CHAPTER IV

THE DISCOVERY OF LAWS

STATEMENT OF THE PROBLEM

FOR now, having decided what laws are and what they state, we have to ask how they are discovered. Laws state invariable associations; but how can we ever be sure that an association is invariable? We may have observed an association many times, and have always found that if one of the associated events or properties occurs, the other occurs also; but if the association is truly invariable, we must know, not only that the association always has been found in the past, but also that it always will be found in the future. Moreover, even if we have found the association every time in the past that we have looked for it, we clearly cannot know that it has occurred when we have not looked for it. The establishment that an association is invariable and the assertion by a law that it is invariable clearly require that we should be able to judge from the observation of one or several occurrences of it all the other occurrences that may happen or have happened. How can we possibly attain such knowledge?

One answer to this question is simply that we do not *know*. We can never be certain that an event will happen in the same way that we are certain that it has happened. Indeed, there is a difference in the sense of the word "know" applied to the two cases—a difference in sense which is reflected by the use of different words in most languages. When I have actually experienced an event I have a direct and immediate perception of it which is different in kind, and not merely in degree, from my belief, however confident, that it will happen; it is not merely that I have more knowledge of it, but that the knowledge is of a different kind. It is utterly impossible that I should have of the one event the kind of knowledge which I have of the other. If we are to discuss profitably the problem before us, we must remember this difference. We must not seek of events which have not happened, the kind of knowledge applicable only to those which have happened. And again, we must not seek the kind of knowledge—it is once more a different kind—that we have of purely logical or internal propositions. When I say that a black cat is black, I am quite certain that the statement is true because by "a black cats' I *mean* a cat that is black; to say that a black cat is not black is not untrue; it is meaningless. The knowledge that I have of the truth of the statement is necessarily different from that which I can have of the statement that there is such a thing as a black cat or that all cats are black; and the difference is once more in the kind of knowledge and arises from a difference in the kind of statement; it is not a difference in degree of certainty.

The problem would be expressed better if we merely compared our knowledge of various future events and asked why we are more certain that some will happen than that others happen and how we arrive at this superior knowledge, for then we are sure of comparing always knowledge of the same kind. Of some future events we are as certain as we can be in respect of knowledge of this kind; we arc as certain as we can be that the sun will rise to-morrow. It would be ridiculous to say that we are not certain because we do not feel towards that prediction the same mental attitude that we feel towards the assertion that the sun rose to-day or the assertion that to-day is not to-morrow. For, once more, the difference in mental attitude necessarily arises from the difference in the nature of the statements. All that we can ask relevantly is why we are as certain that the sun will rise to-morrow as we can be of any future event and why we are so much less certain that it will not rain to-morrow.

It is obvious that our certainty in one case and our uncertainty in the other are derived from our previous experience of the happening of similar events, and that the difference in knowledge is due to a difference in that previous experience. Of course this statement does not help us to solve our problem, for since laws are undoubtedly derived from previous experience, it is clear that it is there that the foundation and evidence for them must be found. But the form in which the problem has been put enables us to avoid altogether a question to which those who have discussed the matter have usually devoted most of their attention. They have asked how it is that previous experience gives any knowledge of future experience and what justification there can be for asserting in any case whatever that we have such knowledge. The point of view that I tried to suggest in the last few paragraphs is that this question is essentially unanswerable because it is based on the neglect of the fundamental distinction between different kinds of knowledge. Our "knowledge" of future events simply *is* something based on our knowledge of past events; when we say that we know something about the future we only mean that we have a mental

attitude based on past experience; and it is absurd to ask why it is based on past experience, for, if it were not so based, it would be something quite different. In my opinion (though the reader should be warned that others would dissent strongly) it can only lead to confusion of thought to attempt to compare this knowledge with other kinds of knowledge and to ask how they stand in relative certainty. And yet some comparison of knowledge of future events with other kinds of knowledge is always intended when it is asked how we have such knowledge from experience of the past.

