

## CHAPTER V

### THE EXPLANATION OF LAWS

THROUGHOUT the previous chapter I wrote as if the ordering of nature which is effected by laws was all that was necessary to satisfy our intellectual desires and so to fulfil the purpose of science. But really when we have discovered laws, we have fulfilled only part of the purpose of science. Even if we were sure that all possible laws had been found and that all the external world of nature had been completely ordered, there would still remain much to be done. We should want to *explain* the laws.

Explanation in general is the expression of an assertion in a more acceptable and satisfactory form. Thus if somebody speaks to us in a language we do not understand, either a foreign language or the technical language of some study or craft with which we are not familiar, we may ask him to *explain* his statement. And we shall receive the explanation for which we ask if he merely alters the form of his statement, so as to express it in terms with which we are familiar. The statement in its new form is more acceptable and more satisfactory, because now it evokes a definite response in our minds which we describe by saying that we understand the statement. Again we sometimes ask a man to *explain* his conduct; when we make such a demand we are ignorant, or pretending to be ignorant, of the motives which inspired his action. We shall feel that he has offered a complete explanation if he can show that his motives are such as habitually inspire our own actions, or, in other words, that his motives are familiar to us.

But expressions, or the ideas contained in them, may be more acceptable and more satisfactory, on grounds other than their familiarity; and all explanations do not consist of a reduction of the less to the more familiar. Indeed it would seem that the explanations which, in the view of the man in the street, it is the business of science to offer do not involve familiar ideas at all. Thus we may expect our scientific acquaintances to *explain* to us *why* our water-pipes burst during a frost or *why* paint becomes dirty sooner in a room lit by gas than in one lit by electricity. We shall be told in reply that the bursting of the pipes is due to the expansion of water when it is converted into ice, and the blackening of the paint to the combination of the white pigment with sulphur present in coal-gas to form compounds that are dark, and not white, in colour. Now in these instances, the ideas involved in the explanation are probably less, and not more, familiar than those that they are used to explain. Many more people know that water-pipes burst during a frost than know that water (unlike most liquids) expands when it freezes; and many more know that their paint goes black than know that lead carbonate (one of the commonest white pigments) is converted by sulphur into black lead. sulphide.

Why then do we regard our questions as answered? Why do we feel that, when we have received them, the matter is better understood, and our ideas on it clearer and more satisfactory? The reason is that the events and changes have been explained by being shown to be particular examples of a general law. Water *always* expands when it freezes, although it does not always burst household pipes; for it may not be contained in pipes or in any closed vessel. And lead carbonate *always* reacts with sulphur in the form present in coal-gas, even if it is not being used as a pigment. We feel that our experience is no longer peculiar and mysterious; it is only one instance of general and fundamental principles. It is one of the profoundest instincts of our intellectual nature to regard the more general principle as the more ultimately acceptable and satisfactory; it is this instinct which led men first to the studies that have developed into science. In fact, what was called in the last chapter the "ordering of experience by means of laws might equally well have been called the explanation of that experience. Laws explain our experience because they order it by referring particular instances to general principles; the explanation will be the more satisfactory the more general the principle, and the greater the number of particular instances that can be referred to it. Thus, we shall feel that the bursting of the pipes is explained more satisfactorily when it is pointed out that the expansion of water when converted into ice explains also other common experience, for instance that a layer of ice forms first on the top of a pond and not on

the bottom.

Doubtless there are other kinds of explanation; but it is important for our purpose to notice that the explanations of common life often depend on these two principles—that ideas are more satisfactory when they are more familiar and also when they are more general; and that either of these principles may be made the basis of an explanation.

When it is asked what is the nature of the scientific explanation of laws—and it is the purpose of this chapter to answer that question—it is usually replied that it is of the second kind, and that laws are explained by being shown to be particular examples of more general laws. On this view the explanation of laws is merely an extension of the process involved in the formulation of laws ; it is simply a progress from the less to the more general. At some stage, of course, the process must stop; ultimately laws so general will be reached that, for the time being at least, they cannot be included under any more general laws. If it were found possible to include all scientific laws as particular instances of one extremely general and universal law, then, according to this opinion, the purpose of science would be completely achieved.

I dissent altogether from this opinion; I think it leads to a neglect of the most important part of science and to a complete failure to understand its aims and development. I do not believe that laws can ever be explained by inclusion in more general laws; and I hold that, even it were possible so to explain them, the explanation would not be that which science, developing the tendencies of common sense, demands.

The first point is rather abstruse and will be dismissed briefly. It certainly seems at first sight that some laws can be expressed as particular instances of more general laws. Thus the law (stating one of the properties of hydrogen) that hydrogen expands when heated seems to be a particular instance of the more general law, that all gases expand when heated. I think this appearance is merely due to a failure to state the laws quite fully and accurately, and that if we were forced to state with the utmost precision what we mean to assert by a law, we should find that one of the laws was not a particular case of the other. However, I do not wish to press this contention, for it will probably be agreed that, even if we have here a reference of a less general, to a more general law, we have no explanation. To say that all gases expand when heated is not to explain why hydrogen expands when heated; it merely leads us to ask immediately why all gases expand. An explanation which leads immediately to another question of the same kind is no explanation at all.

