III

EXPLAINING AND PREDICTING

1. *Covering Law as a Sufficient Condition*

IN the preceding chapter I have tried to show that although there may be a grain of truth in the claim that being able to indicate a covering law is a *necessary* condition of giving an explanation, the claim as it is usually made is both logically artificial and methodologically misleading. It obscures distinctions of logical and methodological interest by failing to recognize the extent to which words like 'use', 'function', 'implicit', 'requires', 'law', &c., which commonly appear in its formulation, are susceptible of further analysis, and it fails to take account of the legitimate sense in which historians explain conditions and events which are unique. In the present chapter I want to examine in a similar way the claim that citing a covering law, together with statements setting forth antecedent conditions, is a *sufficient* condition of giving an explanation; and once again, I shall urge that although there is an element of truth in it, this claim, too, is artificial and misleading. In the course of the investigation I shall try to make some progress toward a more satisfactory general account of the logic of 'explanation', by comparing what is offered as explanation in history with what is offered in some other fields.

In asking the question we now have to consider, we approach the problem of what it is to give an explanation from an altogether different direction. The question is no longer whether, in some interesting sense, we *must* have a law, but rather, supposing that we have an appropriate empirical law, whether we then *ipso facto* have the materials for giving an explanation. For in spite of historians' interest in the unique, and although in some cases there is no covering law to which one could sensibly be said to appeal, it would be rash to deny that routines are *ever* recognized in history. Indeed, historians

sometimes explicitly point out that the events they study fall under some law or laws. Even Fisher, in spite of his notorious belief that history is 'one great fact', with respect to which there can be no generalization, when explaining the course of Roman expansion, allows himself to remark: "an orderly power ringed about by turbulence always finds itself compelled to establish peace and security upon its frontiers."[1] What I now wish to question is the view that to point to such a generalization is necessarily to explain what falls under it as an instance. I shall argue that something more than this is required, and that to fail to recognize this is to misconceive the logic of 'explanation'.

In putting the problem in this way, I shall be pursuing the kind of inquiry sketched at the beginning. Our question is: 'What are the conditions which have to be met in order to give an explanation in history?' Covering law theorists seem to, and are commonly taken to, say that there is one and only one condition: subsumption under an independently validated general law. Or, as Popper and Hempel both put it: the logical structure of *explanation* is equivalent to that of *prediction* and *verification*, one logical model serving to elucidate what we mean by all three. In the preceding chapter, in the interests of an orderly consideration of various grounds for dissatisfaction with the covering law model, I did not question the view that explanation is just 'prediction upside down'; my argument was rather that neither explanation nor prediction need be law-covered in historical cases. I now want to argue, however, that there is a logical dissimilarity between explanation and prediction of the greatest importance, and that to regard them as strictly correlative operations is to depart from the ordinary meaning of the term 'explanation', which is also its meaning in history. I shall argue that, because of this dissimilarity, it would be incorrect to say that if a person knows that a certain event occurred, and he has information from which it might justifiably have been predicted, then he has all that is needed to explain the event in question.

[1] Quoted by S. Hook, in *The Hero in History*, London, 1945, p. 144.

It is possible that, upon reflection, some of those who support the model's claims would not accept quite such a strict interpretation of what they say. Thus, although Hempel declares that an explanation "is not complete unless it might as well have functioned as a prediction", he does not actually say in so many words that this is the *only* requirement to be met.[1] But, on the other hand, he says nothing to suggest that anything more *is* required—various other criteria being mentioned only to be discarded as the marks of 'pseudo' explanation. And Gardiner, in the course of his exposition of Hume's regularity analysis of causation, tells us that 'an event is explained when it is brought under a generalization or law. It becomes an instance of a general rule. . . .'[2] This account, he adds, is "substantially correct"; and the only qualifications he actually makes are the ones outlined in Chapter I: that in historical contexts we shall find dispositional explanation falling outside this analysis, and that the generalization in other cases will be quite vague because of its loose component terms, its wide *ceteris paribus* clause, &c. I can find nothing in either Gardiner's or Hempel's formulations which would indicate disagreement with the more precise statement of another covering law theorist that "the logical core of explanation is provided by extensional connections or 'subsumptions'. . . . Where we have this pattern, nothing else is needed for explanation; where we lack it, nothing else suffices."[3]

In '"If", "So" and "Because"', Ryle puts the same general point in another way. In Ryle's terminology both arguments and explanations are 'applications' of a corresponding hypothetical or inference license. An argument of the form '*p* so *q*' requires the covering hypothetical 'if *p* then *q*', in the sense that it is only valid if 'if *p* then *q*' is true. Similarly, argues Ryle, '*q* because *p*' also requires 'if *p* then *q*', and is an application of it, although in a different way.[4] The latter phrase may seem promising to those who feel uneasy about the covering

[1] Op. cit., p. 462.
[2] Op. cit., p. 1.
[3] D. C. Williams, 'Some Remarks on Causation and Compulsion', *Journal of Philosophy*, 1953, p. 123. See also the passage quoted from Hook in Chap. I, section 2.
[4] p. 331.

law claim; but the only difference actually brought out is the fact that although '*p*' and 'if *p* then *q*' are all that is required to justify the argument '*p* so *q*', it is not the case that '*q*' and 'if *p* then *q*' are all that is required to justify the explanation '*q* because *p*'—for we also need independent knowledge of '*p*'. This difference is, of course, a genuine one. But I shall argue in this chapter that even so, we might still deny that '*p*' and 'if *p* then *q*' give an explanation of '*q*'.

