been the first to call this assumption in question and deliberately to try the experiment. He took two charged bodies and investigated the forces between them, first, when they were separated by air, and, second, when the space separating them was filled with sulphur or other non-conductors. He found that the action was changed by the presence of the sulphur, and concluded accordingly that electrical action could not be action at a distance.

In so far as he concluded that his discovery was irreconcilable with the fluid theory of electricity, he was wrong. Crucial experiments, which decide once and for all between conflicting theories are, like "canons of induction" and many other things, myths which exist only in the imagination of philosophers completely ignorant of science. Almost any experimental result *can* be reconciled with almost any theory, if sufficient subsidiary assumptions are made; the only question is whether it is worth while making them. In the case before us, the additional assumptions necessary to reconcile Faraday's result with the fluid theory are so simple and obvious that few people would feel any objection to them. We have seen that our theory proposed to explain the attraction of an uncharged by a charged body by supposing that, under the influence of the charge, the opposite electricities in the uncharged body are separated into different parts of that body. If, then, a plate of sulphur is placed between two charged bodies, A and B, the distribution of the electricities in the sulphur will be changed; before the sulphur was there the only force acting on B was that due to A; when the sulphur is there, to the force on B due to A must be added the force on B due to the electricities in the sulphur, which, being separated, no longer counteract each other. The difference between sulphur and air can be attributed to a difference in the quantities of opposite electricities contained in those two substances, or to a difference in the ease with which they are separated by the external charge. As a matter of fact, this explanation can be shown to be perfectly adequate; by making suitable assumptions about the electricities in sulphur and air all the effects obtained by substituting one for the other can be deduced from the fluid theory of electricity, which takes the forces between two charges to be dependent only on their magnitude and the distance between them.

**29. Faraday's Theory.**—It is in considering such cases as this that it is important to remember the artistic as well as the logical aspect of theories. If Faraday, the greatest physical genius of all time, had wanted to support the fluid theory of electricity, he would have been the first to see the possibility of the explanation that has been indicated. But he did not want to. That theory never satisfied his artistic sense, and the facts that he had discovered justified him in seeking another, and indicated the place where he might seek. It suggested that he might seek an analogy for the difference between a charged and an uncharged body, not in the addition to the uncharged body of some extraneous substance, but in a change of the properties of the medium surrounding the body.

The analogy which he adopted was suggested by the forms taken by iron filings in the neighbourhood of a magnet. If a card is laid horizontally over a bar magnet and iron filings scattered over it, the particles arrange themselves in a series of curved lines which run from one pole of the magnet to the other; if the poles of a single magnet are replaced by two similar poles belonging to two different magnets (such poles, of course, repel each other) the filings still arrange themselves in curved lines, but the lines starting from one pole do not end on the other, nor do they cross any of the lines starting from the other. The laws of attraction and repulsion between the poles of magnets are the same as those between electric charges, and by a slight modification of the experiment, in which a mixture of red-lead and sulphur replaces the filings, similar lines may be sketched out in the region between two electric charges. The form of the lines is determined by the condition that a small charged body placed at any point on one line will continue to move along that line without leaving it, till it reaches the opposite charge or recedes to a great distance.

A glance at the figure given by two opposite attracting poles suggested to Faraday that the attraction might be explained by imagining the lines of filings replaced by stretched elastic strings, which by their tendency to contract dragged the poles together. In order to explain the curved form of the lines he had to imagine further that each of the strings, while it tended to contract in the direction of its length, tended at the same time to repel its neighbour; this additional property explained at the same time the repulsion of similar poles. This idea was the basis of Faraday's theory. He imagined that electrical attractions and repulsions were due to the presence of these strings, or "lines of force," as he termed them; a charged body is one on which the lines end, and bodies carrying opposite charges are opposite ends of the same lines. The difference between a charged body and an uncharged body does not, on this theory, consist in a difference of the bodies themselves, but in the presence or absence of these lines of force connecting the bodies with other bodies. The theory of "action at a distance" is replaced by that of "action through a medium."

**30. Properties of "Lines of Force."**—The theory may be developed along the same lines as the older theory; the first step is to define measurable quantities in a manner suggested by it. The previous theory suggests that "quantity of electricity" or "pressure of electricity" should be measurable; Faraday's theory suggests that "number of lines of force" and "tension along or repulsive pressure exerted by lines of force" should be measurable, and such magnitudes have been defined consistently. Of course, since the two sets of magnitudes are defined by reference to the same observations, there must be a connection between them. As a matter of fact, it is found that "number of lines of force" is proportional to "quantity of electricity," while there is rather a complex relation between "tension of the lines of force" and "electric potential." The theory suggests that the tension along any line of force should be independent of the presence of other lines, but dependent on its length; unfortunately the suggestion is false (as was the suggestion about the "mass of electricity"); for the tension along a line of force is independent of its length, but dependent on the number of other lines in the neighbourhood.