## WHAT IS A THEORY?

How then does science explain laws? It explains them by means of “theories,” which are not laws, although closely related to laws. We will proceed at once to learn what a theory is, and how it explains laws.<sup>1</sup> For this purpose an example is necessary, even though its use involves entering more into the details of science than is our usual practice. A great many laws are known, concerning the physical properties of all gases ; air, coal-gas, hydrogen and other gases, differ in their chemical properties, but resemble each other in obeying these laws. Two of these laws state how the pressure, exerted by a given quantity of gas on its containing vessel, varies with the volume of the vessel, and with the temperature of the gas. Boyle’s Law states that the pressure is inversely proportional to the volume, so that if the volume is halved the pressure is doubled; Gay-Lussac’s states that, at a constant volume, the pressure increases proportionally to the temperature (if a certain scale of temperature is adopted, slightly different from that in common use). Other laws state the relation between the pressure of the gas and its power of conducting heat and so on. All these laws are “explained” by a doctrine known as the Dynamical Theory of Gases, which was proposed early in the last century and is accepted universally to-day. According to this theory, a gas consists of an immense number of very small particles, called molecules, flying about in all directions, colliding with each other and with the wall of the containing vessel; the speed of the flight of these molecules increases with the temperature; their

<sup>1</sup> The reader should be warned that he must remember that the word “theory” is here used in a strictly technical sense, of which the meaning is about to be explained. He must not attach to it any of the ideas associated with the word in ordinary language. In Chapter VIII some reference

will be made to the use of "theory" in contra-distinction to "practice."

impacts on the walls of the vessel tends to force the walls outwards and represent the pressure on them; and by their motion, heat is conveyed from one part of the gas to another in the manner called *conduction*.

When it is said that this theory explains the laws of gases, two things are meant. The first is that if we assume the theory to be true we can prove that the laws that are to be explained are true. The molecules are supposed to be similar to rigid particles, such as marbles or grains of sand; we know from the general laws of dynamics (the science which studies how bodies move under forces) what will be the effect on the motions of the particles of their collisions with each other and with the walls; and we know from the same laws how great will be the pressure exerted on the walls of the vessel by the impacts of a given number of particles of given mass moving with given speed. We can show that particles such as are imagined by the theory, moving with the speed attributed to them, would exert the pressure that the gas actually exerts, and that this pressure would vary with the volume of the vessel and with the temperature in the manner described in Boyle's and Gay-Lussac's Laws. In other words, from the theory we can deduce the laws.

This is certainly one thing which we mean when we say that the theory explains the laws; if the laws could not be deduced from the theory, the theory would not explain the laws and the theory would not be true. But this cannot be all that we mean. For, if it were, clearly any other theory from which the laws could be deduced, would be equally an explanation and would be equally true. But there are an indefinite number of "theories" from which the laws could be deduced; it is a mere logical exercise to find one set of propositions from which another set will follow; and anyone could invent in a few hours twenty such theories. For instance, that the two propositions (i) that the pressure of a gas increases as the temperature increases (z) that it increases as the volume decreases, can be deduced from the single proposition that the pressure increases with increase of temperature and decrease of volume. But of course the single proposition does not explain the two others; it merely states them in other words. But that is just what logical deduction consists of; to deduce a conclusion from premisses is simply to state the premisses in different words, though the words are sometimes so different as to give quite a different impression.' If all that we required of a theory was that laws could be deduced from it, there would be no difference between a theory which merely expressed the laws in different words without adding anything significant and a theory which, like the example we are considering, does undoubtedly add something significant.

It is clear then that when we say the theory explains the laws we mean something additional to this mere logical deduction; the deduction is necessary to the truth of the theory, but it is not sufficient. What else do we require? I think the best answer we can give is that, in order that a theory may explain, we require it—to explain! We require that it shall add to our ideas, and that the ideas which it adds shall be acceptable. The reader will probably feel that this is true of the explanation of the properties of gases offered by the dynamical theory. Even if he did not know (and he probably does not know apart from what I have just told him) that the laws can be deduced from the theory, he would feel that the mere introduction of moving particles and the suggestion that the properties of a gas can be represented as due to their motion would afford some explanation of those properties. They would afford some explanation,

1 The reader should be warned that some people would dissent vehemently from this assertion, even if the laws could not be deduced correctly; they would then offer an explanation, although the explanation would not be true.

And this is, I believe, the reason why he would feel thus. Only those who have practised experimental physics, know anything by actual experience about the laws of gases; they are not things which force themselves on our attention in common life, and even those who are most familiar with them never think of them out of working hours. On the other hand, the behaviour of moving solid bodies is familiar to every one; every one knows roughly what will happen when

such bodies collide with each other or with a solid wall, though they may not know the exact dynamical laws involved in such reactions. In all our common life we are continually encountering moving bodies, and noticing their reactions; indeed, if the reader thinks about it, he will realize that whenever we do anything which affects the external world, or whenever we are passively affected by it, a moving body is somehow involved in the transaction. Movement is just the most familiar thing in the world; it is through motion that everything and anything happens. And so by tracing a relation between the unfamiliar changes which gases undergo when their temperature or volume is altered, and the extremely familiar changes which accompany the motions and mutual reactions of solid bodies, we are rendering the former more intelligible; we are explaining them.

That is to say, the explanation of laws offered by theories (for this example has been offered as typical) is characteristically explanation of the first of the two kinds with which the chapter started. It is explanation by greater familiarity, essentially similar to that offered when a statement is translated from an unknown to a known language. This conclusion may be surprising, and indeed it is not that generally advanced. Before developing our view further, it will be well to examine the matter from another point of view.