2. *Generalizations and Explanatory Theories*

If covering law theorists were right in claiming that their model formulates a sufficient condition of explaining something, then reference to a covering law ought *always* to explain what falls under its apodosis clause. But it is surely not difficult to think of cases from everyday affairs which furnish evidence to the contrary. When puzzled by something, we do not ordinarily find it enlightening to be told: 'That's what always happens.' Indeed, although such a remark appears to be just an idiomatic, incomplete way of subsuming what happened under a general law, we should often feel justified in protesting: 'That's no explanation at all.'

What, for instance, is the explanatory force of the common-sense generalization, 'Red sky in the morning is followed by rain'? Does the fact that the sky was red this morning *explain* the fact that rain fell before lunch? Surely not. Translated into Ryle's symbolism, what happens in such cases is that although '*p*' and '*q*' and 'if *p* then *q*' are all true, still '*p*' does not explain '*q*'—which on his account is impossible. The hypothetical licenses the corresponding argument, but it does not license the explanation. Having a good reason for expecting something is not necessarily being able to explain why it occurs. This fact may easily be obscured by a purely formal analysis of the relation between different types of sentence. For it is necessary to distinguish 'because' sentences which only represent '*p*' as a *reliable inductive sign* of '*q*' from those which represent it as *the explanation* of '*q*'. Suppose someone says: 'It will rain before lunch because the sky was red this

morning.' Since Ryle's symbolism is tense-neutral, it would be quite proper to render this as '*q* because *p*'. In such a use the relation between the 'because' sentence and the covering hypothetical is exactly what Ryle says it is: nothing more than the hypothetical is required to license the 'because'. But this is because the latter is now just the argument itself in a different form. This conclusion can be supported by noticing the circumstances under which it would be possible to say '*q* because *p*' with the values indicated by the present example. We should say this only when we do not know independently that '*q*' is true, and this is exactly the condition under which we argue '*p* so *q*'.

The failure of at any rate some general laws to explain particular cases falling under them is even more obvious if we happen to select a classificatory generalization—for instance, that favourite of the formal logicians, 'All swans are white'. Such a general 'law' might, perhaps, at some stage of its career, achieve the status of an analytic statement. But as long as we did not make 'whiteness' a defining characteristic of swans, and the 'law' thus remained a true generalization, reference to it could scarcely be represented as explaining the fact that any particular swan was white. The most it could conceivably do in an explanatory way is explain why an investigator might say that a swan, as yet unobserved, would be white; that is, it would merely provide his justification for predicting its colour. Why then should a medieval historian who has discovered, say, that Sir Brian Tuke was bow-legged, be expected to regard as explanatory the assurance of a more experienced colleague that all medieval knights were?

Merely knowing that a red morning sky is always followed by rain would not explain today's downpour. Merely knowing that all medieval knights were bow-legged would not explain Sir Brian's bandy knees. In the face of such difficulties, some covering law theorists appear, at times, to be prepared to modify the sufficient condition claim by drawing a distinction between different *kinds* of laws. A distinction of this sort is often made, for instance, between mere empirical generalizations and the laws of the theoretical sciences. Thus Professor S. Toulmin, in *The Philosophy of Science*, represents empirical generalizations, arrived at inductively by the observation of similar cases, as proper only in that branch of science called 'natural history'—a descriptive rather than explanatory study.[1] For genuine explanations we are referred to branches of science using higher level theoretical laws, which cannot be discovered by simple induction at all—such laws as 'Light travels in straight lines', or 'Gases have a molecular structure'. That light travels in straight lines (at least partly) explains the fact that the shadow thrown by a 10-foot wall at sun's elevation 45° is 10 feet. That gases have a molecular structure (at least partly) explains why a balloon of air expands when heated.

Our concern here is, of course, with explanation in history rather than what Popper calls the 'pure generalizing sciences'. But I think it will be worth our while to look briefly at this distinction which philosophers of science sometimes draw between different kinds of laws. For, although no full analysis can be attempted, there are certain logical differences between the two which seem to me suggestive for a general account of explanation which departs from the covering law model.

What is it about a theoretical law which gives it explanatory force? The most common answer is that the laws of a theoretical science do not, so to speak, stand on their own; they are components of explanatory theories, often of very great scope. Individual laws like the ones mentioned above get their full meaning only in terms of the theories from which they are derived—in the case of our examples, the geometrical theory of optics and the molecular theory of gases. Theory and law are set up together; they are made for each other. Indeed, it is scarcely an exaggeration to say that when a law of this sort is called upon to explain a case falling under it, it is the whole theory which is brought to bear. The theory is implicitly called upon in the sense that only if we know the theoretical background will reference to the law itself explain. As we often

[1] London, 1953, especially chap. iii.

put it, the phenomena are explained *in terms of* the theory (a phrase which, I shall argue later, is suggestive of the explanatory force of theories).

But when the question is pushed a stage farther, and we ask why such indirect reference to whole theories is explanatory while reference to a mere covering generalization is not, we generally find covering law theorists, and even some who would not count themselves as such, reverting to the essentials of the position which we have just questioned, namely that an explanation is satisfactory in so far as the data contained in it could have functioned as a prediction mechanism. Thus we find Toulmin calling theories 'inference techniques', as if it were this characteristic which distinguished them from empirical generalizations arrived at by simple inductive inquiry.[1] (Whether he would say they are *mere* inference techniques, I am not sure.) But an inference technique, or license, could be derived from a generalization *or* a theory; what we have to discover is in what respects the latter differs in explanatory force.

