

INTRODUCTION

we have to choose either a positivist (and naturalist) position, or an idealist (and anti-naturalist) position. This is, however, incorrect. We will criticize how positivists have succeeded in dictating the way in which the methodological unity issue has been debated. In discussing three different conceptions of the natural sciences, positivism, realism, and conventionalism, we will attack the normal assimilation of the two dichotomies, and particularly that of the positivist and naturalist positions. We will examine the issues involved in a naturalism based on a non-positivist, realist view of science.

Let us now outline the three parts of this book. In part 1 we analyse and contrast positivist, realist and conventionalist conceptions of the natural sciences. In each case we characterize their basic claims, outline the justifications provided for these, and indicate the main difficulties and criticisms that can be made of them. Our own commitment is to a realist position, modified by certain conclusions which have led others to adopt a non-realist, conventionalist view of science.

In part 2 we use this classification of alternative conceptions of science, and especially the distinction between positivism and realism, to analyse different writers and schools within the social sciences. We consider those who, although adopting a naturalist position, differ in the view of science that they hold. Initially we look at various positivist sociologists and schools of sociology in the nineteenth and twentieth centuries. We then analyse a clear example of realism in the social sciences, namely, Karl Marx and his attempt to found a science of historical materialism. Finally, we consider the concept of structure, which is central to any realist science. We note, however, that there are other forms of structural or structuralist analysis in the social sciences; some of these can be seen as either positivist or as a form of conventionalism.

In the last part, we examine the two central problems in any science of social life; the nature of human action, and the possibly value-distorted, subjective, or ideological character of the social scientific enterprise. We will see that some of the anti-naturalist arguments involved here are analogues of conventionalist claims in the philosophy of the natural sciences. We will consider the plausibility of an anti-naturalist position by reference, not to a positivist, but a realist, naturalist position. First, then, we consider how subjective meanings can be analysed as causes of human action. We will then consider whether there is a distinctive form of interpretative understanding in the social sciences. Second, we examine different conceptions of ideology; we analyse in detail one form of ideological distortion, that of reification; we confront various issues involved in the concept of value-free sociology, especially the relationship between concepts and reality; and we defend a non-relativistic sociology of science and knowledge.

part one

Conceptions of science

In the following three chapters, we will present a critical analysis of three widely differing conceptions of the natural sciences – positivism, realism, and conventionalism. The analysis will be conducted primarily by reference to Anglo-American philosophers of science who have developed and defended these positions in the twentieth century. These positions have important connections with earlier views in the history and philosophy of science, and also with more general philosophical and intellectual movements. But, first, let us provide a brief outline of the three positions.

For the positivist, science is an attempt to gain predictive and explanatory knowledge of the external world. To do this, one must construct theories, which consist of highly general statements, expressing the regular relationships that are found to exist in that world. These general statements, or laws, enable us both to predict and explain the phenomena that we discover by means of systematic observation and experiment. To explain something is to show that it is an instance of these regularities; and we can make predictions only on the same basis. Statements expressing these regularities, if true, are only contingently so; their truth is not a matter of logical necessity, and cannot be known by *a priori* means. Instead, such statements must be objectively tested by means of experiment and observation, which are the only source of sure and certain empirical knowledge. It is not the purpose of science to get 'behind' or 'beyond' the phenomena revealed to us by sensory experience, to give us knowledge of unobservable natures, essences or mechanisms that somehow necessitate these phenomena. For the positivist, there are no necessary connections in nature; there are only regularities, successions of phenomena which can be systematically represented in the universal laws of scientific theory. Any attempt to go beyond this representation plunges science into the unverifiable claims of meta-

physics and religion, which are at best unscientific, and at worst meaningless.

The realist shares with the positivist a conception of science as an empirically-based, rational and objective enterprise, the purpose of which is to provide us with true explanatory and predictive knowledge of nature. But for the realist, unlike the positivist, there is an important difference between explanation and prediction. And it is explanation which must be pursued as the primary objective of science. To explain phenomena is not merely to show they are instances of well-established regularities. Instead, we must discover the necessary connections between phenomena, by acquiring knowledge of the underlying structures and mechanisms at work. Often, this will mean postulating the existence of types of unobservable entities and processes that are unfamiliar to us: but it is only by doing this that we get beyond the 'mere appearances' of things, to their natures and essences. Thus, for the realist, a scientific theory is a description of structures and mechanisms which causally generate the observable phenomena, a description which enables us to explain them.

We have noted already that, despite their differences, positivists and realists share a certain general conception of science and its objectives. What is common to those philosophers of science we call 'conventionalists' is their rejection of these shared attitudes. But their reasons for doing so are varied. It may be argued that observations cannot by themselves determine the truth or falsity of theories, and that no useful distinction between theory and observation can be maintained. Or, that there are no universal criteria for choosing rationally between different theoretical frameworks, and that moral, aesthetic, or instrumental values play an essential part in such choices. More radically, the idea of an external reality which exists independently of our theoretical beliefs and concepts may be rejected. Associated with these different claims are different positive conceptions of science. In all of them, there is a sense in which the adoption of theories is a matter of convention. But 'conventionalism', as we use this term, does not denote a homogeneous set of views about science, which can be simply contrasted with positivism and realism. What unites conventionalists is their opposition to the view of science as providing true descriptions and explanations of an external reality, through theories which can be objectively tested and compared by observation and experiment.

So far, these accounts of positivism, realism and conventionalism have been couched, as far as possible, in terms that are not specific to the way in which they have been developed in twentieth-century philosophy of science. For we wish to emphasize that each of these views has a long history in science and philosophy. The realist

position, with its emphasis on causal explanations through the discovery of essences, was systematically articulated by Aristotle, developed by various medieval philosophers, and continued through the seventeenth-century scientific revolution and after; for example, in the writings of Locke, who developed an epistemology based on the corpuscularian realism of the science of his time. Positivist philosophy of science was significantly developed in the early eighteenth century, through the work of Hume and Berkeley, with the denial of causal necessity in nature, the defence of a regularity view of causation and explanation, and the rejection of any scientific concepts which went beyond the realm of the observable. Elements of positivism were already present in the writings of medieval philosophers, such as Ockham; and the positivist tradition continued through the nineteenth and twentieth centuries when, until quite recently, its dominance seemed assured. Likewise, several of the ideas involved in conventionalist philosophies of science have a long history, particularly in astronomy. From very early on, there developed the view that all that mattered for an astronomical theory was that it should 'save the appearances', that it should be a computational device which enables us to make correct and useful predictions about the observed movements of the heavenly bodies. Such theories should not be seen as describing any physical reality, or making claims to truth: their value was primarily instrumental. If more than one theory adequately 'saved the appearances' the choice between them should be made on the basis of partly aesthetic criteria, such as mathematical elegance.¹

These different conceptions of science have also been related to more general philosophical positions and movements. In the twentieth century, these relationships are particularly interesting. Until about twenty years ago, the dominance of positivist philosophy of science was closely associated with the dominance of the logical positivist movement, itself a complex blending of the Humean empiricist tradition with the late nineteenth-century development of mathematical logic. The recent attacks on positivist philosophy of science, which have taken both realist and conventionalist forms, have themselves been linked with some of the philosophical movements that emerged in opposition to logical positivism. Thus, several conventionalist philosophers of science have been influenced by the later writings of Wittgenstein, and realist philosophy of science has partly been developed from the standpoint of 'scientific realism', a position which is opposed both to logical positivism, and also to the movement of analytical philosophy inspired by Wittgenstein, Ryle and Austin.²

It should already be clear that our own use of the term 'positivism' is a more restricted one than is usual, particular in debates about the methodological unity of the natural and social sciences. First, the

term is frequently used in such a way that the positions which we distinguish as positivist and realist are conflated under the terms 'positivist', or 'empiricist'. Although positivism and realism have common features, and have both been developed within a broadly empiricist philosophical tradition, the failure to distinguish the two positions is misleading. Indeed, the differences between these positions itself reflects important divergences within the empiricist tradition, several of which are manifested in the differences between Hume and Locke. Second, 'positivism' is sometimes used as if synonymous with 'naturalism', a use which we have already criticized. Third, the term is also applied to a more general intellectual tradition which, although partly constituted by what we have called positivist philosophy of science, also involves other, more sweeping claims. In particular, positivists in this wider sense not only adopt a certain view of the natural sciences, but insist that science, conceived in this way, is the only legitimate form of human knowledge. Other intellectual enquiries must either conform to this model of knowledge, or be dismissed as providing no real knowledge at all. Questions of values, theology and metaphysics are to be rejected in this manner, and the embryonic social sciences must, to deserve the name of 'science', be developed on the lines of the natural sciences.