### DIFFERENCE BETWEEN THEORIES AND LAWS

It was stated before that it has been usually held that the explanation of laws consists in showing that they are particular examples of more general laws. If this view were applied to the example under discussion, it might be urged that the dynamical theory explains the properties of gases because it shows that they are particular examples of the laws of dynamics; the properties of gases are explained because they are shown to be the consequences of the subjection of the molecules, of which the gases consist, to the general laws of all moving bodies. Here, it might be said, is the clearest possible instance of explanation by generalization, a simple extension of the process involved in the discovery of laws.

But, against this view, it must be pointed out that the most important feature of the theory is not that it states that molecules are subject to dynamical laws, but that which states that there are such things as molecules, and that gases are made up of them. It is that feature of the theory which makes it a real explanation. Now this part of the theory is not a particular instance of any more general law; indeed it is not a law or anything that could be an instance of a law. For it is not, according to the criterion laid down in Chapter II, part of the proper subject-matter on which science builds its foundations. Molecules are not things which we can see or feel; they are not, like the ordinary material bodies to which the laws of dynamics are known to apply, objects discernible to direct perception. We only know that they exist by inference; what we actually observe are gases, varying in temperature and pressure; and it is only by these variations that we are led to suspect the existence of the molecules. We may apply once more our fundamental test of universal agreement which serves to distinguish the objects concerned in laws from any others. If somebody denied the existence of molecules, how could we prove him wrong? We cannot show him the molecules; we can only show him the gases and expound the theory; if he denied that the theory proved the existence of the molecules, we should be powerless. We cannot prove by his actions that he is perverse or deluded; for his actions will be affected only by the properties of gases, which are actually observed, and not by the theory introduced to explain them. Actually the dynamical theory of gases has been denied by men of science of high distinction. Usually the denial was based partly on the assertion that the laws of gases could be deduced accurately from the theory, but it has often been accompanied by the contention that, even if they could be deduced accurately, the theory was not true, and not worthy of acceptance. No denial of that case would be possible if the theory were indeed a law.

We conclude therefore—and the conclusion is vital to the view of science presented here—that a theory is not a law, and consequently, that the explanation afforded by a theory cannot simply be the explanation by generalization which consists in the exhibition of one law as a particular instance of another. It does not follow that theories have nothing to do with laws, and that it is

immaterial for the theory that the laws of dynamics are true, and of very great generality. We shall see presently that this feature is of great importance. But it does not involve that the theory is itself a law.

## THE VALUE OF THEORIES

After this protest against a dangerous misunderstanding, let us return and develop further our view of theories. So far the truth of a theory has been based on two grounds; first, that the laws to be explained can be deduced from it; second, that it really explains in the sense that has been indicated. But actually there is in addition, a third test of the truth of a theory, which is of great importance; a true theory will not only explain adequately the laws that it was introduced to explain; it will also predict and explain in advance laws which were unknown before. All the chief theories in science (or at least in physics) have satisfied this test; they have all led directly to the discovery of new laws which were unsuspected before the theory was proposed.

It is easy to see how a theory may predict new laws. The theory, if it is worthy of consideration at all, will be such that the old laws can be deduced from it. It may easily be found on examination that not only these laws, but others also can be deduced from it; so far as the theory is concerned, these others differ in no way from the known laws, and if the theory is to be true, these laws that are consequences of it must be true. As a matter of fact, it is very seldom that a theory, *exactly* in its original form, predicts any laws except those that it was proposed to explain; but a very small and extremely natural development of it may make it predict new laws. Thus, to take our example, in order to explain the laws (Boyle's and Gay-Lussac's) to which the theory was originally applied, it is unnecessary to make any assumption about the size of the molecules; those laws can be deduced from the theory whatever that size (so long as it is below a certain limit) and the assumption was at first made for simplicity that the molecules were mathematical points without any size at all. But obviously it was more natural to assume that the molecules, though extremely small, have some size and once that assumption is made,

'If a drop of water were magnified to the size of the earth, the molecules would be about the size of cricket balls.

Laws are predicted which had not been discovered at the time and would never have been suspected apart from the theory. Thus, it is easy to see that, if the molecules have a definite size, the behaviour of a gas, when the number of molecules contained in a given vessel is so great that the space actually occupied by the molecules is nearly the whole of the space in the vessel, will be very different from its behaviour when there are so few molecules that practically all that space is unoccupied. This expectation, a direct result of the theory, is definitely confirmed by experiments which show a change in the laws of a gas when it is highly compressed, and all its molecules forced into a small volume.

This test of predicting new and true laws is always applied to any theory when it is proposed. The first thing we do when anyone proposes a theory to explain laws, is to try to deduce from the theory, or from some slight but very natural development of it, new laws, which were not taken into consideration in the formulation of the theory. If we can find such laws and prove by experiment that they are true, then we feel much more confidence in the theory; if they are not true, we know that the theory is not true; but we may still believe that a relatively slight modification will restore its value. It is in this way that most new laws are actually suggested for the purposes discussed in the previous chapter. At the present time, in the more highly developed sciences, it is very unusual for a new law to be discovered or suggested simply by making experiments and observations and examining the results (although cases of this character occur from time to time); almost all advances in the formulation of new laws follow on the invention of theories to explain the old laws. Indeed it has been urged that the only use of theories is thus to suggest laws among which some will be found to be true. This opinion has been much favoured by philosophers and mathematicians and has always been accompanied by the opinion that it is the

end and object of science to discover laws It has also been professed (especially at the end of the nineteenth century) by people who know something about science and actually practised it; but I think that these people only professed the view because they were afraid what the philosophers might say if they denied it. At any rate, for myself, I cannot understand how anybody can find any interest in science, who thinks that its task is completed with the discovery of laws.