And because such a question is meaningless the answers given to it are meaningless also. They always consist in some attempt to prove from very abstract and obscure premisses a doctrine called by the high-sounding title of Uniformity of Nature; it is argued that, for some reason to be found in transcendental philosophy, nature must be such that what is true of her in one part, in one region of space or at one period of time, must be true of her in any other part. But the value of such a doctrine depends entirely on the meaning attributed to "nature." If the world means merely the non-human, external world of common sense (as in Chapter II), then the doctrine is simply untrue. Nature, in this sense, is not uniform; there are events which happen once and never happen again; and it is precisely because there are such events that we distinguish between past and future. If it were really true that "history repeats itself," there would be no history; history is the record of events which have not repeated themselves and the proverb—like almost all proverbs—merely represents an attempt to obtain, by an epigrammatic form, credence for an assertion which nobody would otherwise believe. It is true that many events which do not so repeat themselves, and perhaps the most important of these events, are characteristically human and do not, therefore, form part of common-sense "nature"; but there are enough non-recurrent events, which have nothing to do with man, to distinguish between past and future and thus to controvert the assertion that all nature is uniform in all its parts.

If, on the other hand, we mean by "nature" in this connexion the carefully scrutinized nature of science, then the doctrine merely states that nature is nature. For this "nature" or external world of science is characterized by and distinguished from everything else by the fact that it is uniform ; for, as we have seen, it is made up of invariable associations concerning which universal agreement can be obtained. Any part of experience that is not uniform would not consist of invariable associations and would be at once excluded from this closely regulated nature. Indeed the problem before us is simply that of how we distinguish the uniform from the non-uniform parts of the nature of common sense, for that is our task in establishing the relations which are asserted by laws. To attempt to base a method of making the distinction on the assumption that all nature is uniform is simply to misunderstand the problem that is to be solved.

AN ATTEMPTED SOLUTION

After this clearing of the ground, we can attack the problem. What is the feature of our previous experience which makes us so certain that the "law" of the rising and the setting of the sun asserts a truly invariable association and, consequently, that the sun will rise tomorrow? In answer every one would say that our belief is certain because we have observed the association an immense number of times without observing any failure. And doubtless this is the reason in this particular case, but other instances suggest that the answer is not fundamental or complete. For there are instances in which an association which has been found as invariable has at length been broken; and there are instances in which a law is asserted confidently as the result of a single observation, so that there has been no chance of proving any invariability. The instance of the first kind that is always quoted in these discussions is that of the black swan. Until Australia was discovered, swans had been found invariably to be white in a very large number of observations, and natural historians would have been justified in asserting, according to the principle suggested, the law that all swans are white; and yet the law was false, for some swans in Australia are black. Instances of the second kind are plentiful in actual science. When a chemist makes a new compound, he often determines its melting-point or density; as a result of a single measurement he will often be prepared to assert that its melting-point or density is higher (say) than that of water, and nobody will dream of doubting that the association he asserts is invariable or that subsequent measurements will lead to the same result.

These examples seem to prove that a large number of favourable instances, even if without exceptions, is neither sufficient nor necessary to establish a law. But at the same time they suggest what is the

additional element required. We have omitted to take into consideration other laws closely similar to those that are under discussion. The chemist is certain that, in measuring the melting-point of a new compound, he is establishing an invariable relation, because from the examination of a great number of other compounds he has found that the density is an invariable property. On the other hand seventeenth-century naturalists ought to have regarded with suspicion a law that all swans are white (and probably they did actually so regard it) because the examination of other animals would have shown them that colour is by no means an invariable property, but is liable to vary very widely even among closely related species. In putting the matter as we did, the full evidence was not disclosed. The evidence for the invariable density of a new compound is not the single measurement of it, but the general law that all densities are invariable properties. This law is established by the observation, not of a single instance or of one or two, but of a very large number of instances, in none of which the relation has been found to fail. The evidence for the assertion of the law of the density of the new substance is really of exactly the same nature as that for the rising of the sun to-morrow.

This mode of expressing the matter is probably not quite correct; for closer examination would show that it is difficult to regard the assertion that density (unlike colour) is an invariable property as a true law. It would be better to say that there are certain associations (such as that of density or melting-point with the other properties of a substance) which, if they occur at all, we expect to be invariable. In other words, we expect to find laws of certain forms, and if we find an observation which might be a particular instance of a law of one of these forms, we are much more ready to jump at once to the conclusion that this law is indeed true than we should be if the law, of which the observation would be a particular instance, is not of one of these forms. And one of the reasons why we expect such laws is that we have previously found a large number of them; however, as we shall see presently, this is not the only reason.