The answer which emphasizes the inference-licensing role of the theory shows that, in spite of the promising distinction made between the two types of general statement, the covering law thesis remains intact. For we are told that the difference between the two is one of scope, generality or power—and it is *predictive* power which is meant. To use Rylian language, theories are many-sided in their applicability; they consist of hypotheticals which are highly determinable, not determinate; open, rather than highly specified. 'All swans are white' applies only to swans and only to them in respect of their whiteness. 'Light travels in straight lines' applies to shadow lengths, telescopic sightings, eclipses, mirror images, and a host of other phenomena. It applies not only to many different cases, but to many different kinds of cases. It is in line with this account that Toulmin sometimes says, not that covering generalizations do not explain their cases at all, but that they afford only 'shallow explanations'. 'This rolls downhill because

[1] Op. cit., p. 28.

it is a stone, and stones generally do roll downhill' is an explanation which takes us no farther than 'childhood dynamics'.[1]

The notion that the explanatory force of generalizations and of theoretical statements differs only in degree is a popular one. Thus Professor H. Feigl, having characterized explanations as "primarily a procedure of inference (just like the closely related prediction)", and having noted the complex structure of scientific theories, observes: "No wonder that the 'Aha-experience' is much stronger for these deductions from theories than from the more simple deductions from empirical laws."[2] By comparison with the latter, theoretical explanations are "high-grade". Gardiner, too, notes that it is 'the systematic character' of a science which allows it to give explanations with precision and force. Of common-sense explanations using empirical generalizations, he remarks: "They do not make a close structural analysis of the phenomena they roughly link together: they are content to notice a certain simple compresence or succession in experience, and that is all. In consequence, the explanations which they provide are of a vague and frequently unreliable kind, admitting of a multitude of exceptions."[3] Gardiner's lengthy discussion of the role of 'scientific theory and conceptual systems' issues in the conclusion that, in spite of there being differences between highly theoretical explanations and appeals to covering generalizations, the differences are not important for an account of the logic of explanation; for the difference is mainly a matter of the degree of confidence we have in each.

Is there nothing further to be said about the explanatory force of scientific theories? If we accept the account so far given, we are left with a mystery to explain. For to say that a theoretical explanation differs from subsumption under an empirical generalization only in the superior predictive reliability of the theory fails even to suggest why reference to at

[1] Op. cit., p. 50.

[2] 'Some Remarks on the Meaning of Scientific Explanation', reprinted in Feigl and Sellars, *Readings in Philosophical Analysis*, p. 512.

[3] Op. cit., p. 16.

least *some* generalizations provides no explanation at all. And it has the additional disadvantage, it seems to me, of condemning the explanations historians ordinarily give as 'low grade'. For there are few historical events which we can hope to explain in terms of theories borrowed from the special sciences, and there is no such thing as a general theory of history—in the sense of 'theory' employed in the formal sciences.

Some philosophers, it is true, look forward to the day when such a general theory will be constructed; and sociologists often seem to aim at repairing the deficiency. Other philosophers and methodologists of history regard the hope of constructing such a theory as illusory in view of the historian's concern with a concrete and miscellaneous subject matter. But whether such an enterprise has any likelihood of success or not, it would surely be unplausible to maintain that the giving of a genuine, or even of a 'high-grade', explanation in history must await the theory's formulation. For historians seem *already* to be able to explain at least some events to their own satisfaction. Perhaps typical explanation in history is not a lower grade version of the scientific kind, but something with logical peculiarities of its own. Perhaps what Feigl calls the 'Aha-experience' does not just arise out of our recognizing the predictive possibilities of the set of theoretical statements sometimes brought into the explanation of a particular state of affairs.

3. *The Model of the Continuous Series*

Let me attempt to strengthen the case for such a conclusion by discussing in some detail a concrete example from everyday affairs: an example which, in an important way, will be found to lie on middle ground between typical explanations in science, given in terms of a covering theory, and equally typical explanations in history, where no such theories are used. The fact that it is drawn from the sphere of mechanics will make it an especially useful choice for my present purpose, since that will, for the moment, cut out certain complications which are introduced when we consider explanations of

intelligent human actions, complications with which I wish to deal independently in Chapter V.

Suppose that the engine of my motor-car seizes up, and, after inspecting it, the garage mechanic says to me: 'It's due to a leak in the oil reservoir.' Is this an explanation of the seizure? I should like to argue that it depends on who says it and to whom—or, to put the matter in more formal terms, it depends on what else is presupposed, or contextually supplied. To the assistant mechanic standing near by, who knows all about internal combustion engines, it may very well be an explanation. To me, who am quite ignorant of what goes on under the bonnet, it is no explanation at all.

Let us try to put more precisely the difference between what I have to go on and what the mechanic knows. For I need not be so uninformed as not to know what is being referred to by the term 'oil reservoir'. Nor need the mechanic know 'all about auto engines' in order to transform 'There's a leak in the oil reservoir' from a mere statement of fact into an explanation. Would it have been enough, then, if he had had just enough experience of motor-cars to know that whenever oil reservoirs have leaks, the engine sooner or later seizes up? This would accord very well with the covering law theory if it were true; but surely it is not. I could have arrived at such a generalization by the most careful inductive procedure, and I might have absolute and justifiable faith in it. There may never have been a contrary case in the records of this garage, or of any other one I examine: whenever reservoirs were leaky, engines may have seized up. But this would make me none the wiser as to why an oil leak should have led to the seizure; it does not warrant my claiming that I know 'the explanation'.

If I am to understand the seizure, I shall need to be told something about the functioning of an auto engine, and the essential role in it of the lubricating system. I shall have to be capable of a certain amount of elementary trouble tracing. I need to be told, for instance, that what makes the engine go is the movement of the piston in the cylinder; that if no oil

arrives the piston will not move because the walls are dry; that the oil is normally brought to the cylinder by a certain pipe from the pump, and ultimately from the reservoir; that the leak, being on the underside of the reservoir, allowed the oil to run out, and that no oil therefore reached the cylinder in this case. I now know the explanation of the engine stoppage. What is there in this account that covering law theory leaves out?