It will be noted that, if the new theory is adopted, it will be quite impossible to give any answer whatsoever to the question "What is electricity?" for the theory makes no use of the conception of electricity. The question suggests the answer that electricity is a substance which is added to bodies when they show the properties which are described by saying that the bodies are "electrified," whereas the present theory denies that assertion. It would have been no more rational to expect Faraday to answer the question than it would be to expect an adherent of the view that the earth is flat (even supposing her to be of an intelligence sufficient to answer any question) to tell us what is the radius of curvature of the earth.

Faraday's theory, then, proposes to explain the attractions of charged bodies by the tensions and pressures of the lines of force connecting them. The alteration of these forces with a change in the nature of the medium between the bodies is supposed to be due to a change of the properties of the lines with the substance through which they pass. The tension of the lines of force varies with the nature of the non-conductor through which they pass; in sulphur, for instance, the tension is less than in air. In a conductor the lines of force cannot exist at all; such a body is one in which the ends of the lines can move freely, whereas in a non-conductor the ends are fixed; accordingly, under the tension of the lines, the ends approach each other and the lines shrink and disappear.

**31. Faraday's Theory and Electro-magnetism.**—This process of the shrinking of the lines of force, and their ultimate collapse into the substance of a conductor, represents on our theory what on the older theory was called the passage of a current of electricity through that conductor, and accordingly it is with this process that the heating of the wire and the deflection of the magnetic needle must be associated. During the process of collapse the portion of the lines which are originally outside the conductor will move at right angles to themselves, as the pressure is relieved by the disappearance of the neighbouring lines until they reach the conductor.

A consideration of the magnitude of the magnetic effect under various conditions, in some of which the line collapses without moving at right angles to itself, while in others it does so move, shows that it is only when the lines move at right angles to themselves that a deflection is produced in the magnetic needle over which they are passing; if the ends of the line approach each other, so that there is no motion of the line as a whole except in the direction of its length, no magnetic effects are produced. This conclusion, it may be noted, could have been deduced from considerations of energy based on the fact mentioned before, that the tension of a line does not vary with a change in its length.

The magnetic effects, then, which were formerly attributed to the flow of a current of electricity must now be attributed to a motion of the lines of force at right angles to their length. The argument which led us before to imagine that electricity possesses inertia now forces us to imagine that the lines of force possess inertia when they are moving at right angles to themselves, but not when they are moving along their own length. The great importance of this conclusion will appear presently.

32. "**The Æther.**"—We must now notice a point of some difficulty concerning problems the discussion of which has led to endless confusion. It has been said that the properties of the lines of force must be considered as dependent on the nature of the substance through which they pass. Now, this fact suggests that the lines are made of that substance in some such way as waves are made of water; how otherwise should a change of substance alter their properties? But electrical attractions take place even if there is no substance between the attracting bodies—even if, that is to say, they are placed in the most complete vacuum that we can produce. What, in such a case, is the substance which the lines are made of? Every one, of course, has the answer ready. The lines, they will say, are made

of "the æther, the medium which fills all space and through which light is propagated." And such was the answer that would have been given by Faraday, and it is an answer of the utmost importance, for it seems to provide a new reason for thinking valuable the theories of light which introduce the conception of the æther. In passing, I would ask the reader to recall the remarks which were made about the assertion that "electricity is a substance," lest he is misled into asserting, without due thought and understanding, that the æther is a substance; "the æther," like electricity, is the conception of a theory, not of a law.

But let us examine the matter a little more nearly. If the lines of force passing through sulphur are made of sulphur, we should expect them to move when the sulphur is moved. Now, if the lines move while their ends remain fixed, the resultant force which they exert on the bodies to which their ends are attached will change in direction, if not in magnitude; if all the lines are dragged to one side, the force on both bodies will be displaced to that side. We should expect, then, that if two bodies were charged with a large plate of sulphur in between them and the sulphur was afterwards moved to one side, the direction of the forces on the bodies would change. But experiment shows that there is no change whatever (if the plate of sulphur is so big that there is no fear of the lines getting out of it), and we must conclude that the lines have not moved with the sulphur, but have remained where they were.