But positivism purely as a philosophy of science may be maintained without these additional claims. Thus some philosophers have adopted this view of science, without thereby rejecting other forms of knowledge as meaningless or unintelligible. Instead, they may merely wish to distinguish science from, for example, theology, sometimes even to protect theology from the claims of science, rather than using science as a weapon against theology. Similarly, though positivist philosophers of science deny the relevance of values to the conduct of science and the validity of its results, this denial need not be based upon the rejection of value claims *in toto*: they may simply be regarded as having no legitimate part in science.³

Our procedure in these three chapters will be as follows. In chapter 1, we examine the positivist view of science, beginning with its analysis of explanation. We then consider its general view of scientific theories, and their relation to observations. An important aspect of this view is its use of a distinction between theoretical and observational terms; this we examine separately. And in the final section, some further characteristics of positivist philosophy of science as a whole are considered, especially those associated with its concept of 'the logic of science'.

Throughout chapter 1, although we emphasize the difficulties and problems that are raised by various positivist doctrines, we do not attempt any systematic criticism of the position. But in chapter 2, we present the realist view of science both as an alternative to

positivism, and in terms of its critical diagnosis of the problems facing positivism. Thus in the first three sections, we partly parallel the corresponding sections of chapter 1, by discussing the realist accounts of explanation, theories, and the distinction between theoretical and observational terms. We conclude by pointing to some of the problems for a realist philosophy of science, and drawing out some of the characteristics common to both positivism and realism.

The organization of chapter 3 is rather different from that of the first two chapters. Only in the final section do we consider conventionalism as a general position in the philosophy of science, and even here we emphasize the diversity of positions that may be grouped under this single category. In the earlier sections of the chapter, we confine ourselves to a critical analysis of several writers and arguments which stand in opposition to both positivism and realism.

Finally, the accounts we provide of both positivism and realism are composite constructions, and although there may be philosophers of science who subscribe to every element of one of these, this is not assumed by the procedure we have adopted. Thus, we do not claim that any writer who supports, say, the positivist view of explanation, will necessarily support all the other doctrines we describe as positivist – though support for at least some of them is likely. The various elements of the realist and positivist positions do fit together into coherent, alternative conceptions of science, most fully developed in the twentieth century, but present at many earlier stages in the history of science and philosophy.⁴

1 Positivist philosophy of science

1 The positivist view of explanation

That the explanation of an event consists basically in showing that it is an instance of a well-supported regularity has long been maintained by positivist philosophers of science. By examining the detailed development of this view by twentieth-century positivists, we will also gain insight into the main difference between these more recent writers, and their predecessors. This consists in the considerable reliance of the former upon the techniques and concepts of modern formal logic. These have been used both to express characteristically positivist views of science with a far greater degree of rigour and precision than was previously possible, and to provide a basic framework in which to analyse the nature of scientific theories.¹ There can be little doubt that the attendant rigour and precision have often been highly beneficial to the philosophy of science – though not always to the positivists, since the careful and systematic statement of their views has frequently revealed serious problems and difficulties which may be unresolvable within the positivist framework.

We will examine the positivist view of explanation in the form in which it has been presented and defended by Carl Hempel, since his account is, in several senses, exemplary.² Let us begin with the following passage (1948, p. 246), in which he describes a typical case of scientific explanation, and presents an analysis of it which indicates his general conception of such explanations:

A mercury thermometer is rapidly immersed in hot water; there occurs a temporary drop of the mercury column, which is then followed by a swift rise. How is this phenomenon to be explained? The increase in temperature affects at first only the glass tube of the thermometer; it expands and thus provides a larger space for the mercury inside, whose surface therefore

be used as a premise in D-N arguments with a second premise of the form 'Patient *i* has Koplik spots at time *t*', and with a conclusion stating that *i* subsequently shows the later manifestations of the measles. An argument of this type is adequate for predictive purposes, but its explanatory adequacy might be questioned. We would not want to say, for example, that *i* had developed high fever and other symptoms of the measles because he had previously had Koplik spots.

Clearly, many cases of a similar kind can easily be found. For example the regular relationship between barometer readings and changes in weather conditions cannot be regarded as providing explanations of the weather, despite their enabling us to make predictions about it. Thus, even if the D-N model adequately represents the necessary conditions for scientific explanation, these conditions are not sufficient.

It might be thought that this objection could be avoided if a suitably restrictive account were given of the law-statements that function as premisses in the deductive argument. In other words, if it were possible to distinguish between explanatory and non-explanatory laws, we could then eliminate the latter group as acceptable explanans-statements. Obviously, any direct appeal to the concept of 'explanatory law' is ruled out on the grounds of circularity, since the D-N model is itself intended to provide the analysis of scientific explanation. A more promising approach might lie in restricting the laws to those which express a causal relationship. For, one reason for rejecting the Koplik-spots example as non-explanatory is that we do not believe these spots are the cause of the fever.

However, this approach faces at least two difficulties. First, Hempel, together with many other positivists, does not wish to restrict the concept of scientific explanation to that of causal explanation. Second, the positivist treatment of causal relations is such that a distinction between causal and non-causal laws is very difficult to draw. This is mainly because of their adoption of a Humean, regularity theory of causation. But we will defer discussion of these points until the next chapter.

More will be said about the positivist account of scientific laws in the following section. But let us now examine the I-S model, since its problems cast further light on those of the D-N model. In the I-S model, we explain some particular event by showing that a statement describing it is supported with a high degree of inductive probability by a set of premisses, at least one of which is a statement of the statistical probability that an event of one kind will be followed by, or associated with, an event of another kind. Suppose, for example, that we are drawing a marble from an urn that contains 1,000 marbles,

one black, and the others white. We draw a white one, and explain this by the high inductive probability of doing so, given that the statistical probability of drawing a white marble from such an urn is 0.999.

The difficulty with such examples, and thus of the model they are based upon, is well expressed by Alan Donagan (1966, p. 133):

In cases of this sort the obvious thing to say is that there is *no* explanation of any individual outcome. You will be deceived into imagining that there is only if you confound what it was reasonable to expect with what has been explained. Reasonable expectations and explanations differ fundamentally. It is more reasonable to expect at the first attempt to toss heads with a coin than to win roulette on a given number; but the grounds why it is more reasonable do not *explain* why you succeeded in tossing heads and failed to win at roulette. After all, you might have won at roulette and tossed tails. *With respect to explanation*, chance situations where the odds are equal do not differ from those where the odds are fifty to one or a thousand to one.

Where the odds are equal, as in tossing a coin, we do not regard getting heads as explained by the 0.5 probability of doing so. Donagan's point is that although the much higher probability of drawing a white marble enables us to make a fairly certain prediction, it does not thereby constitute an explanation. The difference between explanation and non-explanation cannot consist in differing degrees of probability. We can relate this objection to the one that was made against the D-N model in the following way. In both cases, there is a failure to distinguish between providing the grounds for expecting that an event will occur, and explaining why it does occur. The problem of non-explanatory laws in the D-N model is greatly magnified in the I-S model, where, instead of universal laws, we have only statistical probabilities. To accept the I-S model of explanation requires a prior acceptance of the assimilation of predictive and explanatory knowledge that is involved in the D-N model, and more generally in the positivist view of scientific explanation.

2 The positivist view of theories

For the positivist, scientific theories consist of sets of highly general universal statements, whose truth or falsity can be assessed by means of systematic observation and experiment. The results of these observations and experiments can be known either with total certainty, or at least with a far greater degree of certainty than anything else, including the theories which are evaluated by reference to them. The universal statements of scientific theories are usually

termed 'laws', though it is often maintained that there is an important difference between 'theoretical laws' and 'empirical laws', with only the former qualifying as constituents of a theory. This difference itself depends upon a distinction between theoretical (or non-observational) terms and observational (or non-theoretical) terms. We will examine this distinction in the next section. For the moment, we will ignore the difference between theoretical and empirical laws, and concentrate upon the accounts given by positivists of the relations that hold between theories and the results of observations and experiments. (For convenience, we often use the term 'observations' to refer to such results.)