It seems to me that my understanding of the engine seizure is very directly related to the fact that I can now *trace the course of events by which it came about*. The mechanism has been revealed: the oil ran out the hole; nothing came into the cylinder to lubricate the piston; the movement of the dry piston against the walls of the cylinder made them hot; the hot metals expanded and locked tightly. *Of course* the engine seized up—and I say this because I can now envisage a *continuous series of happenings* between the leak and the engine seizure which themselves are quite understandable—as the original sequence 'leak-to-seizure' was not.

Let me make my point clearer by anticipating two likely objections. The first, which is a version of the argument used by Russell in his well-known essay on causation,[1] is that the idea of a 'continuous series' is philosophically naïve because of the infinite divisibility of space and time. Russell used this argument to outlaw the word 'cause' from science, but this aspect of it need not concern us here.

It does not seem to me that such an argument from spatio-temporal infinity raises any real difficulty for the point I wish to make. For there is no harm in admitting that the various sub-events which would have to be mentioned in an explanatory account of the engine seizure form a continuous series in a relative rather than an absolute sense. That is not to say merely that each link in the chain of circumstance is itself closer to some ideal of continuity, so that the best explanation would be the one which carried the process farthest. The point is rather that in offering a sum of sub-sequences to

[1] 'On the Notion of Cause', in *Mysticism and Logic*, London, 1918.

explain a gross one, the former must be *acceptable* to some person, investigator, craft, audience, &c. They must themselves raise no further demand for explanation in that particular context. They are—to use a convenient term of Dr. F. Waismann's—'hat-doffing' phenomena.[1] They do not puzzle us; we ask no questions of them; we just 'take off our hats to them'. So although Russell's objection to the notion of a continuous series is, in a sense, formally sound, it is pragmatically false. And, as I shall argue further in this, and succeeding, chapters, there is an irreducible pragmatic dimension to explanation. In a case like the one under consideration, it would, of course, be open to anyone to question whether a particular series is, in fact, a continuous one. But this, although it may reveal what is a 'hat-doffing' phenomenon for him, does not prove that no series are ever continuous, i.e. that the term 'continuous series' has no use.

The second objection may seem at first a more embarrassing one. For a covering law theorist might at this point ask how my admittedly more complicated account of what is required to explain the engine seizure differed in any *logical* way from the covering law theory itself. For what I call 'hat-doffing' sub-sequences, it may be contended, are surely themselves accepted and unquestioned just because they are 'what always happens', 'what was to have been expected under the circumstances'; and since, in the present chapter, the claims of the model on its necessary condition side are not being questioned, these sub-sequences can be regarded as straightforward cases of subsumption under law. No doubt we often find it desirable to break down a big explanatory job into parts, each to be dealt with separately; and the way this is sometimes done in history has already been discussed.[2] But the question to be answered here is whether, in cases where although we do not just call upon a single covering law, we nevertheless call upon a covering conjunction of them, any other logical criterion is applied. And the covering law objector will regard it as obvious that it is not.

[1] See Toulmin, op. cit., p. 117.

[2] In Chap. II, section 6.

The objection is both plausible and important. But I want to insist that what is added to covering law requirements by the analysis of our example is essential, and that it is a logical condition of giving the explanation—at any rate, in the broad sense of 'logic' familiar among analytic philosophers. For it is my claim that it is essential to the notion of giving an explanation that even if subsumption under law were a necessary condition of it, there should be criteria which allow us to distinguish some law-covered phenomena from others.

The difference between my analysis of the present example and that of the standard covering law theory could be emphasized thus. The general law, 'Whenever your oil leaks out your engine seizes up', does not explain the fact that my engine seized up after my oil leaked out—in the context of puzzlement envisaged. But reference to a series of facts constituting the story of what happened between the leakage of the oil and the seizure of the engine does explain the seizure. Even if it were true that these smaller scale events were each covered by law in the sense that in every case I would be prepared to assent to a law corresponding to a sub-sequence, the laws involved would be, at most, part of the explanation of the gross event, not of the sub-event they cover; so that when they do function in an explanation they are not *covering* laws at all.

Thus, although the engine seizure—the gross event—may be said to be explained by assuming many sub-laws like, 'When the walls of a cylinder and piston are dry they heat and expand with motion', the law mentioned would not in turn explain why the piston expands and heats up—if we were to go on to ask that question. The sub-law is part of the explanation of the gross event, although it does not *cover* it; the same law covers the sub-event, although it does not *explain* it. Once a gross event has been seen to require an explanation, then there is a two-levelled structure of events and laws to be reckoned with. Satisfactory explanation, if it employs laws at all, employs laws only of the lower level. To appeal to a gross covering law would be, in effect, to short-circuit the real work the explanation is intended to do. Subsumption under such a law can scarcely, therefore, be represented as a sufficient condition of giving the explanation.

Let me try to clarify my point by anticipating a further difficulty which may seem to lurk behind the claim I am making. I have said that a gross law does not explain the gross event; for this we need sub-laws. And I might have added that the sub-laws, in turn, do not explain the sub-events; for this we need sub-sub-laws, and so on. The conclusion which may falsely be drawn from this is that nothing can ever really be explained, for the attempt to give an explanation leads us directly into an infinite regress. For some philosophers the explanatory regress has seemed to go in another, but equally embarrassing direction. Thus McTaggart, having denied that merely subsuming an event under a law explains it, intimates that this is because "the law itself has not been explained". And no matter how far you carry a hierarchy of higher-order laws explaining lower-order ones, you eventually have to accept a "law which is ultimate and cannot be explained further".[1]

This very common pattern of argument is quite illegitimate —at any rate as a proof that we can never explain anything, or even that we can never explain anything *satisfactorily*. As I was careful to say, the law covering the piston's behaviour does not explain why it heats up *if we should go on to ask that question*. The condition italicized should be taken seriously; for this is *not* the question which was answered appropriately in terms of laws at the level of the piston's behaviour. Should we decide to *change* our question to 'Why did the piston heat up?', then it would become necessary to go beyond laws of the sub-level, and so on. No doubt if we adopt the policy of *continually* changing our question, it will be impossible for anyone to produce an answer which we shall be prepared to accept as a satisfactory explanation. But so long as we ask one question at a time, no regress occurs.