We might get over this difficulty by supposing that the lines, though made of sulphur, are not always made of the same piece of sulphur, just as waves made of water are not always made of the same piece of water; so that the sulphur can move without the lines moving with it. But this idea also proves impossible. For there is another way of testing whether the lines move; we have seen that lines moving at right angles to themselves produce a deflection in a magnet; if the lines do not move when the sulphur moves, no deflection of a magnet ought to be produced when the sulphur is moved. But delicate experiments have shown that magnetic forces are produced when the sulphur is moved, even if the charged bodies on which the ends of the lines rest are unmoved. While experiments on simple attractions seem to show that the lines do not move with the sulphur, experiments on magnetic forces show that they do.

To resolve the contradiction the theory must be slightly changed, and we must return a little way towards the fluid theory of electricity. That theory supposed that the charged bodies influenced the sulphur by separating the opposite electricities in it; and if the effect of the sulphur on the attraction had been known, it would have been attributed to that separation. But separation of the electricities means, on Faraday's theory, the development of lines of force joining them, and accordingly we may suppose that the lines of force joining the external charged bodies in passing through the sulphur cause the development in it of new lines. Since the tension along a line depends on the number of lines in the neighbourhood, the new lines will change the tension along the original lines, and so change the attraction between the external bodies. When the sulphur is moved the new lines move with it and give rise to magnetic forces, but since yet other lines are developed as they move away, the tension along the lines belonging to the external bodies is unchanged. In this way we can explain the fact that the lines in the sulphur seem to move with the sulphur, while the properties of the original lines are unchanged.

On this view there is no need to suppose that the lines passing through the sulphur are "made of sulphur"; there is no need to imagine that they are made of anything. The lines of force from the external charged bodies are just the same through whatever substance they pass, and the apparent difference between the lines in sulphur and the lines in air is not due to a change in the properties of the lines, but to a difference in the number of secondary lines in the neighbourhood. It is sensible to say that the lines of force are made of anything, only when we can imagine their being made of different things; lines of force are just lines of force, independent for their existence of all surrounding bodies, and there is no more to be said about them.

But if lines of force passing through sulphur are not made of sulphur, there is no need, when the lines pass through a vacuum, to imagine the vacuum filled with a substance of which the lines may be made; in other words, our electrical theory, so far from providing additional support for the conception of the æther filling all space, does not require such a conception at all. All it needs is the conception of lines of force; where there are no lines there is no need for the presence of anything at all. We do not require to imagine present everywhere a substance of which the lines of force may be made when charged bodies come into the neighbourhood, for the bodies bring their own lines with them, ready-made and unalterable.

All this argument may seem hypercritical and unnecessary, but it is really intensely important. For so long as we imagined that lines of force passing through sulphur were made of sulphur, and lines of force passing through æther were made of æther, the fact that moving the sulphur without moving the charged bodies produces magnetic forces suggested that moving the æther would also produce magnetic forces. It is not immediately clear what is meant by "moving the æther," but many physicists of twenty years ago spent much labour in devising experiments by which they thought the æther might be moved relatively to charged bodies; when they found that such experiments resulted in the production of no magnetic force whatever they were much astonished, and produced elaborate hypotheses to account for their failure. But we see now that the analogy from which they worked was absolutely false ;[1] moving the sulphur produces magnetic forces because the subsidiary lines developed in the sulphur were moved. There are no such lines in a vacuum, for there are no electricities to be separated and, in the light of the arguments given here, none of the experiments tried could have been expected to produce magnetic forces. The analogy was based wholly upon the idea that an æther existing everywhere is necessary to Faraday's theory ; it is not necessary ; all that is necessary are the lines of force which are not made of the medium through which they pass.

33. **Electricity and Optics.**—But what of light ? The conception of the æther was first required for optical phenomena ; if we retain it for these but abandon it for electrical phenomena, it would seem that the hopes which naturally arose when we thought that the æther was necessary both to electrical and optical phenomena, that a connection might be established between the two branches of science must remain permanently unfulfilled. Can we abandon the æther for optical phenomena and explain them also on the ideas of lines of force ? We can ; it is possible to explain optical phenomena in terms of the properties attributed to lines of force, just in the same way as we explained the phenomena of electric currents in terms of the properties of the fluid electricity. The theory which achieves this explanation is the famous "electro-magnetic theory of light," developed by Maxwell on the basis of Faraday's ideas. So far we have not seen that Faraday's theory has any great advantage over the older theory ; indeed, we have to modify the former so as to bring it more into accord with the latter ; we have had, after all, to re-introduce the notions which we rejected on p. 55. But now at

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[1] Of course I do not mean to imply that the use of a false analogy showed in any way a weakness of those who employed it. As I have said before, it is so easy in science to be wise after the event, and nobody succeeded in seeing the arguments just given until the results to which they should lead were known.

length it is to justify itself by true suggestions which could never have been attained by its rival.