We can begin by noting briefly the various criteria that positivists have offered in their attempts to characterize scientific laws. Though there have been disagreements about these criteria, and their relative importance, there are several which have been widely accepted. First, statements which express laws must have the syntactical form of universal conditions. In simple cases, they will be of the form: 'for all x , if x has the property P , then it has the property Q .' (Symbolically, $(x)(Px \rightarrow Qx)$, which may be read, roughly, as 'All P s are Q s'.) Examples of scientific laws which can be cast in this form are: 'all planets move in elliptical orbits', or 'all bodies subject to no external forces maintain constant velocity'. Second, such statements must not be restricted in their application to any finite region of space and time: they must hold true for all times and places. Thus, 'all humans now in this room are mortal' would not qualify as a law, because of the spatial and temporal restrictions in the statement. Third, none of the terms occurring in scientific laws can refer only to particular, individual items. That is, they must not contain what are, logically, 'names'. This category includes both what are grammatically termed 'proper names', such as the names of individual people, places, or things; and also expressions which necessarily refer to one, and only one, item, such as 'the highest mountain in the world'. Finally, scientific laws do not express any form of necessity, whether this is logical necessity, or what is variously called 'empirical', 'natural', or 'causal' necessity. Thus, it is maintained, laws express non-necessary, or contingent, relationships, whose truth or falsity can only be known by empirical means: they cannot be known by *a priori* argument. Nor can they be regarded as expressing some mysterious non-logical necessity, a necessity which somehow inheres in nature itself: for positivists, as we have already noted, deny the existence of natural necessity.⁴

Given this characterization of scientific laws and theories, how can the positivist ideal of science as giving us genuine explanatory and predictive knowledge be realized? We have seen already, in section 1, the way in which scientific laws, for the positivist, perform their

double function of explanation and prediction. But neither that account, nor the formal characterization of the laws involved in it, are designed to answer the questions of how we can arrive at true scientific theories, or of how we can evaluate the attempts that are made to formulate such theories. It is here that the relationship between theory and observation becomes vitally important, since, for the positivist, it is only observation that can provide an objective foundation for scientific theorizing.

Though some earlier positivists believed that it is possible, by means of observation, to conclusively verify scientific theories, there is an important feature of scientific laws which, as all modern positivists have emphasized, rules this out. No finite amount of observational evidence (and this is all we ever have) can finally establish the truth of a law which is held to apply to all times and places, and whose instances are therefore potentially infinite in number. This difficulty is, in effect, one version of the 'logical problem of induction', of how one can justifiably argue from past events to future events, from the known to the unknown, and so on. In this century, as before, there have been many attempts to solve (or even dissolve) the problem. But what concerns us here are the positivist responses to the following question: assuming that the logical problem of induction means that no conclusive verification of scientific theories is possible, how should one use empirical evidence to evaluate such theories? We can distinguish two main approaches to this, which we will call the 'confirmationist', and the 'falsificationist'.

The confirmationists argue that one can use empirical evidence to provide varying degree of positive support for the truth of scientific theories. For example, the larger the number of instances which are in accordance with the predictions that can be deduced from a theory, or the more varied the circumstances in which these instances occur, the more strongly confirmed is that theory. Similarly, rival theories can be compared as to their relative degrees of empirical support. And many confirmationists have tried to formalize these relationships of evidential support for a theory, and to construct a 'logic of confirmation', which is sometimes based upon the calculus of probabilities.

For the falsificationist, however, all these attempts are futile: there is no logic of confirmation, only of falsification. Observations should be used solely to show that putative theories are false. If we deduce from a theory a prediction that turns out to be incorrect, then it follows logically from this that the theory itself is incorrect. Unlike the confirmationist, the falsificationist restricts the concept of valid argument to that of deductive argument, and insists that the only kind of deductive relationship that can be established between theory and observation is that in which the falsity of a theory follows from

the falsity of the predictions derived from it. Thus, in evaluating scientific theories by means of observations, we can only use the latter to falsify, and not to confirm, the former.

This falsificationist position is often associated with a more general view of the process of theory formulation and evaluation, the 'hypothetico-deductive method', particularly in the work of Karl Popper. Popper argues that one does not first make observations, arrive at a theory by induction from these, and then seek to confirm the theory by further observations. Instead, the scientist begins by formulating a theory, or hypothesis, and proceeds to test the hypothesis by making potentially falsifying observations. If the theory is falsified, it must be abandoned, and another one formulated to replace it. This formulation of hypotheses is purely a matter of conjecture: there is no 'logic of discovery' by which we can arrive at theories from observations. Indeed, Popper claims that it makes no sense simply to 'observe', without reference to any hypothesis which is being tested. For without such a theory, one does not know what to look for.

Thus the hypothetico-deductive method provides an account both of the way in which the scientist is to arrive at theories, and also of the way these hypotheses are to be evaluated by empirical evidence. But it is important to realize that there is no inconsistency in accepting only one part of this account, whilst rejecting the other. For example, a confirmationist could agree that theory formulation is a matter of conjecture, and not of induction from observations; yet still insist that one should try to accumulate positive support for theories, and that it is possible to measure the degree of confirmation that theories have with respect to this empirical evidence. Similarly, the falsificationist view of theory evaluation could be combined with the claim that one arrives at theories by some process of inductive argument from observations. The hypothetico-deductive method does not have to be either accepted or rejected *in toto*.⁵

But although there are these differences between positivist philosophers of science concerning the relationship between theories and observations, the assumptions that they share are more significant than their disagreements. Two such assumptions will prove especially important in the following two chapters, where we consider the realist and conventionalist positions. First, that the theories which are either confirmed, or falsified, are universal statements about regular, contingent relationships in nature. Second, that the observations used to evaluate such theories provide an objective foundation for science. In particular, the truth or falsity of statements about observations is not dependent upon the truth or falsity of theories; and theoretical disputes can be resolved by reference to agreed observation-statements.

3 Theoretical and observational terms

So far, we have discussed the positivist treatment of the relations between theory and observation, in terms of the various responses to the problems posed by the universality and generality of scientific laws. In other words, the distinction between theory and observation has been construed simply as one between universal statements, or laws, and particular statements, that is, statements about observations. However, as we noted at the beginning of section 2, many positivists wish to reserve the title of 'theory' to a special class of such laws, namely 'theoretical laws'. For although science has, as its objective, the explanation and prediction of observable phenomena, many of the terms which actually occur in scientific theories do not, in any obvious way, refer to such phenomena. These terms are usually described as 'theoretical', and the laws containing them, 'theoretical laws'. For reasons which will emerge shortly, this fact about scientific theories has seemed highly problematic to the positivist. Consider the following quotation from Hempel (1958, p. 177):

Scientific systematization is ultimately aimed at establishing explanatory and predictive order among the bewilderingly complex 'data' of our experience, the phenomena that can be 'directly observed' by us. It is a remarkable fact, therefore, that the greatest advances in scientific systematization have not been accomplished by means of laws referring explicitly to *observables*, i.e. to things and events which are ascertainable by direct observation, but rather by means of laws that speak of various *hypothetical*, or *theoretical*, *entities*, i.e. presumptive objects, events, and attributes which cannot be perceived or otherwise directly observed by us.

Hempel proceeds to give some examples of such hypothetical, or theoretical entities: electric, magnetic and gravitational fields; molecules, atoms, and sub-atomic particles. Other examples which are often classified as 'theoretical' are: genes, viruses, kinetic energy, and electrical resistance.

What is particularly interesting about the quotation from Hempel is the beginning of the second sentence, 'It is a remarkable fact . . .'. For this fact would hardly be described as remarkable by the realist. Indeed, it is just what one would expect, since for the realist, one objective of science is to discover the often unobservable structures and mechanisms which causally generate the observable phenomena. Thus the realist is perfectly prepared to regard terms such as 'electron' or 'molecule' as referring to real entities in the world, in much the same way as non-theoretical terms do – such as 'iron', 'wood', 'red', etc.

Why then should the occurrence of theoretical terms in scientific theories be surprising and problematic for the positivist? The answer lies in the consequences that positivists have believed to follow from their general attempts to distinguish science from other intellectual activities, such as metaphysics, theology, or ethics. For the positivist, a statement can only be properly regarded as scientific if it is possible to ascertain its truth or falsity by means of empirical observation. But statements containing theoretical terms, if these terms are construed as making reference to entities which cannot be observed, appear not to meet this criterion of scientificity: thus they must somehow be understood in a different way.

Clearly, much depends here on how stringently this criterion of scientificity is interpreted. In the twentieth century, both positivists and their opponents have paid considerable attention to the problems of formulating such a criterion. Confirmationists and falsificationists have disagreed about whether the criterion should be stated in terms of the possibility of verifying (or confirming), or of falsifying the statements concerned; the concept of 'possibility' involved in the criterion has proved difficult to elucidate; and the degree of directness with which observations are required either to verify or falsify statements has been much disputed. Furthermore, what we have described as a criterion of scientificity has often been regarded as a criterion of meaningfulness. Thus, instead of the contrast being drawn between science and non-science, it has frequently been seen as one between the meaningful and the meaningless.⁶

However, we do not wish to engage with these issues here. It is sufficient to note that positivist philosophers of science have both adopted a criterion of the kind we have mentioned, and regarded it as implying that theoretical statements are genuinely scientific only if they are construed in a way that does not involve ontological commitments to unobservable entities. That is, they have tended to reject the view that theoretical terms make reference to actually existing, yet unobservable, items in the world.