[1] *Philosophical Studies*, London, 1934, p. 166. McTaggart puts the point in terms of *causal* laws, but I suppress the qualifying term to avoid needless complication here. But see Chapter IV.

To put it another way: a person who adopts the *policy* of always refusing to accept an x as the explanation of y unless the x is itself explained, begins to empty the term 'explanation' of its normal meaning. And if he goes on—as I suspect many objectors do without realizing it—to demand that any explanation of a y in terms of an x should *at the same time* explain x (and so on, *ad infinitum*), he empties the term of all meaning. He really no longer knows what he means when he asks for an explanation; he does not know what would count as one. But we need not take even the first step toward such a position, for a complete or satisfactory explanation is not necessarily one given in terms of what is itself explained. It is in terms rather of what (for the moment, or at this level, or for the purpose of this inquiry, &c.) does not require explanation. It is part of the *logic* of 'explanation' that if something can be explained, there is something else which does not require explanation. But the reason it does not require explanation is not necessarily that we know its explanation already.

4. *The Ordinary Meaning of 'Explain'*

The example of the engine seizure might be regarded as setting up, against covering law theory, a 'model of the continuous series'. Such a model does not necessarily apply to everything which is legitimately called 'explanation'; nor do I wish to suggest that the discussion of the preceding section brings out all the important features of those explanations to which it does apply. But it does, at any rate, provide a useful corrective to the covering law model as it is usually elaborated. It brings out, for instance, the force of saying, as some philosophers do, that explanation is concerned with finding 'middle terms'.[1]

It would appear, indeed, that there is an *essential* complexity about what is ordinarily considered explanatory; that once the demand for explanation arises, an answer which does no more than represent what is to be explained as what we always find happening in such circumstances fails to explain it at all. The

[1] e.g. M. R. Cohen, 'Causation and its Application to History', *Journal of the History of Ideas*, 1942, p. 18.

complexity, the element of *analysis* of the case under consideration, need not always take the form displayed by the engine seizure—it may not, for instance, be a temporal account. But some sort of analysis besides mere certification as a recurring phenomenon, would seem to be essential. I assume, of course, that the demand for explanation arises out of a genuine puzzlement, and that the explanation is offered in good faith—not as a joke, or in order to silence the questioner. Thus, if the objection were raised that it is common practice for harassed parents to respond to their children's 'why' questions with 'That's what always happens', I should insist that such a response, far from being an explanation, is just a way of registering either their inability or their unwillingness to give one.

I should like to make it clear that the application of the present logical doctrine to historical cases requires more than the mere admission—which many covering law theorists are quite prepared to make—that historians, in giving explanations of what they study, normally want to give a fairly detailed account of what happened. For I have argued, not that explanations often *do* go beyond certifying something as 'What always happens', but rather that they *must*. Mr. Gardiner, for instance, would agree that in seeking an explanation of, say, the unpopularity of Louis XIV, an historian would usually feel obliged to do more than cite a covering law which directly generalized his original explanatory statement. But, on his view, this is only because the gross covering law which might be extracted from that statement is too vague, too unreliable as a guide to prediction, so that it needs to be replaced by one more precisely stated; and it is in order to fill out the antecedent clause of such a law that the historian insists on a close analysis of the particular case. My point is rather that it is the *unintelligibility* of the gross sequence, not just the predictive unreliability of a general law corresponding to it, which makes necessary such further analysis. It is a *pragmatic*, not an inductive, modification of the model's account which is required in this connexion.

Exponents of the model who object to my insisting that we take into account a pragmatic dimension of the concept, may perhaps argue that my belief that this is necessary is traceable to a mistake about the lessons to be drawn from the example of the engine seizure. For it may be alleged that my discussion has failed to draw an important distinction between giving 'the explanation' of something, and giving what amounts to 'an explanation for so-and-so'. It may be said that what I represent as pragmatic criteria of explanation are really not criteria which must be satisfied for something to be an explanation at all; they merely reflect the fact that various individuals find some explanations more satisfactory than others—although all may be formally sound, and all properly called 'explanations'. As Mr. J. Cohen puts it: "What is an explanation for one person may be none at all for another, since its achievement in this direction will vary in accordance with the factual beliefs (or even the emotional make-up and current feelings) of those interested."[1] But in discussing the logic of the concept, Cohen thinks it proper to ignore this 'psychological category of explanation'. And what is left turns out to be analysable simply in terms of regularity.

The distinction between giving 'an explanation for so-and-so' and giving 'the explanation' is one which should, I agree, be drawn. But I cannot see that drawing it need be regarded as reinstating the covering law claim. For although the use of the latter expression appears to presuppose objective criteria for what shall count as explanation, while the use of the former presupposes the contrary, the distinction between objectively and subjectively acceptable explanations need not coincide with the distinction between those which are formally and pragmatically sound. There are undoubtedly contexts in which the combination of knowledge and ignorance which gives rise to the demand for explanation, and the standards of intelligibility which will be applied to what is offered as explanation, will vary considerably from person to person. In

[1] 'Teleological Explanation', *Proceedings of the Aristotelian Society*, 1950–1, p. 259.

such contexts there will be no point in speaking of *the* explanation at all. The use of this expression presupposes *shared* criteria, but still criteria of the pragmatic sort. It is not necessary to retreat into a formal definition of 'explanation' as 'showing something to be deducible from a general law' in order to envisage objective standards for what shall count as explanation, although the belief that it *is* necessary may account for some of the reluctance of covering law theorists to regard what I have called the pragmatic dimension of the concept as anything more than a psychological peculiarity.