Let us consider for a moment how there can be an electrical theory of light. It might seem at first sight impossible to establish any connection between the phenomena of optics and those of electricity, between the fact that we see colours and shapes, and the fact that two rubbed pieces of glass repel each other, because the ideas contained in the two statements are so utterly different. But it must be remembered that we do not propose to establish a connection between the *laws* of the two sets of phenomena, but between the theories by which those laws are explained. Not the least part of the value of theories arises from the opportunities which they offer of connecting laws wholly diverse; though the two laws may contain no element in common, the theories by which they are explained may contain common elements, through which the desired connection may be made. In order to achieve this task our first duty is to examine the theories of light and of electricity in the hope of finding sufficient common conceptions to render the problem soluble.

Theories of light, like all physical theories, are founded on analogies. The earliest theory of importance, that of Newton, was founded, like the earliest theory of electricity, on the analogy of the transfer of a substance; the change in our eyes which we call seeing light was supposed to be due to the transference of some substance to our eyes from the luminous body. That theory has proved insufficient; it suggested many laws which proved to be false, and few which proved to be true; it has been wholly replaced by the theory of Huyghens, Young, and Fresnel, based on the analogy of waves.

Waves are changes which are periodic both in time and space—that is to say, changes such that, if we keep our attention fixed on a certain place, a regular series of changes will repeat themselves at that place at regular intervals of time, and such that, if we regard all places at a given moment, a regular series of changes will be seen to repeat themselves at regular intervals of space. The most familiar form of waves are those of the sea, or those which are set up when a stone is thrown into still water. Any given system of simple waves is characterised by three measurable quantities :—(1) intensity, which is represented in water waves by the height from crest to trough ; (2) frequency measured by the number of crests which pass a fixed point in a given time ; (3) velocity, the distance that a given crest travels in unit time.

Now, the undulatory theory of light suggests that light consists of such waves ; the intensity of the waves is supposed to determine the brightness of the light, the frequency the colour of the light, and the velocity the path in which the light travels. If we express the theory as before in the form of an assertion and a dictionary, we get such statements as these :—

*Assertion.*—Light consists of wave disturbances travelling through transparent media ; the frequency of a given disturbance remains unaltered throughout its course ; the velocity of the wave depends on the medium and the frequency ; in a medium A a disturbance of frequency *b* travels with a velocity *c* (where A, *b*, *c* are given definite meanings), and so on, until all velocities for all wave-lengths in all media have been enumerated.

*Dictionary.*—When we say that a light disturbance reaches the space occupied by the eye, we mean that we see light. When we say that the disturbance has a frequency *b*, we mean that we see a colour B, and so on.

From this assertion and dictionary laws can be deduced and compared with experiment ; the agreement has been found almost uniformly satisfactory. The theory of light is probably more complete than any other theory of physics.

The conception, then, which is fundamental in the theory of light is that of waves ; and if we are to have an electrical theory of light, we must show that this conception is applicable to the theory of electricity. At first sight it might seem obviously applicable to the fluid theory, for we are here provided with a material somewhat similar to water through which the waves may travel. But unfortunately this theory gives us no reason for supposing that there is any electricity where there are no charged or chargeable bodies; there is no electricity in a vacuum to carry the waves, and yet the waves travel through a vacuum. It is just because a vacuum is regarded by the older theory of electricity as an absolute blank that it can never be harmonised with a theory of light.

**34. The Electro-magnetic Theory of Light.**—On the other hand, Faraday's theory does not regard a vacuum as an absolute blank; it regards it as permeated by lines of force. But can these lines of force transmit waves? Certainly they can, although such waves may seem to be rather different from water waves. Lines of force we have imagined to be like stretched strings under a certain tension along their length and possessing a certain inertia when moved at right angles to their length; in so far as motion at right angles to their length is concerned, they are exactly like ropes stretched between a fixed post and the hand.[1]

Now, if the hand holding the rope is moved quickly to and fro at right angles to the length of the rope, a

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[1] The use of the word "exactly" here may, or may not, be unjustified. There are certain things suggested by the analogy between ropes and lines of force for which no experimental evidence can be adduced at present. Thus the ropes are all quite distinct, and a disturbance started along one rope travels out in a straight line along that rope and does not start disturbances in other directions. In the case of light it is probable that the idea of regarding each rope as separate has to be abandoned, and that a disturbance started at any point travels indifferently out in all directions; that, in fact, in respect of the way in which a disturbance travels out from the source a better analogy is to be found in the case of waves excited by a stone thrown into a pond than in the case of a rope shaken at one end. But it is impossible to discuss this matter sufficiently without referring to problems of quite recent controversy.

wave which may be easily seen travels out along the rope. The properties which we have attributed to lines of force lead to the conclusion that waves might travel along them; can these waves be identified with those of light.