Several different strategies have been adopted by positivists in their attempts to deal with what is often called the 'problem of theoretical terms'. Most of these have involved the construction of an exclusive and exhaustive dichotomy between two 'languages' or sets of terms: theoretical (or non-observational) and observational (or non-theoretical). And a frequent device has been the formulation of 'correspondence rules', through which definitions can be given to theoretical terms by means of statements containing only observational terms.⁷ In examining these strategies, we will proceed in the following way. First, we will comment on some characteristics of the theoretical-observational dichotomy. Next, we will outline two of the forms of correspondence rule that have been advocated. Finally,

we will note some of the criticisms that have been made of the dichotomy, and of the way it has been used. Other, more fundamental criticisms, will be raised in our discussion of realist and conventionalist views in the next two chapters.

The dichotomy between theoretical and observational terms (and thus between the two 'languages' which contain these terms) has typically been constructed by giving a positive characterization of the observational, and then defining the theoretical as whatever is not observational. An observational term is one which refers to what can be observed, and whose meaning can thus, in many cases, be defined ostensively. Thus statements containing only observational terms satisfy the positivists' criterion of scientificity, since their truth or falsity can be determined by means of observation. The precise content of the dichotomy depends, however, on the way in which 'observation' or 'observable' are defined: here, many different accounts have been given. Sometimes, only the direct perceptual experience of an individual observer is allowed the status of observation. But this definition generates severe problems for the intersubjective agreement amongst different observers which is essential to the positivist ideal of science as objectively controlled by observation. Alternatively, the physical operations involved in measurement and experimental procedures may be invoked as constituting the observable; or the material objects and properties that we commonly encounter and refer to in our everyday life. Another approach, often called the 'pragmatic' theory of observation, is to define as observational all those terms whose correct or incorrect application can be agreed upon by the scientists concerned. Finally, in most of these definitions, considerable significance is attached to the measurability of the observable, to the possibility of quantification: indeed, the difficulties of appealing to direct perceptual experience are partly due to its failure to meet this requirement.⁸

But despite these differences about what is to count as observable, the resulting observation-languages have several common characteristics, which are of central importance to positivist philosophy of science. We can usefully summarize these by saying that the observational language has been regarded by positivists as both *epistemologically and ontologically privileged*. By 'epistemologically privileged' we mean this. First, that the truth or falsity of statements containing only observational terms can be known either with total certainty, or at least with a far greater degree of certainty than those containing non-observational, or theoretical, terms. Second, that observational statements can be verified or falsified without reference to the truth or falsity of theoretical statements, that is, those containing at least some non-observational terms. This second element of epistemological privilege is often termed 'theory-neutrality', and has been attacked

by some of the philosophers of science whose views we will examine in chapter 3. By 'ontologically privileged', we mean that terms belonging to the observation-language, and only these terms, can be regarded as making genuine reference to items in the physical world. Only such items can properly be said to exist. Thus it is these items that scientific theories attempt to explain and predict; that provide the basis of our empirical testing of such explanations and predictions; and which, in some sense, our theories are about.

We turn now to the positivist use of correspondence rules. It is by means of such rules that many positivists have attempted to deal with the problem of theoretical terms. Ideally, they have hoped to provide definitions of theoretical terms in the observation language, by linking the former, via correspondence rules, with statements in the latter. By doing this, the epistemologically inferior status of theoretical statements would be remedied, their apparent commitments to unobservable ontologies avoided, and their lack of scientificity made good. Several different types of correspondence rule have been formulated. For various technical reasons, which we will not discuss here, none of them has proved entirely successful. Furthermore, they derive their general rationale from a philosophical theory of meaning, the 'verification theory', which, as we will see in the next chapter, is rejected by the realist. But we will outline two simple types of correspondence rule, in order to give some idea of the procedures that are involved.

Let us consider the theoretical term 'magnetic' (theoretical according to most positivist accounts of what is observable). The first type of correspondence rule, used by advocates of what is called 'operational definition', might be formulated in the following way: 'for any object, x , x is magnetic if and only if, whenever a small piece of iron is placed near it, the piece of iron moves towards it'. More generally, we could say: ' x has the theoretical property Q if and only if, when x is subjected to test conditions C , it manifests the response R '. (Symbolically: $Qx \equiv (Cx \rightarrow Rx)$.) Such operational definitions would, if successful, enable the positivist to replace all theoretical terms and statements by observational ones. However, for technical reasons, these definitions also generate highly unwelcome consequences – such as, that any object is magnetic whilst no piece of iron is near it – which soon led to their abandonment by positivists. (Unfortunately, for a long time after this, the ideal of operational definition was pursued enthusiastically by many social scientists.)

In their place, a second type of correspondence rule was proposed, involving what is often called the method of 'partial interpretation', in which the meaning of the theoretical term is only partly specified. A simple example of such a rule for 'magnetic' would be: 'if a piece of iron is placed near x , then x is magnetic if and only if the piece of iron

moves towards it'. More generally, we have: 'in test conditions C , x has the theoretical property Q if and only if it manifests the response R '. (Symbolically: $Cx \rightarrow (Qx \equiv Rx)$.) Thus what we get here is a partial specification of the meaning of theoretical terms, that is, the meaning they have in certain test situations. Further specifications can then be given, by correspondence rules which give the meanings of the terms in different situations.

There are several difficulties with this account, which we will not mention here.⁹ However, the main point is that the success of this second type of correspondence rule depends partly on whether the list of rules for a particular theoretical term is finite in number. If not, as seems likely, then it will not be possible, even in principle, to eliminate theoretical terms from science, and replace them by observational terms. And if this is impossible, it might be taken to indicate that it is also impossible to avoid the ontological commitments of theoretical terms, and to maintain the ontologically privileged status of the observation-language. Alternatively, it might be taken to provide a rationale for what is called the 'instrumentalist' view of science, in which theoretical statements are regarded not as true or false, but as computational devices for the generation of successful predictions about observables. We will return to this view at the end of chapter 3; whilst in chapter 2, we will consider a realist interpretation of the positivists' correspondence rules. But let us now examine some problems of the theoretical-observational dichotomy itself.

There are many different distinctions that can be drawn between the observable and the unobservable, and also between the theoretical and the non-theoretical. Each of these is of some importance in different contexts, but there is no one distinction that has priority over all the others, and which can be used to perform the several different functions which positivists have assigned to the theoretical-observational dichotomy. Further, it is difficult to state these distinctions as holding between two sets of *terms*, since the status of any one term may change historically, or vary from one situation in which it is applied to another.

Consider, for example, the term 'virus'. If one allows as observable anything that can be detected either by the senses or by instruments, then it is now an observational term (since viruses can be observed by use of an electron microscope), but earlier it was not. A similar historical change of status results from the application of the pragmatic theory of observation, mentioned earlier. Alternatively, if the observable is defined as that which can be detected without the use of instruments, 'virus' remains non-observational. But is it a *theoretical* term? If one distinguishes between terms that are central to a developed scientific theory, and those that are not, then 'virus' is

now theoretical, but not earlier on. However, if 'theoretical' is taken to mean 'speculative' or 'purely hypothetical', then viruses, whose existence is now well-confirmed, were once theoretical, but now non-theoretical.

Even at any one time, the status of a particular term with respect to such distinctions is often problematic. Suppose we define as observable that which does not require the use of instruments for its detection. Consider the term 'moon'. Sometimes, as with the Earth's moon, the term is applied as a result of observations with the naked eye: but for most other moons, it is not. Somewhat similarly, one can often detect the presence of an electrical charge without the use of instruments; but to make precise quantitative measurements, instruments are necessary. So are the terms 'moon' and 'electrical charge' to be counted as observational or non-observational?¹⁰

It seems that the basic error of positivist philosophers of science has been to use a single dichotomy to resolve several different issues. First, there is the problem of finding some epistemological foundation for scientific theories, of distinguishing between the degrees of certainty with which various kinds of statement can be affirmed or denied, and thus of giving some account of the 'objective control' of science. Second, there is the problem of finding some way of assessing the relative merits of competing scientific theories, which themselves often provide different ways of characterizing the results of experiments and observations; of constructing some theory-natural ^{neutral} observation language. Finally, there is the ontological issue, the question of determining in general what sorts of items can properly be said to exist, and to enter into causal relations with each other. In the following two chapters, we will be returning to these problems, and the relations between them.

4 Positivism and 'the logic of science'

So far, we have been examining the specific claims made by positivists about the nature of scientific explanation, the relations between theory and observation, and so on. We will now make some comments about the way in which positivists have conceived of the philosophy of science itself, and of the kinds of results it can establish. 'The logic of science' is a phrase which has often been used to represent this conception of the philosophy of science, and it will be convenient to organize our remarks around its various connotations.