For the explanation of laws, though it is formally quite a different process from the discovery of laws, is in its object merely an extension of that process. We seek to discover laws in order to make nature intelligible to us; we seek to explain them for exactly the same reason. The end at which we are aiming in one process as in the other is the reconciliation with out intellectual desires of the perceptions forced on us by the external world of nature What possible reason can be given for attaching immense importance to one stage in the process and denying all intrinsic importance to another? Surely so long as anything remains to be explained it is the business of science to continue to seek explanations.

## THE INVENTION OF THEORIES

And here again arises obviously a question very similar to that discussed in the previous chapter. A theory, it has been maintained, is some proposition which satisfies these conditions (1) It must be such that the laws which it is devised to explain can be deduced from it; (2) it must explain those laws in the sense of introducing ideas which are more familiar or, in some other way, more acceptable than those of the laws ; (3) it must predict new laws and these laws must turn out to be true. Of course we have to ask now how such theories are to be found; we might find theories to satisfy the first two conditions by the exercise of sufficient patience in a process of trial and error; but how can we possibly be sure that they will satisfy the third? The answer that must be given will be clear to the reader if he has accepted the results of the previous discussion. There cannot be, from the very nature of the case, any kind of rule whereby the third condition may certainly be satisfied; the meaning of the condition precludes any rule. Actually theories are always suggested in view of the first two conditions only; and actually it turns out that they often fulfil the third. And once again they most often turn out to fulfil the third when they are suggested by those exceptional individuals who are the great men of science. It is when the theory seems to *them* to explain the laws, when the ideas introduced by it appear to *them* acceptable and satisfactory, that nature conforms to their desires, and permits to be established by experiment the laws which are the direct consequence of those ideas.

The form in which that statement is put may appear rather extravagant, and we shall return later and consider some questions which it seems to raise. But the general view that true theories are the expression of individual genius will probably seem less paradoxical than the view put forward in the previous chapter, that true laws also contain a personal element. The difference is indicated by the words that are used; we speak of the "discovery" of a law, but of the "invention" of a theory. A law, it is implied, is something already existing which merely lies hidden until the discoverer discloses it; a theory, on the other hand, does not exist apart from the inventor; it is brought into being by an effort of imaginative thought. I do not think that distinction will bear examination; it seems to me very difficult to regard laws and not theories as something existing independently of investigation and wholly imposed by the external world, or theories and not laws as the product of the internal world of the intellect. For both theories and laws derive their ultimate value from their concordance with nature and both arise from mental processes of the same kind. Moreover, as has been suggested already, in the more highly developed sciences of today theories play a very large part in determining laws; they not only suggest laws which are subsequently confirmed by experimental investigation, but they also decide whether suggested laws are or are not to be accepted. For, as our discussion in the previous chapter showed, experiment alone cannot decide with perfect definiteness whether or no a law is to be accepted; there are always loopholes left which enable us to reject a law, however much experimental evidence may suggest it and enable us to maintain a law (slightly modified) even when experimental evidence seems directly to contradict it. An examination of any actual science will show that the acceptance

of a law is very largely determined by the possibility of explaining it by means of a theory; if it can be so explained, we are much more ready to accept it and much more anxious to maintain it than we should be if it were not the consequence of some theory. Indeed many laws in science are termed "empirical" and regarded with a certain amount of suspicion; if we inquire we find that an empirical law is simply one of which no theoretical explanation is known. In the science of physics at least, it would almost be more accurate to say that we believe our laws because they are consequences of our theories than to say that we believe our theories because they predict and explain true laws I

On such grounds I reject the view (though it is generally prevalent) that laws are any less the product of imaginative thought than are theories. The problem why nature conforms with our intellectual desires arises just as clearly with one as with the other. Nevertheless it is doubtless true that the personal and imaginative element is more obvious and more prominent in theories than in laws. One aspect of this difference has been noted already; the acceptance of a theory as true does involve a personal choice in a way that a law does not. Different people do differ about theories; they can choose whether or no they will believe them; but people do not differ about laws; there is no personal choice; universal agreement can be forced. Again, if we look at the history of science, We shall find that the great advances in theory are more closely connected with the names of the great men than are the advances in laws. Every important theory is associated with some man whose scientific work was notable apart from that theory; either he invented other important theories or in some other way he did scientific work greatly above the average. On the other hand there are a good many well-known laws which are associated with the names of men who, apart from those particular laws, are practically unknown; they discovered one important law, but they have no claim to rank among the geniuses of science.<sup>2</sup> That fact seems to indicate that a greater degree of genius is needed to invent true theories than to discover true laws.

The same feature appears in the early and prehistoric stages of science. Science, as we have seen, originally took over from common sense laws which had been already elaborated; and although it has greatly refined and elaborated those laws and has added many new types, it has never wholly abandoned the laws of common sense. Modern science depends as much as the crudest common sense on the notion of a "substance" (a notion

1 Except in so far as people may refuse to admit a law or to regard it as anything but empirical, because it is not in accordance with some theory. But then they admit that the laws describe the facts rightly; they only suggest that some other and equally accurate way of describing them would be preferable.