THE ELASTICITY OF LAWS

But our answer is not complete yet. If this were all, I think we should feel far more uncertainty than we actually do feel about most of our laws. However many favourable instances we had observed, if we felt that a single unfavourable instance, if it occurred, would destroy the law, we should never be free from uneasiness. The contrary instance might occur; we might go to our laboratory one morning and find that the density of some substance which we had measured the day before was now quite different. Our confidence in the law is largely based on the fact that such an unfortunate incident would not necessarily destroy our belief in the law.

This statement may be surprising. Surely if a law states that some relation is invariable; and if, as we professed in Chapter III, we are going to be really strict in our interpretation of invariability, then a single contrary instance must destroy the law. For an association which has failed once, even if it has not failed a million times, is not strictly invariable. True; but what exactly is the association we are asserting? We are asserting that a certain density is invariably associated with a certain substance. If we find a new density we cannot maintain the invariable association if we attribute it to the same substance as that to which the old density was attributed. But why should we not attribute it to a new substance? If we try the experiment over again and find that we do not get the same result as before, what is to prevent us avoiding any discrepancy between the *two* experiments by simply saying that they are not made on the same substance?

Indeed this way out of the difficulty has been adopted implicitly in the case of the black swan. Since we have known of black swans, we do not say that there are not white swans; we recognize two kinds of swans, one of which is black and the other white. Nor do we recognize any error in the assertion, by those who did not know of black swans, that all swans are white. All the swans that they knew anything about were white and have always remained white. The apparent difficulty arose only because the new birds were called swans. If we confine that term to the birds which were originally called swans, any law about swans is quite unaffected by the discovery of birds which resemble swans in some respects, but which, since they are not wholly the same, should not be called swans.

But, it may be urged, the case is not really parallel to that which we must suppose if we want to face the difficulty fairly. Black swans differ from white in other things than their colour, so that there is a reason quite apart from their unexpected colour for distinguishing them from white swans. Again, even after the discovery of black swans, white swans could still be found. But

suppose—we will return now to the instance of density— that when we re-determined the density and found it changed, we could not detect any change in any other property of the substance and that we could not find a substance which, resembling this substance in all other respects, had the density found in the first experiment, could we then maintain the invariability of the association? Well, it would doubtless be very awkward and men of science would get into a fever of excitement, but they *could* maintain their law. For the supposition that *nothing* had changed between the two experiments is impossible to realize; the mere fact that a previous experiment had been made and that the second experiment had been made after the first is sufficient to make some change between the two. Of course our usual conception of a substance excludes the idea that such changes—a mere repetition of a measurement or the mere lapse of time—could change its properties and make it a new substance; we should have to alter our conception of a substance. But that conception has been already altered so greatly since it was taken over from common sense that there would be no impossibility and no insuperable inconsistency in maintaining that, since we made the first experiment, the substance on which we made it had vanished from our ken and been replaced by some other substance, which might naturally enough have a different density.

Indeed we should have to maintain something of the kind, for, whatever we might do, the fact would remain that we have observed two densities which cannot be those of the same substance and cannot be asserted by the same law. Either we must include the two observations under different laws, or we must leave one (or both) of them outside laws altogether. We adopt this last alternative if we regard the first measurement simply as a mistake; a mistake is something that is excluded necessarily from the subject-matter of science and to which, therefore, a law can have no reference. It is quite possible that, if such a case as we are imagining actually occurred, we should adopt this course; but it must be remembered that we might adopt the other and remove the discrepancy, not by rejecting the observation, but by stating two laws. Which alternative we shall adopt depends on all the circumstances, and here it is convenient to note why the observation of a very large number of favourable instances is important in the establishment of a law. If we have based a law on a large number of instances, and subsequently find other instances apparently discrepant, then, if, when we choose between the alternatives just mentioned, we reject the law, we place all these large number of observations outside the province of science. And this we are loath to do; we want to reduce as much as possible of our experience to order by means of laws, and the rejection of the whole of our past experience as one great "mistake," accords ill with that purpose. When we have ordered a very large number of instances by means of a law, we shall want to maintain that law at all hazards; and we shall be much more willing to introduce other laws to include instances apparently discrepant, and so to avoid the necessity of rejecting the material on which the original law was based, than we should be if we have only ordered a very small number of instances.