We have noted the reliance of positivists upon the techniques and concepts of formal logic. Thus, explanation is analysed as a form of logical argument; theoretical terms are given definitions through formal correspondence rules; and attempts are made to specify the

logical relationships involved in the testing of theories. These features alone justify the use of 'the logic of science' as an appropriate description of the way in which positivists have regarded and carried out their enterprise.¹¹ But this phrase may also be used to contrast what the positivist sees as the *philosophical* approach to science, with various others, such as the historical, psychological or sociological. Generally, positivists have either been uninterested in these, or, at least, convinced that they have no great relevance to the philosophy of science, which is concerned only with its 'logic'. Thus, the general characterization of the historical development of science, or the psychological processes involved in the theoretical activity of scientists, or the organization of the scientific community and its relations to other aspects of society: all these have been regarded as forms of enquiry quite different from that of the logic of science, and their results as strictly irrelevant to it. This claim of non-relevance may itself be supported by reference to some version of a general distinction between prescription and description. So the positivist conceives of the logic of science as dealing with questions of rational evaluation and justification, in contrast to, say, the sociologist's concern with the empirical description and explanation of scientific activity.

Let us consider two of these contrasts – between the logic of science and, respectively, its psychology and history – in more detail. The first of these is well illustrated in the positivist attitude towards the role of models and analogies in science. An undoubted feature of scientific activity is the way in which many theories are presented by means of analogies with relatively familiar objects and processes. For example, Huygens justified his use of the term 'waves' in his theory of light and sound in the following manner (quoted in Achinstein, 1968, p. 204):

I call them waves from their resemblance to those which are seen to be formed in water when a stone is thrown into it, and which present a successive spreading as circles, though these arise from another cause, and are only in a flat surface.

Such analogies, and the models which are based upon them (in this case, the 'wave model' of light and sound), are typically regarded by positivists as of only psychological interest. They are thus seen as 'heuristic aids': either to the scientist who first formulates the theory, by some act of creative imagination, or to laymen (or other scientists), who find it psychologically helpful in understanding a theory to have it presented in terms of models or analogies. But such features of scientific activity are irrelevant to the logic of science, which is not concerned with these psychological processes.¹²

The contrast between the logic of science and its history is more complex. Most positivists reject the view that, somehow, a correct philosophy of science can be derived from a study of its history; or that philosophical claims about science can be confirmed or refuted by the fact that scientists have, or have not, proceeded in this way historically. However, many positivists have been interested in producing a 'rational reconstruction' of the history of science, in which various episodes are appropriately re-described in terms of the vocabulary and doctrines of the logic of science. Indeed, this may take the form of a very general characterization of the way in which science develops historically. Here we find two main accounts, which bear close affinities with some of the views about the relations between theory and observation discussed in section 2. First, an inductivist account, according to which science can be seen as a process of steady accumulation, in which more and more facts are discovered, with increasing accuracy, and theories become more general and universal in their scope. Second, a hypothetico-deductive account, in which science develops by the successive formulation and rejection of theories, and is thus neither steady nor cumulative in its progress. The history of science is the history of the abandonment of hypotheses which failed to meet the tests of observation and experiment. Nonetheless, there is a sense in which progress is made: although one can never be sure that present theories are correct, they are at least closer to the truth than those which have already been falsified.¹³

Whichever of these two accounts is adopted, positivists tend to characterize the historical development of human knowledge as a process in which various types of pre-scientific explanations of natural phenomena are eventually replaced by properly scientific ones. This view is often supported by reference to a number of 'famous episodes', where the scientific attitude is seen as finally triumphing over theological, animistic or metaphysical explanations. One such example, from eighteenth-century chemistry, is the replacement of the *phlogiston* theory of combustion, by Lavoisier's *oxygen* theory. Positivists view *phlogiston* as an unscientific concept, which neither belongs to, nor is definable within, the observational language of science. We will now outline these two theories, since the nature of the historical conflict between their proponents raises issues that will be significant in later chapters.

Priestley, a supporter of the *phlogiston* theory, argued that the combustion of substances such as metals or sulphur was due to the *phlogiston* present in them being released and absorbed into the air, leaving behind metal calxes, or sulphuric acid. Thus: combustible substance - *phlogiston* = burnt substance. When calxes or acids were heated, with carbon, they absorbed the *phlogiston* contained in

the carbon, and the original substances were regenerated: burnt substance + *phlogiston* = combustible substance. Against this, Lavoisier argued that, during combustion, metals or sulphur combined with oxygen from the air. For example: sulphur + oxygen = sulphuric acid. Conversely, burnt substance - oxygen (withdrawn by the carbon) = combustible substance. We should also note that what Lavoisier came to call 'oxygen' had earlier been discovered by Priestley, by heating mercury calx. But Priestley called this gas 'dephlogisticated air', because he believed its properties, such as supporting rapid combustion, were due to its containing less *phlogiston* than ordinary air, thus being able to absorb more *phlogiston* during combustion.¹⁴

Let us now consider another feature of positivism associated with the phrase 'the logic of science'. This is the idea that the philosophy of science involves an analysis of science which is not directly dependent upon the actual and varying specific contents of different scientific theories, but which is conducted at a 'higher' or 'meta-' level. Furthermore, it is often believed that by analysing science in this way, we can discover various abstract, universal and objective criteria to which any actual scientific theory or explanation must conform. Such criteria can also be appealed to as external, rational standards to resolve the disputes between proponents of rival scientific theories or explanations that may occur, and have occurred, in the history of science.

Finally, and closely connected to this last feature, there is an underlying claim that there is only *one* logic of science, to which any intellectual activity aspiring to the title of 'science' must conform. Thus, the so-called 'social sciences' must do this, if they are to deserve their title: and many positivists have argued that, though at present 'immature', they can in fact do so. This claim, which we have earlier referred to as 'naturalism', has been a characteristic feature of positivism, both in this century, and before - so much so that the terms 'positivism' and 'naturalism' have come to be used interchangeably. We have already argued that such a usage is highly misleading. But it is worth adding here, that this belief in the methodological unity of the sciences has itself been only one aspect of a more general commitment to what positivists have termed 'the unity of science'. This latter ideal has involved, in addition to methodological unity, a substantive unity, which is to be achieved by the systematic reduction of all sciences to one basic science, usually physics. Such a reduction (or rather, a series of reductions) can be achieved either by defining the concepts of one science (say, biology) in terms of those of another science (say, chemistry), or by deriving the laws and theories of the former from those of the latter. Thus, ideally, we would have a hierarchy of sciences, beginning with

physics, and proceeding through chemistry, biology, psychology and sociology, in which all are 'reduced' to the first.¹⁵

We will not examine this ideal any further here. Instead, we turn in the next chapter to an analysis of one of the alternative conceptions of the natural sciences – realism.

2 Realist philosophy of science

1 The realist view of explanation

We argued earlier that the main difficulty for the positivist account of scientific explanation arises from the existence of logical arguments which, though satisfying the specified conditions, enable us only to predict, and not to explain, the occurrence of particular events. For the realist, this difficulty is highly significant, since it indicates a basic inadequacy of the positivist account. The inadequacy consists in confusing the provision of grounds for expecting an event to occur, with giving a causal explanation of why that event occurred. The premisses which, for the positivist, constitute the explanans may often be such that they only give us good, or conclusive, reasons for believing that the explanandum-event either will or did occur. They do not necessarily tell us *why* that event did or will occur.¹

For the realist, this problem results from analysing scientific explanation as consisting of a form of logical argument. Even if it is a necessary condition of explanation that such arguments can be constructed, it is not a sufficient one. What is additionally required is the delineation of the causal processes involved in the generation of the phenomena we are trying to explain. Instead of establishing, as in the D-N model, relations of logical necessity between the explanans and explanandum statements, we should discover the relations of natural necessity that exist in the physical world. To show that the truth of the conclusion follows necessarily from that of the premisses is no substitute for describing the necessary, causal connections between things in nature.

But this use of the concept of natural, or causal, necessity has long been rejected by positivists, who have regarded Hume's objections to it as decisive. Hume accepted that our everyday pre-philosophical concept of causation involves both the idea of the cause immediately preceding its effect, and also some idea of a necessary connection

between the two. But the crucial question for Hume was what kind of necessity this could be. Not logical necessity, since it is impossible to deduce, from a statement asserting the occurrence of one event, a statement asserting the later occurrence of another event, which we may happen to regard as the effect of the first one. Thus it is not self-contradictory to affirm the truth of the first statement, whilst denying that of the second. Consider, for example, the two events 'striking a match' and 'the match lighting'. Although we may in fact believe that the former is the cause of the latter, whatever necessity we ascribe to the relation between them cannot be logical necessity, since it is not self-contradictory both to affirm that the match was struck, and to deny that it lit. Further, Hume argued that the idea of necessity cannot be derived from observation: there is no non-logical necessity which somehow exists in nature. However closely and systematically we observe any two events that we believe to be cause and effect, we can never discover any necessary link or connection between them. Thus, in the case of one billiard ball's being said to cause another to move, there is nothing to be observed but the one movement immediately followed by the other.