2 In case this book falls into the hands of some expert physicist, I would suggest that examples may be found in the laws of Stefan, Dulong and Petit, or Bode.

which, as we have seen, implies a law), on the notion of the succession of events in time and the separation of bodies in space and so on. But science has abandoned almost all the theories of pre-scientific days. For there were and are such non-scientific theories; and it is because the plain man has theories of his own, just as much as the most advanced man of science, that it has not been necessary to occupy a larger space in explaining exactly what a theory is; the reader will probably have recognized at once something familiar in the kind of explanation which the dynamical theory of gases offers. The most typical theories of the pre-scientific era were those which explained the processes occurring in nature by the agencies of beings analogous to men—gods, fairies, or demons. The "Natural Theology" of the eighteenth century which tried to explain nature in terms of the characteristics of a God, known through His works, was a theory of that type; in the features which have been described as essential to theories it differed in no way from that which we have discussed. But all such theories have been abandoned by science; the theories that it employs are of a type quite unknown before the seventeenth century.<sup>1</sup> In respect of theories science has diverged completely from common sense; and the divergence can be traced accurately to the work of a few great men. Common sense is therefore more ready to accept theories rather than laws as the work of individual genius.

But while I accept fully the view that the formulation of a new theory, and especially of a new type of theory,

1 An exception is often made in favour of Lucretius, who wrote about 70 B.C. But my own opinion is that moderns, with their fuller knowledge, read into his works ideas which never entered the head of their author. I do not think that (as Mr. Wells maintains in his

Outline of History “) it was merely the barrenness of the soil on which his seed fell that prevented it blossoming into fruit. The sterility of his ideas, contrasted with those of Galileo and Newton, was inherent in them.

is a greater achievement than the formulation of a new law, I cannot admit that the two processes are essentially different. As Galileo was the founder of experimental physics, so Newton was the founder of theoretical physics; as Galileo first introduced the type of law which has become most characteristic of the science, so Newton introduced first the characteristic type of theory. And of the two Newton is rightly judged by popular opinion to have been the greater man. But it is not rightly recognized how great was the achievement of Galileo; indeed his fame is usually associated with things—the observation of the isochronism of the pendulum, or his fight with clericalism over the Copernican theory— other than with his greatest service to science. It is his establishment of the first experimental numerical law that constitutes his highest claim to greatness, and that law was as much an expression of his personality as the theory of Newton.

## THE ANALOGIES OF THEORIES

Mention has just been made of “types” of theories. There are such types, just as there are types of laws, and they play the same rôle in permitting lesser men to complete and extend the work of the greater. Once a theory of a new type has been invented and has been shown to be true in the explanation of laws, it is naturally suggested that similar theories may prove equally successful in the explanation of other laws. And on the whole the suggestion has proved true. In each branch of science there are certain very broad and general theories which have been invented by the founders of that branch; subsequent development of that branch usually consists largely in the amplification and slight modification of such fundamental theories by investigators, many of whom could never have themselves laid the foundation. Indeed, the investigator often feels that in finding an explanation for the laws that he has discovered he has little more latitude than in discovering those laws; it is perfectly clear from the outside what kind of theory he must seek, just as it is clear what kind of law he must seek.

Thus, it may be stated broadly<sup>1</sup> that from 1700 to 1870 all physical theories were of a single type of which the dynamical theory of gases which we have used as an example provides an excellent instance. They were all “mechanical” theories. In our example, the behaviour of gases is explained by an analogy with a piece of mechanism, a set of moving parts reacting on each other with forces which determine and are determined by the motion. That feature is common to all the mechanical theories which played so great a part in the older physics and are still prominent in the newer; they explain laws by tracing an analogy between the system of which the laws are to be explained and some piece of mechanism. Once it was realized that such theories were likely to turn out to be true, the task of inventing theories was greatly simplified; it often became simply that of devising a piece of mechanism which would simulate the behaviour of the system of which the laws were to be explained.

But all scientific theories are not mechanical. In physics it is the admission of theories that do not fall within this class which distinguishes the newer from the older study. And in other branches of science (except where they are obviously founded on physics) theories of other kinds are the rule. For instance, the theory of evolution, proposed to explain the diversity and yet the resemblances of different kinds of living beings is

1 A very important exception must be made of the purely mathematical theories, such as those of Fourier and Ampere. Some indication of the nature of these theories is given in chapter VII.

not mechanical; it does not trace an analogy between the production of such beings and the operation of a piece of mechanism. Can we find any feature that is common to all theories that have proved to be true, or must we (as in the case of laws) rest content with several distinct but well-defined types which have all proved successful and yet display no common characteristic?

I think we can find such a feature. The explanation offered by a theory (that is to say, the part of the theory which does not depend simply on the deduction from it of the laws to be explained) is always based on an analogy, and the system with which an analogy is traced is always one of which the laws are known; it is always one of those systems which form part of that external world of which the subject-matter of science consists. The theory always explains laws by showing that if we imagine that the system to which those laws apply consists in some way of other systems to which some other known laws apply, then the laws can be deduced from the theory. Thus our theory of gases explains the laws of gases on the analogy of a system subject to dynamical laws. The theory of evolution explains the laws involved in the assertion that there are such-and-such living beings by supposing that these living beings are the descendants of others whose characters have been modified by reaction to their surroundings in a manner which is described by laws applicable to living beings at the present day. Again the immense theory involved, in the whole science of geology explains the structure of the earth as it exists to-day by supposing that this structure is the result of the age-long operation of influences, the action of which is described by laws observable in modern conditions. In each case the "explaining" system is supposed to operate according to known laws, but it is not a system of which those laws can be asserted as laws, because it is, by the very supposition underlying the theory, one which could never be observed—either because it is too small or too remote in the past or for some similar reason—and therefore does not form part of the proper subject-matter of science.