My claim that we cannot give a proper account of explanation without bringing out its pragmatic dimension obviously harks back to some older-fashioned discussions of the subject. Professor S. Stebbing, for instance, in her *Modern Introduction to Logic*, represents explanation as the reduction of the unfamiliar to the familiar, the unknown to the known. She writes: "What is familiar is usually taken to be understood, so that in its simplest form the answer to the question consists in pointing out a connection between the fact to be explained and something that is familiar."[1] In his *Probability and Induction*, Mr. W. Kneale gives a different account. "An explanation", he says, "must in some sense *simplify* what we have to accept."[2] He thus regards the explanatory use of theoretical laws (he calls them 'transcendent hypotheses') in science as aimed at reducing "the number of transparent necessitations we need to assume". As my discussion of the explanation of human action in Chapter V will show, I do not think that either Stebbing's or Kneale's account, or, indeed both taken together, bring out *all* the non-inductive requirements we recognize in giving explanations. But there is little doubt that both mention important demands which are in fact often made; and that these are appropriately called 'pragmatic'.

Taking account of the pragmatic dimension of explanation brings the analysis of the concept more into line with the way the word is used in the ordinary course of affairs. Besides 'to make clear the cause, origin or reason of', the *Oxford English*

[1] 2nd edn., London, 1933, p. 389.

[2] Oxford, 1949, p. 91.

Dictionary gives the following as general meanings of 'explain': 'to smooth out', 'to unfold', 'to give details of', 'to make plain or intelligible', 'to clear of obscurity or difficulty'.[1] The model of the continuous series, with its suggestion of unrolling or revealing what was previously unknown or puzzling, is also closer to such common ways of talking as 'explaining my purpose', 'explaining my point of view', 'explaining my meaning', 'explaining the use of this word, that tool, &c.'—all difficult to account for on the covering law model. Such a sampling of ordinary uses of the term 'explain' must at least suggest that the analysis which this model offers cannot have a very wide application—even that it may be a special sense invented for a special purpose.

There is, in fact, some reason for thinking that what the covering law theory gives us is the criterion of a technical sense of 'explanation' found only in narrowly scientific discourse, perhaps only among certain philosophers of science. I remarked in Chapter I that the theory found most of its early support among philosophers who regarded their task as chiefly the analysis of the language and procedures of science, especially physical science. Hempel's formulation begins by laying down the logical structure of explanation as he believes he finds it in physics; he then goes on to show that historical cases approximate to this ideal in varying degrees. There is no apology for the direction of the analysis from physics, where the logical outline is boldly displayed, to other fields, where traces of the model have to be found by dint of careful reconstruction.

Whether a sense of 'explain' is widely employed among theoretical scientists which means no more than 'bring under a general law' I cannot claim to know, although I suspect that it is at any rate less widespread than the philosophers in question would lead us to believe. Professor P. W. Bridgman, for instance, in *The Logic of Modern Physics*, declares that

[1] Explanation in terms of causes is discussed in Chapter IV; explanation in terms of reasons in Chapter V; explanation in the sense of removing difficulty in Chapter VI.

"the essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests"[1]—and it is scientific explanation which he has especially in mind. A view similar to those expressed by Stebbing and Kneale can also be found in N. R. Campbell's *Physics, The Elements*. "Explanation", he writes, "consists in the substitution of more for less satisfactory ideas. Ideas may be more satisfactory either because they are more familiar or because they are simpler."[2] Then, directing his attention to scientific explanation, he continues: "Such explanation of laws as is effected by other laws is explanation of the second kind, the explaining ideas being simpler because they are more general."

But it really does not matter for my own argument whether the majority of scientists and philosophers of science recognize in the covering law model what *they* commonly mean by 'explanation' when they are doing or describing physics. For my present aim is to break down the plausibility of the claim that this restricted meaning—whether it has a legitimate use elsewhere or not—*must* apply to historical cases, and in this connexion it is relevant to show that it in fact departs from the *ordinary* meaning of the term. Furthermore, since the narrow meaning, as the quotation from Campbell suggests, is not entirely unrelated to the ordinary one, the former might be regarded as an abstraction from the latter. For in claiming that the pragmatic criteria are essential, I have not intended to deny that the elaboration of a continuous series may often satisfy the condition that what is explained be predictable from the data which the explanation contains.

It seems to me that what covering law theorists have done is to seize on (and, as shown in Chapter II, to misinterpret) a necessary condition of (some kinds of) explanation which is so closely connected to the purpose of science—control—that it has been mistaken for a sufficient condition. 'Explanation', as covering law theorists use it, is a technical term; and, as such terms so often do, it abstracts from a term in ordinary use the

[1] New York, 1948, p. 37. [2] Cambridge, 1920, p. 113.

aspect which is of most interest in the kind of inquiry for which it is redesigned. Provided we realize what we are doing, there is no harm in such redefinition of terms. But if scientists, for their own legitimate purposes, redefine 'explain' so that it means roughly what covering law theorists say it does, then we are quite justified in advertising our awareness of what has been done by saying that, in fact, scientists do not seem to be much interested in explanation; they care only for 'explanation' (as technically defined). If the purpose of science is indeed the elaboration of predictive mechanisms rather than (as is still sometimes believed) an attempt to 'understand the world', then the technical term 'explain' will be very useful; it will allow us to indicate in a convenient way phenomena the form of which has been captured by some scientific law or theory. What the philosopher of history must resist is any attempt to force the new concept into currency in situations where the job is to *explain* rather than merely to 'explain'. And this, we may with some justification suspect covering law theorists of having done.