Nonetheless, it is also possible to learn by observation that a movement of the first kind is regularly followed by one of the second kind. And this indicates the nature of Hume's positive account of causal relations. To say that one event is the cause of another is to say that the first is temporally prior to the second, and that whenever an event of the same type as the first occurs, it is always followed by one of the same type as the second.²

This Humean view of causation, the 'regularity theory', provides the basis of the positivist analysis of scientific explanation. The statements expressing laws, which constitute one part of the explanans in the D-N model, are essentially statements of Humean regularities. They are not logically necessary statements: as we noted in our outline of the positivist view of scientific laws, they are purely empirical, universal conditional statements. The logical necessity in the D-N model is located in the relation between the explanans (premisses) and the explanandum (conclusion). Further, we can now see one reason why the positivist is unlikely to try to avoid the problem of non-explanatory predictions by restricting the law-statements in this model to causal laws. For, on a regularity theory of causation, many of the cases of non-explanatory regularities will qualify as causal relations: it is difficult to distinguish between causal and non-causal laws in a way relevant to this problem.³

So the realist objection to the positivist account of explanation rests upon a rejection of the Humean view of causation that provides its fundamental rationale. The realist wishes to substitute an alternative account of explanation based upon a non-Humean view of

causal relations. This involves both a criticism of Hume's position, and the construction of a viable alternative. One approach⁴ to these can be introduced by considering the comments made by a realist philosopher of science, Rom Harré, on the following passage from the *Enquiries*, where Hume (1902, p. 77) gives an example of the application of his account of causation:

We say, for instance, that the vibration of this string is the cause of this particular sound. But what do we mean by that affirmation? We . . . mean that *this vibration is followed by this sound, and that all similar vibrations have been followed by similar sounds.* . . .

Harré comments (1970, pp. 105-6):

This, Hume contends, must be the correct analysis since we can form no idea of the connection between the vibration and the sound. But the theory and experiments of sonic physics and neuro-physiology give us a very good idea of the connection between the vibration and the sound. We all know nowadays of the train of pressures in the air, the operation of the ear-drum, the cochlea, and so on, and we now know something of the train of electrochemical happenings between the inner ear and that part of the brain identified as the seat of audition. Furthermore, to explain what we mean by 'the vibration causes the sound', rather than something else, typically involves, I contend, reference to the intervening mechanism which links the vibration in the string to the sound we hear. The vibration of the string stimulates a mechanism which then acts in such a way that we are stimulated and hear a sound.

More generally, Harré argues that to view causal relations as consisting only of temporal precedence and regular succession, is to fail to distinguish between the *meaning* of statements asserting such relations, and one kind of *evidence* upon which they may be based. If we discover a regular relationship between two kinds of phenomena, this gives us some reason to believe that they are causally connected: the existence of the regularity is strong, though not conclusive, evidence for a causal connection. But this is not all we mean in claiming that one thing is the cause of another. In addition, we commit ourselves to the presence of some intervening mechanism which links them together, and it is the scientist's task to discover and analyse the nature of such mechanisms. Thus, in the example about the string-vibration and sound, Harré would maintain that the regular relation between these may provide evidence for the existence of a causal relation. But in saying that there is a causal relation, we mean also that there is some intervening mechanism

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which, in this instance, is described by means of the theories of sonic physics and neuro-physiology.⁵

This claim about the relation between evidence and meaning in the case of causal statements implicitly involves the rejection of a general theory of meaning often supported by positivist philosophers of science. According to this 'verificationist' theory, the meaning of any empirical statement is given by its actual or possible means of verification – that is, by observational evidence, such as Humean regularities. As we will see in section 3, the rejection of this theory is also involved in the realist view of positivist correspondence rules. And in the final section, we will consider some objections to these criticisms of Hume. But let us now look in more detail at the realist alternative to the regularity theory of causation and the positivist view of scientific explanation.

For the realist, adequate causal explanations require the discovery both of regular relations between phenomena, and of some kind of mechanism that links them. So, in explaining any particular phenomenon, we must not only make reference to those events which initiate the process of change: we must also give a description of that process itself. To do this, we need knowledge of the underlying mechanisms and structures that are present, and of the manner in which they generate or produce the phenomenon we are trying to explain. In describing these mechanisms and structures we will often, in effect, be characterizing the 'nature', 'essence', or 'inner constitution' of various types of entity. Suppose we wish to explain an increase in temperature of some particular gas. There are well-established laws relating the temperature, volume and pressure of gases (at least in 'ideal' conditions). Thus we can, with this knowledge, show any particular temperature increase to be an instance of these laws; and we can thereby explain this phenomenon in a manner conforming to the D-N model. But for the realist, a satisfactory causal explanation requires more than this. We need to know the mechanisms at work: and to do so, we must discover the nature or constitution of gases. This is given by the molecular theory of gases, according to which they are composed of a vast number of molecules, which interact in a manner that explains the regular relations represented by the gas laws.

It might, however, be objected by the positivist that this example can easily be accommodated within the framework of the D-N model. The laws relating temperature, volume and pressure can be seen as 'lower level' laws, derivable from, and thus explicable by, the 'higher level' laws of molecular interaction. The latter laws are more general in their scope than the lower level ones, and also contain theoretical terms. But there are several replies to this objection. First, the positivist explanation outlined here is, strictly speaking, of the

lower level laws, and not of the particular phenomenon which is the explanandum in our example. For the positivist, this phenomenon is adequately explained by showing it to be an instance of the lower level laws. This the realist denies. Second, the positivist must view the explanatory power of the molecular theory as stemming only from its greater scope and generality, which enable the lower level laws (and, less directly, particular phenomena) to be deduced from it. But the realist regards its explanatory power as due to its description of the nature, or essence, of gas, thereby providing the kind of account of mechanisms and structures necessary for adequate causal explanation. Finally, by their rejection of the ontological commitments of theoretical terms, positivists eliminate this central feature of the molecular theory's explanatory power.

The realist view of explanation can be conveniently summarized in the claim that answers to why-questions (that is, to requests for causal explanations) require answers to how- and what-questions. Thus, if asked *why* something occurs, we must show *how* some event or change brings about a new state of affairs, by describing the way in which the structures and mechanisms that are present respond to the initial change. To do this, it is necessary to discover *what* the entities involved are: to discover their natures or essences. It is sometimes said that science cannot tell us why things happen, but only how; or, that science is concerned only with description, and not with explanation. But the realist rejects the contrasts implicit in such claims. For, to explain why is partly to say how; and causal explanation itself requires descriptions.⁶

We conclude this account of the realist view of explanation by considering one of the positivist developments of Hume's theory of causal relations, due mainly to J. S. Mill. Let us return to an earlier example, striking a match as the cause of its lighting. If Hume's theory is applied to this, the following problem arises. Although we may talk of the former event causing the latter, we would be unwilling to claim that, whenever an event of the first kind occurs, it is always followed by an event of the second kind. For this regularity only holds given the existence of various other conditions, such as the chemical nature of the match-head and striking-surface, the degree of force with which the match is struck, the presence of oxygen in the atmosphere, and so on. Thus the striking of the match is only one of the causes of its lighting, and not the whole or complete cause. The complete cause must be such that, whenever it recurs, an event of the same kind will follow.

For Mill, then, the cause of any event must be a set of conditions or factors which, taken together, are a sufficient condition for it. No one of these conditions is, by itself, sufficient: each may be termed 'a cause', or 'a causal condition', in contrast to the complete cause,

which is composed of a conjunction of these conditions. Thus sets of conditions replace Hume's single events. But this difference is of little significance to the realist, as can be seen from the following two objections from a realist standpoint. First, causal relations interpreted as sufficient condition relations are still basically Humean regularities. For no element of necessity is involved in the relation between a sufficient set of conditions and the phenomenon of which they are the cause. Second, the realist finds problematic the manner in which different types of causal conditions are somehow combined or conjoined to constitute the complete cause. The term 'condition' is used by Mill to refer, indifferently, to events, states of affairs, processes, and so on: almost anything can turn up in a list of causal conditions. Indeed, in the example used earlier (admittedly our own, but conforming to the general spirit of Mill's account), two of these items – the chemical composition of the match-head and striking-surface – are of a similar kind to those which would typically occur as the structural component of a realist explanation. But for such items to be simply listed, together with others of very different kinds – the striking of the match, and the presence of oxygen – as jointly forming a sufficient set, is, for the realist, misleading and mysterious. These sets are typically presented as logical conjunctions of discrete conditions, as in '(A & B & C)': but what kind of combination of such disparate items could this possibly represent?'

2 The realist view of theories and models

For the realist, the primary purpose of scientific theories is to enable us to give causal explanations of observable phenomena, and of the regular relations that exist between them. Further, such explanations must make reference to the underlying structures and mechanisms which are involved in the causal processes. It is these structures and mechanisms which it is the task of theories to describe. Thus, the central feature of a scientific theory is its description of these items, and of the way in which they operate to generate the various phenomena that we wish to explain.