It is because the explanation offered by a theory is always based on an analogy with laws that the distinction between laws and theories has been so often overlooked. The statement of the dynamical theory of gases about the properties and behaviour of molecules, or of the theory of evolution about the process whereby the existing species of living beings came into existence, is so similar to the statement, asserted by a law, about the properties of actual mechanical systems or about the changes that are proceeding in existing species that the vital difference between the two is forgotten. The statement asserted by a law can be proved by direct perception; it states something which can be observed and which can be the subject of universal assent. The statement involved in the theory cannot be proved by direct perception, for it does not state anything that can be or has been observed. The failure to observe this distinction and the consequent failure to give to theories their true place in the scheme of science is the cause of most of the misunderstandings that are so widely prevalent concerning the nature and objects of science. For it has been admitted that, though the discovery of laws depends ultimately not on fixed rules but on the imagination of highly gifted individuals, this imaginative and personal element is much more prominent in the development of theories the neglect of theories leads directly to the neglect of the imaginative and personal element in science. It leads to an utterly false contrast between "materialistic science and the "humanistic" studies of literature, history and art.

## SCIENCE AND IMAGINATION

At the risk of wearying the reader by endless repetition I have insisted on the fallacy of neglecting the imaginative element which inspires science just as much as art. If this book is to fulfil any useful purpose that insistence is necessary. For it is my object to attract students to science and to help them to understand it by showing them from the outset what they may expect from it. It is certain that one of the chief reasons why science has not been a popular subject in adult education, and is scarcely recognized even yet as a necessary element of any complete education, is the impression that science is in some way less human than other studies. And for that impression men of science are themselves more to blame than anyone else; in a mistaken endeavour to exalt the certainty of their knowledge they deliberately conceal that, like all possible knowledge, it is personal. They exhibit to the outside world only the dry bones of science from which the spirit has departed.

It is true that it is less easy for the beginner to grasp the imaginative element in science than in some other studies. A larger basis of mere information is perhaps required before it becomes apparent. And, of course, he can never hope to share himself the joy of discovery; but in that respect he is no worse off than many who make of science their life-work. But he can, if he will take the trouble, appreciate the discoveries of others and experience at second-hand the thrill of artistic creation. For those who have the necessary knowledge, it is as exciting to trace the development of a great scientific theory, which we could never have developed ourselves, as it is to read great poetry or to hear great music which we ourselves could never have written. But I must admit that the books on science are few which make it easy for the beginner to share that experience. And so, although I can hardly hope that I shall succeed where so many writers have failed, and although the attempt transgresses the strict limit of an introduction, I should like to try to tell again the familiar story of one of the most wonderful romances of science—the story of Newton and the apple <sup>1</sup>

The early chapters of the story must be greatly abbreviated. Copernicus and Kepler, a century before Newton, had shown clearly what were the paths in which the planets move about the sun and the satellites, such as our moon, about the planets. It does not seem clear whether anyone before Newton had thought of inquiring *why* they should move in such paths, or had ever contemplated the possibility of explaining the laws which Kepler had laid down. In science, as in many other things, it is often much harder to ask questions than to answer them. People might have said, and many probably did say: The planets have to move somehow; the paths Kepler describes are quite simple; why should not the planets move in them? It is as ridiculous to ask why they so move as to ask why a man's hair is yellow or brown or red, and not blue or green. The mere conception of explaining the paths of the planets was itself an immense achievement.

And we can see now what suggested it to Newton. Some sixty years before Galileo had, for the first time, discovered some of the laws which govern the motions of bodies under forces. He had shown that, in some simple instances at least, there were such things as "laws of dynamics." The idea occurred to Newton, May not the movements of the planets and their satellites be subject to just such laws of dynamics as Galileo had discovered for the ordinary bodies which we see and handle. If so,

<sup>1</sup> Of course the apple may be mythical—like all the historical objects of our childhood. And it is impossible to be certain what Newton really thought. But his thought might have followed the line suggested here.

we ought to be able to find a set of forces such that if they act on the planets according to Galileo's laws, the planets will move as Kepler has shown that they do move. That seems to us very obvious now; but it was not obvious then. Galileo, as far as we know, never thought of it; nor did anyone in the two generations between him and Newton. And perhaps one reason why nobody thought of it was that they realized instinctively that if they had thought of it they could have got no further. To-day any clever schoolboy could solve the next problem which presents itself, namely that of finding *what* forces, acting according to Galileo's laws, would make the planets move as they do; but that is only because Newton has shown him the way. In order to solve this problem which seems to us now so easy, Newton had to invent modern mathematics; he had to make a greater advance in mathematics than had been made in all the time since the high-water of Egyptian civilization. This achievement of his was quite as wonderful as any other; but as it was not characteristically scientific (in the modern sense) it may be left on one side here.

So he solved his problem. He showed that the planets can be regarded as subject to Galileo's laws, and that the force on a planet must be directed towards the sun, and that on the moon towards the earth; and that these forces must vary in a certain simple way with the distance between planet and sun or moon and earth. The moon follows the course she does because there is a pull between it and the earth just as, when a stone is whirled round at the end of a string, there is a pull between the stone and the hand.