We have attributed to light waves only three properties—intensity, frequency, and velocity; if the relation between these three calculated for the waves along the lines of force is the same in all cases as that found experimentally for the waves of light, there can be no further question that the identification is correct. Now, the velocity with which the waves will travel along the lines of force is determined, as in the case of the rope, by their tension and their inertia. Both these quantities will vary with the medium in which the lines lie; the former can be measured by observations on the attraction of two bodies placed in the medium, the latter by observations on the phenomena described on p. 58 when the wire is placed in the medium; hence for any medium the velocity can be determined.

Comparing the predictions so attained with experiment, we find that in some cases there is agreement, in other cases disagreement. In a vacuum and in air or other gases, the velocity of the waves along the lines of force turns out to be very accurately that which has been measured for light. On the other hand, while in most transparent media the velocity of the light waves varies very considerably with the frequency, our theory predicts that waves of all frequencies should travel with the same velocity; in these media the predictions of theory agree with experiment only when the frequency is very small. It is beyond our province to discuss this matter further, and to show how, by the further development of science, this discrepancy has not only been cleared away, but has been made the instrument of some of our most searching and fruitful inquiries.

It must suffice to say that it has been cleared away, and that the electro-magnetic theory of light is universally accepted to-day as the firm basis of all optical theory. Almost all the progress that the last generation has seen in theoretical optics has been directly due to the suggestions of that theory.

## CHAPTER V

### MAXWELL'S THEORY

**35. Historical Considerations.**—If my only object were to explain the nature of the prevalent electrical theories, there would be little to add to the account of that which is based upon the ideas of Faraday. But it is neither possible nor desirable to ignore wholly the history of the development of a science; it would be misleading to imply that the line of thought which has been sketched actually led to the electro-magnetic theory of light, and the omission of all reference to the methods by which that theory was actually attained would lead to the neglect of one of the most important instruments used in the study of physics.

Faraday himself only sketched the bare outline of the theory which has been given; he merely indicated in a general way that the broad features of electric or magnetic action could be interpreted by the action of lines of force at rest or in motion. He was no mathematician, and he did not attempt to introduce into his theory the magnitudes which have been freely used in our discussion. He recognised, of course, that there was a tension along the length of the lines of force and a repulsion at right angles to them; but he did not define a magnitude based on those conceptions, or attempt to connect them with the measurable magnitudes introduced by the older electrical theory which he was endeavouring to replace. He does not seem even to have realised that the lines must possess inertia. The introduction of the conception of an inertia of the lines of force is due to J. J. Thomson, who, more than fifty years after Faraday's work and twenty years after the electro-magnetic theory of light had been established by quite different arguments, developed Faraday's ideas along the lines which have been sketched in the preceding chapter, and showed that they led directly to all the results which Maxwell had attained by other means.

Without defining the magnitudes, tension along the lines of force, and inertia of the lines, it would be impossible to predict with what velocity a disturbance would be propagated along the lines. The main proof of the electro-magnetic theory of light was therefore quite beyond the reach of Faraday. But it is certainly strange that the idea never occurred to him that these lines might possibly provide a mechanism for the propagation of disturbances through a vacuum; and it is all the more strange because Faraday was firmly convinced that a connection might be established between optical and electrical phenomena, and spent much time in trying experimentally to produce some influence on the propagation of light by subjecting the materials through which it passed to electric and magnetic forces. He eventually discovered such an influence, but it led to no further developments; indeed, it was not reconciled with the electric theory of light until to that theory had been added the yet more modern theory of electrons. The common feature of electrical and optical phenomena which forcibly suggests a connection between them is that both kinds of actions can be propagated through a vacuum; and Faraday failed because he did not look for the connection here.

It was left for Maxwell, as greatly the superior of Faraday in mathematical ability as his inferior in physical insight, to attain the all-important conclusions which are really the logical outcome of Faraday's ideas. But the method by which he attained them was wholly different from that which has been indicated in the foregoing paragraphs. He introduced the magnitudes derived from the conceptions of a tension along the lines of force and a pressure at right angles to them, but though he was the first to point out the analogy between an electric circuit and a system possessing inertia, he did not attribute inertia to the lines of force. He treated electrostatic phenomena consistently from Faraday's standpoint, but when he came to deal with electromagnetic phenomena, he reverted almost completely to the methods characteristic of Ampère. The theory which led him to the electro-magnetic theory of light, though influenced by Faraday's work, was logically independent of it. It was different in its very nature. Faraday's theory was a physical, Maxwell's a mathematical, theory; and here we must stop for a moment to consider briefly what a mathematical theory is.