This brief characterization of the realist position can best be expanded by considering the function of models and analogies in theories. We noted in the previous chapter that many scientific theories are based upon, or involve some reference to, models and analogies that link those theories to other types of phenomena, or other areas of scientific and common-sense knowledge. Stated in this vague and general way, this is a claim that both positivists and realists can agree upon. But, as we will see, they disagree significantly about more precise and specific ways of interpreting it. Let us begin by giving some examples of models and analogies, ignoring for the

moment the differences between these two concepts. In the following passage, Peter Achinstein (1968, pp. 203–4) presents some typical scientific analogies, accompanied by illustrative quotations:

- 1 The analogy between an atom and a solar system, in which the nucleus is likened to a sun and the electrons to planets revolving about it in elliptical orbits:
'The alternative [to a harmonic model of the atom] was to copy the motion of the planets around the sun. The reason planets do not fall into the sun is that they have reached stable orbits in which the centripetal force required to constrain them is exactly the force of gravitation pulling them in . . . Similarly in the atom, a revolving electron if moving fast enough would not fall into the positively charged nucleus.'
- 2 The analogy drawn by Huygens between waves of light, sound, and water:
'I call them [light and sound] waves from their resemblance to those which are seen to be formed in water when a stone is thrown into it, and which present a successive spreading as circles, though these arise from another cause, and are only on a flat surface.'
- 3 The analogy between a gas and a container of billiard balls, in which the molecules in the gas are likened to perfectly elastic billiard balls striking the sides of the container as well as each other.

We can use these examples to give some indication of the relationship between analogies and models. In the case of 1, we might talk of the solar model of the atom, based upon an analogy with the solar system; in 2, of the wave model of light and sound, based upon an analogy with the behaviour of water; and in 3, of the molecular model of gases, based upon an analogy with billiard balls in a container. We can also draw distinctions, which are important from the realist standpoint, between a *model*, the *source* of a model, and the *subject* of a model. Roughly, a model is an attempted representation of the nature of that which is the subject of the model; it is analogically related to its source, which is an already understood phenomenon. In other words, models are *of* a subject, and modelled *on* a source. Thus, in 1, the subject of the model is the structure of the atom; the source of the model is the solar system; and the model is a representation of this subject, analogically related to the source. Likewise, with the other examples. Further, we can use these distinctions to draw attention to the diverse ways in which well-known models in science are often named. Sometimes, as with 'The Bohr model of the atom', the name derives simply from the model's author. Or, as with 'the wave model of light', it derives from the basic feature ascribed by

the model to its subject. Finally, as with 'the billiard ball model of gases', the name derives from the model's source; though, in this case, we have an alternative name, 'the molecular model', deriving from its subject.

In this account of models, the concepts of analogy and representation are clearly important. In general, to claim that there is an analogy between two items is to claim that there are both similarities and dissimilarities between their various features, and that the similarities enable us to understand one of the items by means of our knowledge of the other. The relevant features may range from the straightforwardly observable, such as size, to the more 'abstract', such as conformity to the same laws. Thus, in the billiard ball model of gas, the features ascribed by that model to the constituents of gas differ from those of billiard balls with respect to colour and size, but are similar with respect to shape and mode of interaction.

The concept of representation is more problematic. It should be noted that the expression 'representational model' is sometimes used to refer to three-dimensional physical objects (or two-dimensional diagrams), such as tinkertoy models of molecules, or engineering models of dams and aeroplanes. Such objects are frequently employed in science, but they involve different senses of the terms 'model' and 'representation' to those so far considered: we will exclude these different senses in what follows.⁸ Basically, the realist regards a model as an attempt to *describe* structures and mechanisms which are often unavailable to observation, even with the use of scientific instruments. Thus the relationships between the descriptive terms of a model and the characteristics of its subject are, in principle, the same as those between any such terms and the items to which they refer. However, the description or representation provided by a model typically involves various forms of abstraction and idealization of the actual features of its subject. For example, the billiard ball model abstracts from these features in omitting reference to the attractive and repulsive intermolecular forces. It also presents some of them in an idealized form, in ascribing 'perfect elasticity' to the constituents of gases, a property which is possessed by no actual object in the physical world. Nonetheless, it would be wrong to regard abstraction and idealization as peculiar to models, or to any attempt to describe unobservable entities. They are present at every level of scientific theorizing, as is shown by the use of such concepts as the perfectly frictionless surface, an idealization which need not refer to unobservable entities, nor be part of a model.⁹

Let us now consider the way in which, for the realist, models and analogies function in the processes of theory-formulation and testing. In order to explain observable phenomena, and the regularities that obtain between them, scientists must attempt to discover appropriate

structures and mechanisms. Since these will typically be unavailable to observation, we first construct a model of them, often drawing upon an already familiar source. The model is such that, were it to represent correctly these structures and mechanisms, the phenomena would then be causally explained. Having constructed the model, we proceed to test it as a hypothetical description of actually existing entities and their relations. To do so, we work out further consequences of the model (that is, additional to the phenomena we are trying to explain), that can be stated in a manner open to empirical testing. If these tests are successful, this gives good reason to believe in the existence of these structures and mechanisms, the *initial* reason for this belief being that, were they to exist, the original phenomena would be adequately explained. Further, it may prove possible to obtain more direct confirmation of these existential claims, by the development and use of suitable instruments. Finally, the whole process model-building may then be repeated, in order to explain the structures and mechanisms already discovered.¹⁰

How does this account compare with some of the positivist views of theory-formulation and testing that we outlined in section 2 of the previous chapter? Clearly, the realist must reject the idea that we arrive at theories by an inductive procedure, by moving from particular observations to generalizations about these observed phenomena. Whatever the realist may think about the logical problem of induction, such inductive arguments can never justify the postulation of unobservable entities. The move of generalization, from 'some' to 'all', is not a move from observables to the unobservable structures and mechanisms which explain them.

The hypothetico-deductive method, however, might at first seem closer to the realist view. Models could be regarded as hypotheses, which are tested by deducing their observable consequences. To some extent, the realist can accept this, but with important qualifications. First, whereas the hypothetico-deductivist claims that hypotheses are purely conjectures, which 'come from nowhere', the realist emphasizes the frequency with which some form of analogical argument, from source to model, may be employed. Second, the realist's hypotheses are about underlying structures and mechanisms, whereas the positivist's hypotheses are, at best, theoretical laws, whose theoretical terms are not interpreted as involving ontological commitments. Indeed, many of the hypotheses introduced by realists are of an existential kind. That is, they postulate the existence of entities that have not been observed, and may not be open to any available method of detection. Examples of such existential hypotheses are the postulation of the existence of viruses, of various sub-atomic particles, or of magnetic fields. Thus, to describe the realist view of theories as involving use of the hypothetico-deductive

method is misleading if the nature of the hypotheses, and their differences from those advocated by positivists, is not fully recognized.¹¹

Let us conclude by contrasting the attitude of positivists and realists to the function of models and analogies in scientific theories. There is an orthodox view of the contrast that should be rejected. According to this, the positivist regards their function as purely heuristic: models and analogies are psychologically helpful in arriving at, and presenting, scientific theories, but their significance is limited to this. Whereas the realist sees them as an essential feature of theories, enabling us intelligibly to represent unobservable structures and mechanisms. But this orthodox account mistakenly assumes that realists and positivists accept the same conception of models and analogies, whilst disagreeing only about their function. Against this, we suggest that their disagreements about what constitutes an adequate scientific theory result in their having different conceptions of the nature of models and analogies. Since the positivist is concerned to arrive at theories which are statements either of empirical or theoretical laws, there is no function at all for models which, conceived from the realist standpoint, are descriptions of actually existing structures and mechanisms. It would be misleading to say that positivists regard such models as serving only a heuristic function, since on their view of theories, there is *no* function for models conceived in this way. The most that the positivist could allow is that we sometimes use an already familiar *source* as a way of suggesting laws that would explain a phenomenon apparently dissimilar to the source, and that we may find it easier to understand a theory if stated in these familiar terms. Indeed, the positivist position tends to deny the relevance of the distinction between models and their sources.

It could also be misleading to say that, for the realist, models and analogies are essential to scientific theories. If theories are seen as attempted descriptions of structures and mechanisms, the distinction between theories and models seems to collapse, since models are characterized in precisely the same way. The distinction can only be maintained by saying that, for the realist, to call something a model is to refer to a relatively early stage in the process of theory-building, in which the use of sources for the construction of the model is of considerable importance, and the status of the model as a correct representation is still highly speculative. However, we will return to some of these issues at the end of the next chapter. Let us now consider the realist view of the distinction between theoretical and observational terms.¹²

3 Realism and theoretical terms

In the previous chapter, we suggested that positivists were mistaken in trying to use the theoretical-observational dichotomy to solve what are, in fact, three distinct problems: the construction of an epistemologically certain foundation for science, the provision of means for comparing and evaluating rival theories, and the delimitation of an acceptable scientific ontology. The relationship between the first two problems will be examined in the following chapter. Here, we will be concerned with the ways in which realists and positivists differ with respect to the relationship between the first and third. And, for the most part, we will ignore the various difficulties of drawing any single distinction between the theoretical and the observational.