It is all too easy to confuse two questions: the purpose of science and the meaning of explanation. If we keep them distinct, we shall know what to say if a covering law theorist retorts: 'I admit that there is an ordinary meaning of the term "explanation" more or less as you have outlined it, but I am interested only in scientific explanation, and in historical explanation in so far as it is scientific.' Provided that 'scientific' is not simply taken to be equivalent to 'reputable', the proper reply is to invite the objector to show that the technical, derivative sense of 'explanation' is in fact used in history; and if it is not, to show good reason why it should be adopted. That the need for making such a case for it should have been overlooked is due to the too facile assumption of many covering law theorists that common sense and historical explanations are just woollier versions of the kind scientists give, in conjunction with the belief that in science explanation is always given on the covering law model. But once it is suspected that covering law theory, at most, formulates a

criterion for a technical sense of the term, a great deal of the persuasiveness of those who urge the adoption of the model as 'scientific' disappears.

5. *Theoretical and Historical Explanation*

I have argued that what covering law theorists really advocate is the importation into historical studies of a special, technical sense of the term 'explanation' designed for narrow scientific uses. But even to say this may not bring out the full extent to which covering law theory *prescribes* a sense of the term, rather than calls attention to one already accepted. For although, as I have already said, it is not my purpose here to assess the adequacy of the covering law model in scientific contexts, it would appear to be at least arguable that reference to a scientific theory may be explanatory in the *ordinary* sense, while reference to a generalization is not. In so far as reference to a theory does give an explanation—in science or elsewhere—it seems to me that it does so not for the quasi-inductive reasons suggested by Gardiner and others, but because it is a means of satisfying just the kind of pragmatic demands which we have been discussing.

How did we come to think that reference to a theory *ipso facto* explained what fell under it? Let me hazard a hypothesis. Why does the theory of geometrical optics explain the length of particular shadows? At the risk of stretching Toulmin's account, it is surely because a ray diagram goes along with it, allowing us to think of light as travelling along ray lines, some of the lines passing over the wall and others coming to a dead halt on its surface. The shadow length is explained when (to use a phrase of Toulmin's) we think of light as 'something travelling', i.e. when we apply to it a very familiar and perhaps anthropomorphic way of thinking. If we were just given an equation or even a geometrical figure, this would not be sufficient to explain the shadow lengths, no matter how faultlessly this mechanism allowed us to calculate them.

Although it may be considered reactionary to say so, it seems to me that scientific theories normally have to meet two

quite different demands. First, they must increase our predictive power, i.e. have the characteristic of *generality*. Gardiner's discussion of the role of scientific theories emphasizes only this aspect. Second, they must explain the phenomena, i.e. have *intelligibility*. It is wrong to think that satisfying the first demand automatically satisfies the second—which is what covering law theorists in effect say in so far as they allow theory a special place in explanation. Some theories, we must admit, may be just inferring techniques, since they may lack a model. But if there are any such, perhaps we should think twice about calling them *explanatory* theories; at most they 'explain', in the technical sense.

My contention is, therefore, that in so far as the light ray theory explains shadow phenomena, it is because of its implicit reference to rays of light running tramlike along celestial rails from a certain source. Similarly, that the volume of a gas expands with increase of heat, is explained by the kinetic theory of gases, in that it allows us to think of gases as composed of little particles which increase the momentum with which they strike the sides of their container. Thus the role of theory in such explanations is really *parasitic* upon the fact that it suggests, with the aid of postulated, unobservable entities, a 'hat-doffing' series of happenings which we are licensed to fill in. The theory allows us to tell 'a likely story' behind the appearances. But if the travelling of observable entities along observable rails in a similar way would not explain a similar pattern of impact on encountering a wall, and if the jostling of a tightly packed crowd would not explain the straining and collapsing of the walls of a tent in which they were confined, then the corresponding scientific theories would not explain shadow lengths and the behaviour of gases.

In history, as I have already remarked, explanations are seldom given by means of, or in terms of, theories. In this respect, they are to be contrasted not only with explanations in the formal sciences, but with everyday explanations of the sort illustrated by the engine seizure as well. For the latter was a theoretical explanation of a sort. The mechanic's

announcement, 'There's a leak in the oil reservoir', is explanatory only when taken in conjunction with what we might call the theory of the internal combustion engine. It is the assistant mechanic's general knowledge of the way auto engines work which allows him to fill in for himself the missing links in the chain of circumstances, on the basis of the chief mechanic's statement. As in the strictly scientific case, no independent knowledge of the intervening links of the chain is needed. The theory itself is sufficient to license the *interpolation* of a 'hat-doffing' series behind appearances—i.e. under the bonnet.

In another respect, however, the mechanical example is more like an historical case. For the mechanic's theory licenses the filling in of potentially observable happenings; the explanation derived from it employs no abstract entities. In typical historical cases, too, the continuous series constructed by the historian's explanatory narrative will consist of observable happenings. The peculiarity of the historical case is that, normally, each event in the series will be established independently from evidence. There will be no general theory, even of the mechanical kind, to make detailed research into the actual course of events unnecessary. But lack of an organizing theory, as we should now be able to see, need not prevent the historian from giving explanations which are quite as 'high-grade' as those given in theoretical terms in other fields. We often explain by means of, or in terms of, a theory, but there is nothing in the nature of such explanation which need persuade us that we cannot explain satisfactorily without one. For to explain with the aid of a theory is to do indirectly what the historian, perhaps painstakingly and piecemeal, does directly: reduce what is puzzling to what is not.