**36. Mathematical Theories.**— We have seen that a physical theory, in one of its aspects, is a set of propositions from which other propositions (laws) can be deduced; logically, the propositions of the theory are simply equivalent to the laws which they are intended to explain, but they differ from those laws in suggesting other laws. In these respects a mathematical theory is very similar to a physical theory; it also consists of propositions from which laws can be deduced, and from it also can be deduced laws which were not contemplated in the original formation of the theory. The main difference between a physical and a mathematical theory lies in the nature of the propositions of which it is composed, and in the conceptions introduced by those propositions.

The conceptions of a physical theory are always derived from analogy with some mechanical system of which the action is quite familiar to our experience; the conceptions of a mathematical theory are of the same nature as those which are employed in the study of pure mathematics. What this nature is it would be impossible to describe within the limits of this little book, and unfortunately there are few books in which it is described in a manner suitable for the general reader. But with one set of these conceptions, the rational numbers, every one who has ever learnt arithmetic is acquainted, at least superficially. To these conceptions, and to many other conceptions of pure mathematics, the operations of addition and subtraction are applicable; but there are other conceptions of pure mathematics to which not only these operations are applicable, but also others which are not applicable to the rational numbers. It is these conceptions which are introduced by the branch of mathematics known as the infinitesimal calculus, and the use of which distinguishes modern from ancient mathematics.

Now, the magnitudes which are introduced by physical theories are analogous to the rational numbers, in so far as operations analogous to addition and subtraction are applicable to them, while the operations of the infinitesimal calculus are not applicable; from the very nature of these last operations it is impossible to define in terms of the sensations, which are the ultimate basis of physics, magnitudes to which they are applicable. It is impossible, therefore, to state scientific laws which involve conceptions to which the very powerful and highly developed methods of reasoning which make up modern mathematics can be applied. If we are to avail ourselves at all of these methods, we must introduce into our science conceptions which are not definable in terms of sensations—conceptions, that is, which are typical of theories and not of laws.

This introduction is effected by a mathematical theory. The physical laws state certain relations between the physical magnitudes. Thus if we are considering the magnetic effects due to a current circuit, we may have the following magnitudes—the strength of the current (C), the directive force exerted on a certain magnet (H), and various lengths (L, M, N, &c.) which determine the position of the magnet with respect to the circuit. The law states that when C has the value $c$, and L, M, N the values $l, m, n$, then H has the value $h$, where $c, l, m, n, h$ are all some definite numbers. The mathematical theory introduces new conceptions C′, H′, L′, M′, N′, to which the operations of the infinitesimal calculus are appli-

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cable, and states certain other relations between these conceptions; and these other relations must be such that it can be logically deduced from the propositions stated that if C′, L′, M′, N′ have the values $c$, $l$, $m$, $n$, then H′ has the value $h$. The propositions which lead to this conclusion form the mathematical theory. It is to be noted that the mathematical theory, like the physical, can be divided into assertions and a dictionary; the assertions are the propositions just described, and the dictionary is a set of propositions such as that, when I say that the value of C′ is $c$, I mean to assert that the value of C is $c$. This may seem all very unnecessarily complicated, but I assure the reader that many confusions have arisen from a failure to note the features here described.

In considering the methods by which mathematical theories are to be framed, we are met by difficulties similar to those which we noticed in dealing with physical theories. All that we know definitely is the laws which are to be deduced from the theory, while there are many different theories from which the same laws can be deduced. How is the choice between the innumerable different theories, all of which lead to the same laws, to be made? Once a theory is propounded it is possible to deduce from it not only the laws to explain which it was invented, but also very many other laws, and the theory will not be satisfactory unless these new laws also, when investigated, are found to be in accordance with observation; this feature supplies a criterion by which the value of a theory, when it is once propounded, may be tested, but it does not afford any help in propounding the theory for the first time.

As a matter of fact, an examination of the successful theories shows that they possess the same feature as successful physical theories; they are successful not only in explaining old laws and in predicting new ones, but also in satisfying the æsthetic needs of the intellect. The particular form which has been chosen out of the innumerable alternatives for the propositions of the chief mathematical theories has clearly in each case been selected because of its simplicity and neatness, because it possesses those perfectly indefinable qualities which are attractive to the pure mathematician and a mystery to every one else. We note again the striking fact that the propositions which have the almost miraculous power of predicting true laws are also those which appeal to these irrational desires of the intellect. It is this constant association of two very diverse properties which makes theories valuable and true science possible.