THE PURPOSE OF LAWS

It will be seen that in this discussion the question from which it started has almost been left out of account. We asked how we managed to establish laws, by the examination of our past experience, which were true also for future experience. The considerations that have been put forward suggest that this problem is not answered, but is hardly contemplated at all, in the actual discovery of laws. When we are seeking laws, we are only thinking about the experience that we have actually had; and the problem which we seek to solve is one that has reference only to that experience. We seek to order the experience, to change it from a miscellaneous collection of apparently unconnected observations to a connected series of particular instances of a few wide principles. These principles by means of which, and in terms of which, we order our past experience are laws; they state, as has been said so often before, associations between events and properties which have proved in our past experience to be invariable. It is because the associations have proved invariable throughout this experience that by means of them we can order the experience as many particular instances of a few principles. When our experience is increased by the addition of observations which *were* future but are now past, we seek once more to order in the same manner our increased volume of experience; but in this increased volume all experience is of equal value, that which was future is in no way different from that which was past, for all is now past. It may happen that the order established for the original experience is equally valid for that which we now have; the

portion that is added can again be regarded as particular instances of the laws which were established as a result of the original experience. And if that happens, we have no reason to change our laws. But if that does not happen, if the laws established for the original experience do not prove valid when the volume of it is increased, then we have two alternatives. We may either reject altogether the added experience and say that it is not proper subject matter for science, or we may alter slightly (or radically) our laws, so that they now order satisfactory both the $_{old}$ and the new. If we adopt the second alternative, the new laws propounded must still be such that they order the old experience, and they must therefore present some features of great similarity to the old laws. Which of the two alternatives we shall adopt depends upon which method leads to the most satisfactory ordering of the complete experience. For this reason the first alternative is never adopted if the second is available; for it means that we must leave unordered a portion of experience which we thought could be ordered.

This is, I believe, the attitude that is actually adopted by men of science in establishing laws. And if that is so, the conception of prediction does not enter into explicit consideration at all. We do not try to find laws that will predict; we only try to find laws that will order the experience that we have. It is possible to adopt that attitude because, although we know that we shall have future experience which has not been taken into consideration, that future experience can never force us to abandon the ground we have gained and to "disorder" the order that has been established. Whatever the experience may be, it will be possible either to order the increased volume of experience, or else to reject altogether from the subject-matter of science some portion of it, leaving only the remainder to be ordered.

THE VALUE OF LAWS

But to the practical man that attitude will not seem very satisfactory. It appears to deprive science of all objective value. If scientific laws are true, only because they can always be re-interpreted -so that nothing can prove them false, then science is merely a childish game unworthy of the attention of any serious man. If, when science asserts that the sun will rise to-morrow, it only means that, if the sun does not rise, we propose to alter somewhat our laws of the solar system, science is mere trifling. What the plain man means by that assertion is that the sun *will* rise and that the expectation of its rising is a sound basis for the conduct of life; he does not mean something that can be made true or false just as we please. It was all very well—I can hear an objector say—to insist at the beginning of the chapter that we can have no" knowledge "of future events; it is undeniable that we have some kind of knowledge which we habitually use in our practical life; and if the only kind of knowledge that science admits is a determination never to be proved wrong, then we must seek elsewhere for the information that undoubtedly is to be had.

Of course, I do not deny all this; and now I shall try and show how the two points of view may be reconciled. Men of science, though they pay no direct attention to prediction, are not really indifferent to the success of their predictions, interpreted in accordance with the plainest common sense. If their predictions always failed, it would mean that each addition to experience would mean a new ordering of the whole. This ordering doubtless could be accomplished in some fashion, but it would have no value. The achievement of science would be like that of Penelope, who wove a cloth that she unravelled each night and started afresh each morning. If all our predictions were failures, we could, I suppose, continue our task of ordering experience, but no sane man would do so. Science is only worth while because it does make real progress. The ordering established for past experience is on the whole valid for future experience. The exceptions are comparatively few, and, even when they occur, it is found that the alteration of the order is so slight—it is often only a natural development of the old order—that the necessity for repeating the task is not wearisome. Time unravels, not the whole web, but only a few minor portions in which the shuttle has gone awry. Scientific laws do predict exactly in the manner which the plain man desires; and it is really as necessary for the purposes of science that they should do so as for the purposes of practical life.