And now—as I like to think—he had ended his labour. He realized that he had made a stupendous discovery, which must revolutionize, as it actually has done, the whole science of

astronomy. He had shown that the laws of dynamics apply to planets as well as to ordinary bodies on the earth, that the planets were subject to forces, and had determined what those forces were. What more could he do? What explanation of his result could be offered or even demanded. He had ordered the solar system, and who could be so foolish as to ask why the order was that which he had found and no other? But after his morning's work which finally completed his forthcoming treatise on the matter, he sat in his orchard and was visited by some of his friends from Cambridge. Perhaps they too were natural philosophers and he talked to them about what he had been doing; but it is more likely that they sat idle, talking the thin-blooded intellectual scandal that must have always flourished in academic society, while Newton played with the historic kitten.

And then the apple fell from the tree. Newton was suddenly silent in a reverie; the kitten played unheeded with the fallen fruit; and his friends, used to such sudden moods, laughed and chattered. After a few moments he must have paged; he rushes to his desk in the house; scribbles a few hasty figures; and now the theory of gravitation is part of the structure of the universe. The falling apple, a trivial incident which he had seen a thousand times before, had loosed a spring in his mind, set unconsciously by all his previous thought. He had never consciously asked himself **why** the moon was pulled towards the earth until, in an instant of revelation, the apple appeared to him not "falling" (the meaningless word he had always used before), but *pulled towards the earth*. The idea flashed on him quicker than it could be spoken. If both the moon and the apple are pulled towards the earth, may they not be pulled by the same force? May not the force that makes the apple "fall" be that which restrains the moon in its orbit? A simple calculation will test the idea. He knows how far the moon is from the earth and how the force on it varies with the distance and with the size of the moon. If the moon were brought to the surface of the earth and reduced to the size of the apple would the force on it be such that it would fall with the speed of the apple? The answer is, Yes **1** The motion of the planets is therefore explained both by generalization and by familiarity. That motion is merely one particular instance of a general principle of which the very familiar fall of heavy objects is another.

What I want to impress on the reader is how purely personal was Newton's idea. His theory of universal gravitation, suggested to him by the trivial fall of an apple, was a product of his individual mind, just as much as the Fifth Symphony (said to have been suggested by another trivial incident, the knocking at a door) was a product of Beethoven's. The analogy seems to me exact. Beethoven's music did not exist before Beethoven wrote it, and Newton's theory did not exist before he thought of it. Neither resulted from a mere discovery of something that was already there; both were brought into being by the imaginative creation of a great artist. However there is one apparent difference; Beethoven having

**1** The reader who knows the story—and who does not?—will see that here I deviate widely from history. Newton did not know how far the moon was from the earth; current estimates were wrong; and at first he was therefore doubtful of his theory. But when the distance was measured more accurately, he found it agreed perfectly with his theory. I hesitate to suggest that it was Newton's theory that had changed the distance

I feel, too, that some people will think that I must be very antiquated in my knowledge if I glorify Newton so greatly when the daily Press has been assuring us lately that his ideas have been completely overthrown by Einstein. This is not the place to discuss what Einstein has proved. I admire his work as much as anyone, but he has not invalidated in the smallest degree the great discovery of Newton which is discussed in the text. It is still as certain as ever it was that the fall of the apple and the "fall" of the moon are merely two examples of the very same fundamental principle; and it is as certain as ever that the motions of the planets are subject to the same laws as those of terrestrial bodies. What is now not quite certain is whether Galileo's laws are strictly applicable to circumstances very different to those of the experiments by which he proved them.

conceived his symphony had no need to test it in order to discover if it was "right," while Newton had to compare the results of his theory with the external world, before he was sure of its true value. Does not this show that Newton's achievement was not so perfectly personal and

imaginative as Beethoven's?

I do not think so. First, Beethoven's work *had* to be tested; the test of artistic greatness is appeal to succeeding generations free from the circumstances in which the work was conceived; it is very nearly the test of universal agreement. But it is another point of view I want to emphasize here. It is said that Newton's theory was not known to be true before it was tested; but Newton knew that it was true—of that I am certain. To our lesser minds there seems no imperative reason why the force on the moon and the force on the apple should be related as closely as the theory of gravitation demands, merely because it would be so delightfully simple if they were; but Newton probably felt no doubt whatever on the matter. As soon as it had occurred to him that the fall of the apple and the fall of the moon might be the same thing, he was utterly sure that they were the same thing; so beautiful an idea *must* be true. To him the confirmation of numerical agreement added nothing to the certainty; he examined whether the facts agreed with the object of convincing others, not himself. And when the facts as he knew them did not agree, we may be sure that his faith in the theory was in no way shaken; he *knew* that the facts must be wrong, but he had to wait many years before evidence of their falsity was found which would appeal to those who had not his genius and could not be sure of harmony between their desires and the external world.

### ARE THEORIES REAL?

And here we come to our last question. I have been at pains to distinguish theories from laws, and to insist that theories are not laws. But if that contention is true, are not theories deprived of much of their value? Laws, it may be said, are statements about real things, about real substances (such as iron), about real objects (such as the earth or the planets or existing living beings). Laws are valuable because they tell us the properties of these real objects. But if theories are not laws, and if the statements they make are about things that cannot ever be the subject of laws, do they tell us about anything real? Are the molecules (by means of which we explain the properties of gases) or the countless generations of unknown animals and plants (by means of which we explain the connexions between known animals and plants) or the forces on the planets (by means of which we explain their orbit)—are these molecules and animals and forces mere products of our fantasy, or are they just as real as the gases and the animals the laws of which they are led to explain? Are theories merely explanatory, are they like the fairy tales by means of which our ancestors explained to themselves the world about them, are they like the tales we often tell to our children with the same object of explanation, or are they truly solid fact about the real things of the world?