In view of the contrast I have drawn between explanations in historical and non-historical contexts, it may be of interest, in concluding this chapter, to turn briefly to Professor White's question about the nature of specifically historical explanation. For I think that the unsatisfactory answer which he felt obliged to give to it may now be seen to arise at least partly out of his prior acceptance of the covering law view of the

logical structure of all explanation. White, it will be remembered, concluded that there were *no* explanations which could properly be called specifically historical. For if all explanation is given by subsumption of case under law, the only way to distinguish *kinds* of explanation, he thinks, is by the use of either laws or terms essential to the various disciplines; and neither principle isolates a class which can plausibly be characterized as 'historical'. Explanations found in history books which do not belong to any of the formal sciences White therefore assigns to the not yet clearly defined social sciences, even though the laws 'presupposed' by them may not have been discovered by certified investigators in these fields, and the terms employed may not at first seem to 'belong' to any particular discipline.

The corollary which White in this way draws from Hempel's statement of the model has not been accepted by all covering law theorists. Professor E. W. Strong, for instance, objects to the suggestion that historians have, from time to time, presupposed non-existing sciences.[1] Herodotus, he allows, used terms which have now been appropriated by psychologists; but this is not to say that he therefore used specifically psychological terms. Gardiner, too, criticizes White's argument on the ground that history is written in ordinary rather than technical language. "A bona fide historical explanation" of the establishment of new hospitals in England after 1700, for instance, would take the form: "they were the outcome of individual initiative and co-ordinated voluntary effort and subscription";[2] and there are no terms in it which are in any way technical. But neither Gardiner nor Strong says what it is about such explanations which make them specifically historical. Gardiner's constant emphasis on the 'looseness' of historical language may, indeed, give the impression that he thinks it a distinguishing feature of historical explanation that it be expressed in vague terms. But although historical explanations would, on this ground, be marked off from all

[1] 'Criteria of Explanation in History', *Journal of Philosophy*, 1952, p. 60.
[2] Op. cit., p. 63.

scientific ones as 'non-technical', the same criterion would, of course, admit as specifically historical *all* the explanations given in daily life which are also framed in ordinary language.

Professor Popper, although also approaching the question within the framework of covering law theory, offers a different and more direct answer to it. As we saw in Chapter I, one of the implications which Popper draws from the model as he states it is the necessity of distinguishing between the 'historical' and the 'pure generalizing' sciences. The latter do not explain particular facts. To do this is the task of the historical sciences; and they perform that task by assuming or taking for granted the laws which, ideally, these other sciences discover. According to Popper, "*all* causal explanation of a singular event can be said to be historical in so far as the 'cause' is described by singular initial conditions".[1] It is historical, presumably, because it explains a particular fact—a bit of history—by applying to it a known law. Popper does not, at any rate in the sources indicated, say precisely what such specifically historical explanation is to be contrasted with. But the contrast he has in mind appears to be with explanations achieved by *discovering* rather than assuming the law which governs the facts investigated. There is thus, for him, a pragmatic difference—a difference in the *direction of inquiry*—between historical and non-historical explanation.

Popper's criterion has the merit of distinguishing between historical and non-historical explanation *within* the class marked off by Gardiner as non-technical. Yet his criterion, like Gardiner's, is too broad; for it cuts across the class of technical (i.e. scientific) explanations as well. It would, for instance, classify as historical the explanations given by a chemistry demonstrator of the changed colour of a piece of litmus paper after being dipped in an acid solution. The statement, 'It was dipped in that acid solution', sets out a 'singular initial condition', but it would hardly be regarded as giving anything which we should normally call an historical

[1] 'The Poverty of Historicism', *Economica*, 1945, p. 83. See also *The Open Society*, vol. ii, p. 262.

explanation. For the real work of the explanation is done by a chemical theory which the demonstrator knows how to apply to the case. And although White's analysis was in other ways unsatisfactory, he was surely right to insist that no criterion which allowed an overlap of, say, 'historical' and 'chemical' explanations could be acceptable.

Indeed, as the discussion of the present chapter has suggested, it would be very natural to draw a sharp contrast between historical explanations and *all* theoretical ones. This becomes more obvious if we rephrase the question, 'What is it to give an historical explanation?', as 'What is it to explain something historically?' A theory of the subject matter, as we have seen, may *excuse* an investigator from explaining a thing historically; a specifically historical explanation is given where what is to be explained cannot be understood merely by referring to such systematic general knowledge. We give theoretical explanations where our knowledge of the subject matter allows explanatory interpolation; we give historical ones where no such interpolation is licensed—where we have to refer to the peculiar history of what is to be explained. On this view, it might be noticed, a historical explanation would be distinguished from an applied sociological one—as on Popper's it would not.

In *The Nature of Historical Explanation* Gardiner warns us against thinking that "provided a careful search is conducted, a 'clear and distinct idea' of what historical explanation *is* will somewhere be found".[1] If by this he means that the term 'historical explanation' has no single 'correct' use, I should not want to disagree. I should not want to claim any more for the sense sketched above than that it is close to what we should probably mean if we called one explanation 'historical' by contrast with another, and that the contrast is different in kind from the one sought by White in terms of covering law theory. That there are other uses of the term 'historical explanation', both broader and narrower than this one, I do not doubt.

[1] Op. cit., p. xi.

A common narrower use would require that the explanatory story itself include reference to facts temporally remote from what is explained—a use suggested by Butterfield's observation that the Whig historians "found a historical explanation for the conduct of the Whigs".[1] And a broader one is employed by Gardiner throughout his book, since he generally takes 'historical explanation' to be equivalent to 'explanation found in history books'. Gardiner's broad use may appear to have the advantage of making it possible to say that historians, when they offer explanations, always offer historical explanations; for we could not say this on the narrower interpretations distinguished above. But, as the chapters to follow will help to make clear, if we adopt the broad use of the term, it is unlikely that we shall find any *logical* features according to which all historical explanations can be grouped together as historical. For the explanations found in history books are a logically miscellaneous lot.

[1] *History and Human Relations*, p. 121.