One important difference between physical and mathematical theories must be noted. We saw that a physical theory, apart from its aspect as a simple logical equivalent of the laws which it explains, is useful on account of what it suggests. The derivation from such a theory of any laws but those which it was invoked to explain can be made with success only by those endowed with the peculiar scientific intuition which renders the greatest physicists susceptible to the true and not to the false suggestions. But in the case of a mathematical theory the derivation of new laws is effected, not by suggestion to which only a few minds are susceptible, but by a process of rigorous logical deduction, which can be carried out by any one who has the necessary mathematical training and which is convincing to any one who can understand it. The conclusions drawn from a physical theory are attained by methods which can only be appreciated by a small minority, while those drawn from a mathematical theory are attained by methods which can be appreciated by any one who will undergo the necessary study. Since in the last resort conviction can only be attained by appealing to principles which everybody accepts, a mathematical theory has always carried with it much more conviction than a physical theory; the great mathematical theories, Newton's theory of gravitation or Ampère's theory of electro-magnetism, have never been doubted, while almost every physical theory has been met with scepticism at some time during its history. But it must not be imagined, therefore, that physical theories are any less valuable than mathematical theories, or that science could be developed without the use of the former. The logical nature of both and their relation to laws and the ultimate foundations of science is the same; the fact that new laws are derived from them by slightly different processes is of little moment when it is realised that, in one case as much as in the other, those new laws must be compared with experiment before they can be safely accepted as true. It is not reasonable to accept as true without further inquiry any result deduced from Ampère's electro-magnetic theory, and yet to insist on overwhelming experimental evidence before a result suggested by Faraday's theory is accepted. These considerations are of greater importance than a reader unfamiliar with the history of science, and with its present controversies, might imagine.

37. **Maxwell's Theory.**—It was such a mathematical theory that led Maxwell to his great discovery. The laws to explain which the theory was primarily designed, were those of the magnetic action due to a current circuit, and those of the induction of a current in a circuit by the change in a neighbouring magnet. Before Maxwell's time these laws had been formulated, and mathematical theory had been invented to explain them. Maxwell altered these theories in two ways. In the first alteration he acted under the influence of Faraday's theory; according to the older theory there could be in a complete vacuum no electric current giving rise to magnetic actions, because such actions were considered to be associated always with conducting circuits; but according to Faraday, the active agents were the moving lines of force which could move even if their ends were fixed, so that there might be an electric current giving rise to magnetic actions in a perfect vacuum. In representing this possibility in his theory, Maxwell was doubtless influenced by considerations of mathematical symmetry and simplicity. He had to introduce some electrical quantity which

could have a finite value even in a vacuum, and a change in which was to be associated with magnetic action; he chose this quantity, so that, when all the actions considered were taking place in a vacuum, the propositions of the theory explaining the laws of magnetic action due to a current were of precisely the same simple form as the propositions of the theory explaining the production of a current by the change of a magnet. The only difference between the two sets of propositions was that, where electrical quantities occurred in one, magnetic quantities appeared in the other; the two sets of propositions were perfectly symmetrical in a way which appealed to one who, like Maxwell, had an interest in pure mathematics.

Maxwell thus attained two sets of propositions, each of which stated relations between electrical quantities, magnetic quantities, and quantities general to all science, such as lengths and times. Inquiring further into the consequences of these propositions, he found that they led to the result that any change produced in the electrical or magnetic state at a certain place would appear at another place after a certain time, determined only by the distance between the two places and by the electrical and magnetic qualities of the medium between them. But this is precisely what is meant by saying that a disturbance is propagated from one place to another with a velocity determined by the electrical and magnetic properties of the medium. If these properties were known, and they could be determined by experiment, the velocity of propagation would be known. On comparing this predicted velocity of propagation with the known velocity of light in the medium, the conclusions were attained which were stated on p. 75. Thus arose the electro-magnetic theory of light; it was not accepted immediately until further consequences had been deduced from Maxwell's theory and again compared with experiment. The most striking advances in this direction were made by Hertz, and led to the final acceptance of Maxwell's theory. But any further consideration of these matters would lead us too far; my object throughout has been to explain rather what science means and how a science develops, than to describe in any detail the actual results which have been attained.