That may seem a simple question to which a plain answer, Yes or No, might be given; but in truth it raises the most profound and abstruse problems of philosophy and really lies without the scope of this book. Our object is to discover what science is; we have learnt what laws and theories are, and what part they play in science; it is not directly part of our purpose to discuss what value all this elaboration has when it is achieved. But in a book of this kind it would be wrong to leave the question with no answer; and I will therefore explain how the matter appears to me, although I know that many other people would give different answers.

I should reply to the questioner by asking him what he means by "real" and why he is so sure that a piece of iron, or a dog, is a real object. And the answer that I should suggest to him is that he calls these things real because they are necessary to make the world intelligible to him; and that it is because they are necessary to make the world intelligible to him that he resents so strongly (as he will if he is a plain man) the suggestions that some philosophers have made that these things are not real. It is true that these suggestions are often not interpreted rightly, and that what the philosophers propose is not so absurd as appears at first sight; but the fact remains that these ideas are of supreme importance to him in making the world intelligible, and that he dislikes the notion that they are in any sense less valuable than other ideas which, for him at least, do not make the world so intelligible. The invariable associations which are implied by the use of the ideas "iron" and "dog" are extremely important in all his practical life; it is extremely important for him that a certain hardness and strength and density and so on are invariably associated in the manner which we assert when we say that there is iron, and that a certain form and sound and behaviour are

invariably associated in the way that we assert when we say that there are dogs. When the plain man says iron and dogs are real objects he means (I suggest) to assert that there are such invariable associations, that they are extremely important, and that they are rendered intelligible only by the assertion that there is iron and there are dogs.

If we accept this view it is clear that we must answer in the affirmative the question from which we started. Theories are also designed to make the world intelligible to us, and they play quite as important a part as do laws in rendering it intelligible. And if anything is real that renders the world intelligible, then surely the ideas of theories—molecules and extinct animals and all the rest of it—have just as much claim to reality as the ideas of laws.

But my questioner will almost certainly not be satisfied with that answer; it will seem to him to shirk just the question that he wants to raise. He will feel that the view that reality is merely what leads to intelligibility deprives reality of all its importance; if science is merely an attempt to render the world intelligible, in what does it differ from a fairy tale—which has often the same object? Or—to put the matter in a different way—intelligibility is a quality that depends on the person who understands; one person may find intelligible what another may not. Reality on the other hand is, by its very meaning, something independent of the person who thinks about it. When we say that a thing is real we do not mean that it is peculiarly suited to our understanding; we mean much more that it is something utterly independent of all understanding; something that would be the same if nobody ever thought about it at all or ever wanted to understand it.

I think the essence of this objection lies in the sentence:

“One person may find intelligible what another may not.” When we feel that science is deprived of all value by being likened to a fairy tale, our reason is that different people like different fairy tales, and that one fairy tale is as good as another. But what if there were only one possible fairy tale, only one which would explain the world, and if that one were intelligible and satisfactory to every one? For that is the position of science. There have been many fairy tales to explain the world; every myth and every religion is (in part at least) a fairy tale with that object. But the fairy tale which we call science differs from these in one all-important feature; it is the fairy tale which appeals to every one, and the fairy tale which nature has agreed to accept. It is not you and I and the man round the corner who find that the conception of iron makes the world intelligible, while the people in the next street do not; in this matter every single living being in the world (so far as we can ascertain his opinion) agrees with us; they all accept our fairy tale and agree that it makes the world intelligible. And nature accepts it too; the law that there is iron enables us to predict, and nature always agrees with our predictions. There is no other fairy tale like this; there is none that denies that there is iron, a substance with invariably associated properties, which is acceptable to every one and which predicts truly. It is just because our fairy tale is capable of the universal agreement which we discussed in Chapter II that we distinguish it from all other fairy tales and call it solid fact. Nevertheless the fact remains that its value for us is that of other fairy tales, namely that it makes the world intelligible.

And now let us turn again to theories. Here, it is true, we cannot apply directly the criterion of universal assent. There is actually much more difference of opinion concerning the value of theories than there is concerning the value of laws; and it is impossible to force an agreement as it can be forced in the case of laws. And while that difference of opinion persists we must freely admit that the theory has no more claim on our attention than any other; it is a fairy tale which may be true, but which is not known to be true. But in process of time the difference of opinion is always resolved; it vanishes ultimately because one of the alternative theories is found to predict true laws and the others are not. It is for this reason that prediction by theories is so fundamentally important; it enables us to distinguish between theories and to separate from among our fairy tales that one which nature is prepared to accept and can therefore be transferred from the realm of fantasy to that of solid fact. And when a theory has been so transferred, when it has gained universal acceptance because, alone of all possible alternatives, it will predict true laws, then, although it has purpose and value for us because it renders the world intelligible, it is so clearly distinguished from all other attempts to achieve the same purpose and to attain the same value that the ideas involved in it, like the ideas involved in laws, have the certainty and the universality that is characteristic of real objects. A molecule is as real, and real in the same way, as

the gases the laws of which it explains. It is an idea essential to the intelligibility of the world not to one mind, but to all; it is an idea which nature as well as mankind accepts. That, I maintain, is the test and the very meaning of reality.