THE FUNDAMENTAL QUESTION

But why do they predict? We return once again to the question which we cannot avoid. The final answer that I must give is that I do not know, that nobody knows, and that probably nobody ever will

know. The position is simply this. We examine our past experience, and order it in a way that appears to us most simple and satisfactory; we arrange it in a manner that is dictated by nothing but our desire that the world may be intelligible. And yet we find that, in general, we do not have to alter the arrangement when new experience has to be included. We arrange matters to our liking, and nature is so kind as to recognize our arrangement, and to conform to it! If anyone asks, Why, what kind of answer can we possibly give; how can we explain why the universe conforms to our intellectual desires?

Here we inevitably touch upon profound problems, which lie far beyond the scope of this little book. I can only say that, for myself, none of the answers that have been offered seem satisfactory explanations, or even explanations at all; they raise more questions and more difficult questions than they answer. But it may be well to draw attention to two considerations that have to be taken into account in any discussion of the matter. The first has been mentioned several times before. It must always be remembered that science does not attempt to order all our experience; some part of it, and the part perhaps that is of most importance to us as active and moral human beings, is omitted altogether from the order. And it is very hard to say whether we omit it because we know that we cannot order it in the same manner as that which forms the proper subject-matter of science, or because we feel instinctively that, even if we could force it into such an order, that order would not be appropriate to it. I incline to the second alternative; it seems to me that there is something so fundamentally different in the internal and external worlds (of Chapter II) that we would not, even if we could, group them in the same categories. But whichever alternative we adopt, it remains equally difficult to explain why even the limited part of experience which science takes as its province conforms so closely to our desires, or why there should be a part which can be selected so that it conforms.

The other consideration arises when it is asked who are the "we'" to whose intellectual desires nature conforms. It is a grave difficulty, inherent in all the many attempts to lay down rules whereby science may discover laws valid for future experience, that they would indicate that anybody who knew the rules could discover laws. But that is not the fact; it is not every one who has that power. Indeed the fact seems to be precisely contrary. Those who have professed the most intimate knowledge of the rules, the great philosophers of science, such as Bacon or Mill, have never been able to apply their rules to the discovery of any law of the slightest value. Laws have been discovered for the most part by people naively innocent of all philosophical subtleties. The great man of science, like the great poet or the great artist, is born and not made; like the artist he must train his faculties, but training alone will not confer them. The vast majority of mankind (a majority which includes a great many of those who have done useful scientific work) cannot discover laws, except in so far as they are helped, in a way we shall notice immediately, by the previous work of an infinitesimal minority. Either they cannot see order in experience at all, or the order which they think they see does not prove to be that to which nature is prepared to conform; they do not discover laws, or the laws that they discover predict falsely. It is only the great leaders of science who see the right order. They and they only, can establish an order which satisfies their intellectual desires and yet find that it is valid for the future as well as for the past. They and they only, are in such harmony with the universe that it obeys the dictates of their minds.

THE SIGNIFICANCE OF GENIUS

I fear this point of view will seem to some readers too mystical for their tastes. Nevertheless I would press it strongly on their attention. Of course I do not claim in the least that it explains why laws, devised even by the greatest of men, do predict, but it is necessary for the understanding of science, as much as for the understanding of art, to recognize that there are great men who surpass their fellows in some scarcely comprehensible manner. Science would not be what it is if there had not been a Galileo, a Newton or a Lavoisier, any more than music would be what it is if Bach, Beethoven and Wagner had never lived. The world as we know it is the product of its geniuses—and there may be evil as well as beneficent genius—and to deny that fact, is to stultify all history, whether it be that of the intellectual or the economic world.