**38. Later Developments.**—At this stage there is a clear and distinct break in the development of electrical science. The older science ends and the newer science begins, which has made such vast strides in the last fifteen years and revolutionised our conceptions of almost all natural phenomena. And though there was a considerable period during which no notable advance was made, it was clear to all the most far-sighted men of science of the time in what direction the new advance would ultimately be made. We have noticed that the electro-magnetic theory of light is thoroughly complete and in accordance with all the known facts, so long as the propagation of light through a vacuum is concerned; it is only when an attempt is made to correlate the electrical and optical properties of material bodies that discrepancies begin to appear.

When we begin to inquire a little closer into even the purely electrical properties of material bodies, places in which none of the theories elaborated are completely satisfactory begin to appear. Some of these have been briefly indicated in our survey; the sharp division which it is necessary to make for the purposes of electrostatics between conductors and non-conductors breaks down on further examination, for all bodies are found to possess in some degree the characteristics of both classes; and again there was a difficulty in reconciling our explanation of the phenomena of charging by induction with our assumption of an inability of the electricity to move in non-conducting bodies. The number of such instances could be multiplied almost indefinitely, and it is clear that, before our theory of electricity is complete, far more attention will have to be made to the differences as well as to the resemblances of different materials in respect of their electrical qualities. We have regarded material bodies hitherto merely as altering slightly the electrical properties of the vacuum in which they are placed; we have made no consistent attempt to connect the electrical properties of bodies with their other properties, or to produce any physical theory to explain those properties. It is in this direction that the attack must now be directed. In terms of the oldest theory of electricity the question might be asked: How are the equal quantities of opposite electricities which are supposed to be present in uncharged bodies distributed within those bodies, and what is the relation between those charges and the atoms or molecules of which the bodies are composed? It was indeed just in this form that the question was put when the first indications of the solutions of the outstanding difficulties was attained. Once more a theory has proved almost more valuable for its failures than for its successes; if the laws which were rightly predicted by Maxwell's theory may be regarded as completing the edifice of the older electrical science, it was the laws which it predicted wrongly which caused the new edifice to arise.

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It is somewhat difficult to comply with the request of the Editor to give hints as to further reading without a more definite knowledge of the requirements of the reader. If this little volume were to be regarded as an introduction to the science of electricity for those who intended to pursue the study seriously, the obvious course to recommend would be the perusal of one of the numerous treatises on the science which are published in the form of textbooks. To attempt to select one of these as the most admirable is a task from which the boldest would shrink; it may be merely recorded that one of the best known is the *Electricity and Magnetism* of Joubert, Foster, and Atkinson, published by Longmans.

But it is probable that any one into whose hands this book falls will either be adequately acquainted with the contents of such textbooks, or will not have a sufficiently specialised interest in the science to care to become so. If the former alternative is correct, no advice as to reading will be necessary; I would merely point out the intense interest of Faraday's accounts of his own researches, which are published in a collected form in three volumes (now out of print); the main part of the theory sketched in Chapter V. is contained in vol. i. series xi. A volume of quite equal interest in the same direction is the collection of the researches of Henry Cavendish, edited by Maxwell (Camb. Univ. Press); the brilliance of Cavendish's work is hardly surpassed even by Faraday, who was largely engaged in rediscovering the unpublished results of his predecessor.

The reader whose interests are more general may also with advantage study some parts of Faraday's writings; but much of them he will find difficult and confused, unless he has at hand a scientific friend to whom he may turn for help and for explanation of the modern views of the problems which the author attacks. Of general works on the theory of electricity which are in any way suited for an intelligent lay reader, the only example which I know is the *first* edition of Sir Oliver Lodge's *Modern Views of Electricity* (Macmillan). If he requires more detailed information than can be given in any comprehensive survey, the only course open to him is to pick what he wants from the textbooks mentioned before.

The literature suited for the lay reader which deals with the developments of electrical science more recent than any which are treated here is much richer. The following books can be recommended, though I must confess that they all appear to me to fall into just those errors which the author of this little volume has tried so carefully to avoid; it must be remembered too that they deal with a growing science, that many of the statements which were true yesterday are not true to-day, and that opinions differ on very important points.

Whetham's *Recent Advances in Physical Science* (Murray).

Lodge's *Modern Views of Electricity* (Macmillan).

Fournier d'Albe's *The Electron Theory.*

Sir J. J. Thomson's two little volumes, *Electricity and Matter* and *The Corpuscular Theory of Matter* (Constable), should certainly be read by any one who has the small amount of special knowledge required ; they share with Faraday's writings the interest which attaches to a contemporary account of his views by one who is foremost in attacking the problems discussed.

My own *Modern Electrical Theory* (Camb. Univ. Press) contains some parts which might be regarded as a sequel to this volume ; but it is more of the nature of a textbook and, moreover (like all the volumes mentioned), is sadly out of date.

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