But in one, as in the other, genius itself is too rare and too short-lived to achieve much by its unaided efforts. Great men—and this is particularly true of the greatest— achieve more by their influence than by their direct action. They change the world by enabling others to complete what they have themselves

begun. And in no direction is this more true than in science. By far the greater part of scientific work has been done, and by far the greater number of laws discovered, by those of us who have not the remotest claims to genius or any but the very pedestrian talents of energy and application. But we are simply following in the footsteps of our masters. In Chapter III we noticed that there were standard forms of laws; there are many laws, all quite distinct and ordering quite different groups of fact, which are yet obviously all of the same form. The laws asserting the properties of a substance provide a notable example; there are many substances, but the laws which assert that there are such substances have all the same form. The properties of hydrogen, which are asserted by the law that there is such a thing as hydrogen, are quite different from the properties of iron, asserted by the law that there is such a thing as iron; yet the laws are of the same form. Now once we have got the idea that there are laws of this form, it is a comparatively simple problem, which can be solved more or less according to fixed rules, to establish the laws of a new substance or, by finding new properties, to alter or augment an old one. And we do know now that laws of this particular form are among those to which nature will conform, and which can be usefully applied in prediction. The stroke of genius was that of the man who first suggested a law of that form; once he had suggested it and showed that such a law is permanently valid, it was easy enough for others to take up the work and find others of the same form.

The discovery of the laws of substances is hidden in the darkness of the past; they are among the ideas which we take over from common sense, and were invented by the unknown giants who laid the basis of human knowledge. But advances quite as important, the discovery of other forms of laws which have been used by the humbler folk who do the spade-work of science, such advances have occurred in historic times. Certain great men are recognized as the founders of certain branches of science, and if we inquire why they are so regarded, we shall usually find (but another reason will be found in the next chapter) that they were the first to establish a law of the form that is specially characteristic of that science. Thus of physics, numerical laws (which we shall discuss later) are especially characteristic; Galileo was the first to establish a numerical law of the type of which almost all modern physics consists; nine-tenths of the work of later physicists in the discovery of laws has been simply the extension of laws of Galileo's form to other fields of experience. Galileo may fairly be hailed as the founder of experimental physics. Other great men have so changed or amplified the form, that their work ranks as independent—Boyle, and Ampere may claim place in this class; but again their fame rests largely on the discovery of a new type of law which has been simply applied elsewhere by lesser men. Of other sciences I am not competent to speak, but if Lavoisier is the founder of modern chemistry it is because he first established a law of the form that asserts chemical combination; and if Linnaeus is the founder of systematic botany, it is because he first established a law of the form that asserts the existence of a particular species of plant.

This then is really the solution of the main question of this chapter, as it faces the practising student of science. He believes that if he can discover a law of a certain form and order his experience in a certain way then that law will predict and nature will conform to that order. And so far, at least since the seventeenth century, his expectation has never been falsified; I believe that in the history of modern science there is no instance of the abandonment of a type of law which has once been firmly established. Progress has been continuous; it has consisted in the establishment of many laws of old types, and very occasionally, in the introduction of new types. Even when at first sight experience has contradicted expectation, it has always been possible (as in the example of the black swan) to remove the contradiction by resolving a law of one of these types into several laws of the same type, or by changing it to a law of another known type.

And what are these types? To answer that question would be to expound all science; I want only to encourage the reader to study science for himself and to find the types. But I have already indicated some of the more important types, such as the law of a substance, the law of a particular kind of animal and the numerical law; and it has been urged that all these laws have the important common feature that the things between which they assert invariable associations are themselves interconnected by other laws. Those who have previously read in the philosophy of science will be surprised that the causal law is omitted, but the reasons of the omission were given in the previous chapter. In physics, at least, it is not an important type, though it possibly may be in other sciences, such as meteorology and medicine. And by omitting the causal laws, we can omit also all reference to the "Canons of Induction" which were supposed by an earlier generation to provide the one and only means for discovering scientific laws. They are futile,

because the problem which they profess to solve is not one which has ever troubled any intelligent person. They tell us how, when we know that an event is the cause or effect of another, we may discover of which other event it is the cause or the effect; as a matter of fact, the crudest common sense, applied in everyday life, serves for the purpose. We might similarly draw up canons for discovering of what substance a given property is a property, when it is once known that it is a property of some substance; but here again the rules would be so obvious as not to be worth formulating. The problem of science is not to discover examples of laws when once we know what kind of law to look for; it is to know for what kind of law to look. And that problem, as we have insisted before, is insoluble except by the genius which knows no rule.