We characterized the positivist view of the observation language as ascribing to it both epistemological and ontological privilege. The realist rejects this ascription of ontological privilege. Scientific theories enable us to give causal explanations of observable phenomena by their description of structures and mechanisms that are typically beyond observation (even if we include as observable those items which can be detected by available instruments). Thus, although the realist may accept that the truth or falsity of observational statements is a matter of far greater certainty than that of theoretical statements, any attempt to limit the ontological commitments of a scientific theory to those made in observational statements is strongly rejected. The question of what exists, or can intelligibly be said to exist, must be kept distinct from that of what we can observe, or know with a high degree of certainty.

However, it is not enough for the realist simply to deny the positivist equation of epistemological and ontological privilege. For that equation, as we noted earlier, is itself based upon the positivist's adoption of a general criterion of scientificity, a principle by which scientific and non-scientific statements can be distinguished. It is this principle, according to which a statement is only scientific if it is possible to ascertain its truth or falsity by means of direct observation, which generates the problem of theoretical terms for positivism. It also suggests the manner in which the problem is to be resolved; for example, by the use of correspondence rules which render theoretical statements scientific by providing definitions of theoretical terms in the observation language. The realist must either reject the whole idea of finding a general criterion of scientificity, or else produce an acceptable alternative criterion.

But the latter option, if taken, may not be particularly difficult. For, by making some relatively minor adjustments to the positivist criterion, realists can allow all the ontological commitments they

might wish a scientific theory to be able to make. Consider this principle: a statement is scientific only if it is possible to make observations that would count in some way for or against its truth or falsity. Such a criterion of scientificity would enable the realist to accept theoretical statements at their face value, and remove the necessity of translating them into an acceptably 'scientific' observation language.¹³

Since the realist rejects the basis of the problem for which correspondence rules were introduced as one type of solution, it is not surprising that such rules, if used at all, take on a very different function. For the positivist, they are ways of specifying the meanings of theoretical terms by relating these to statements in the observation language. For the realist, they suggest ways of indirectly testing the truth or falsity of theoretical statements; or means by which the presence or absence of the items denoted by theoretical terms can be observationally detected or inferred. Thus, in the case of the operational definition of magnetism (p. 20 above), the results of placing a small piece of iron near an object should be seen as a way of *testing* the claim that the object is magnetic, and not as expressing the *meaning* of the statement 'x is magnetic'. Similarly, with partial interpretations: they should not be seen as partial specifications of the meaning of the theoretical term 'Q', but as stating different ways of empirically detecting the presence or absence of the item to which 'Q' refers.

However, for various technical reasons, the realist cannot accept the logical form of either operational definitions or partial interpretations as adequately representing these relations of empirical testing. Instead, something of the following form is proposed: 'if x is Q, then if test-conditions C are applied, result R will occur'. (Symbolically: $Qx \rightarrow (Cx \rightarrow Rx)$.) In this form, positive results provide only indirect, and logically non-conclusive, evidence for the truth of 'x is Q'. We postulate that x is Q, and argue that, if this is so, we should get R when we carry out C. If this is what happens, it gives us some reason to believe that x is Q. But there are many reasons why R may happen, of which x's being Q is only one. Indeed, what is often involved in such cases is an argument from effects to causes (that is, from R as the effect of Q in C, to Q): but one can never be sure that there is only one possible cause for any given effect.¹⁴

Thus the realist regards 'correspondence rules' (if this phrase is still used) as frequently expressing causal relations between theoretical entities and observable phenomena. It is because an entity of some kind exists, or has some property, that when the specified test conditions are carried out, the predicted results occur. This is very different from the positivist view of correspondence rules as specifying

the meanings of theoretical terms. Further, this way of stating the difference suggests that here, just as in their criticism of the Humean theory of causation, realists must also reject the verificationist theory of meaning that underlies the positivist conception of correspondence rules. According to this theory (which entails, but is not entailed by, the positivist criterion of scientificity), the meaning of an empirical statement is constituted by its mode of verification. So, for the positivist, in providing correspondence rules that link theoretical terms to test procedures and their observable results, we are giving the meanings of those terms, and thus of statements containing them. But the realist rejects this theory of meaning. Instead, it is maintained that the meanings of theoretical terms can be understood independently of the construction of test procedures which enable us to verify indirectly the presence or absence of the items referred to by these terms.

We will not discuss the general philosophical grounds for rejecting the verificationist theory. But we will conclude this section by considering two of the ways in which realists may give an account of the meanings of theoretical terms. First, it may be possible to give analogical definitions for such terms, by relating their meanings analogically to some already understood terms. Such analogical definitions may often be derived from the source of a model, but not always, as is shown by the following example provided by Achinstein (1968, pp. 117-18):

When Maxwell introduced the concept of self-diffusion of molecules in a gas he invoked an analogy between molecules and bees in a swarm:

'If we wish to form a representation of what is going on among the molecules in calm air, we cannot do better than observe a swarm of bees, when every individual bee is flying furiously, first in one direction and then in another, while the swarm as a whole remains at rest, or sails slowly through the air.'

The second way is suggested by a criticism, made in section 3 of the previous chapter, of the positivist's attempt to state the theoretical-observational dichotomy as existing between two sets of terms. Many terms, such as 'moon' or 'electric charge', are used to characterize both observable and unobservable items. Assuming that the meanings of such terms are relatively unproblematic in their observational uses, the realist can simply maintain that these meanings remain much the same in their non-observational, or theoretical, uses. This is well illustrated in the following quotation from H. A. Lorentz (quoted in Schaffner, 1969, p. 282), discussing the introduction of the concept of the electron:

If we want to understand the way in which electrical and magnetic properties depend on the temperature, the density, the chemical composition, or the crystalline state of substances . . . we shall be obliged to have recourse to some hypothesis about the mechanism that is at the bottom of the phenomena. It is by this necessity that one has been led to the conception of *electrons*, i.e. of extremely small particles, charged with electricity . . . by whose distribution we endeavour to explain all electric phenomena that are not confined to the free ether. . . .

Here, we are given an initial definition of the term 'electron', not by means of positivist correspondence rules, but by the use of terms that are already understood in observational contexts. There is no reason to believe that these terms, 'small', 'particle', 'charged with electricity', suddenly lose or radically alter their meaning when they are used to characterize a theoretical entity, the electron. Further, Lorentz's comments on the reasons for introducing this concept provide a good example of the realist view of scientific theories and explanation.¹⁵

4 Some problems for realism

We will now consider various objections that might be made against the realist view of science. Some of these are from a specifically positivist standpoint, but others concern the extent to which realism can provide an adequate account of the major kinds of theoretical activity and achievement that actually take place in science. One such objection, to the realist analysis of explanation, is that it is far too restrictive, since there are many scientific theories and laws that can properly be regarded as providing explanations, but not of the sort advocated by the realist. For example, the laws of free fall, or of pendular motion, certainly have some form of explanatory power, and must surely count as significant achievements in the history of science. When some phenomenon is shown to be an instance of these elegantly stated, economically formulated systematizations, it would be wrong to deny that any explanation has been provided, through an undue restriction of this concept to the kind of causal explanation advocated by the realist. And this objection can be extended to the realist view of theories, as failing to represent an important kind of scientific theory, sometimes called 'reticular' or 'abstractive'. The ideal gas laws, or, indeed, the whole of classical mechanics, are examples of reticular theories, which clearly differ from the causal theories advocated by the realist, such as the molecular theory of gases, or the virus theory of infection. In reticular theories, no attempt is made to discover underlying mechanisms or structures, but only

to formulate laws governing the behaviour of a wide range of observable phenomena. Reticular theories typically involve terms, such as 'force', which do not refer to observables: but it would be wrong to interpret them as referring to unobservable causes, unlike terms such as 'virus'. Any descriptively adequate philosophy of science must recognize the significance of both types of theory, and not pay undue attention to either one.¹⁶

Of course, even if these objections are successful against realism, they do not thereby vindicate the positivist position. For, although positivists might argue that the D-N model, and their account of laws and theories, accurately represent these non-causal explanations and reticular theories, it could likewise be objected that their own account of science is descriptively inadequate with respect to those elements emphasized by the realist.¹⁷ Further, both realists and positivists might defend themselves against this double-edged objection by maintaining that philosophical theories about science cannot be refuted by such claims of descriptive inadequacy. To think that this is possible is to ignore the essentially prescriptive nature of the philosophy of science, which distinguishes it from a purely descriptive approach, such as the historical one.

We will not try to resolve directly this general issue about the relations between the philosophy of science and scientific practice. But some light is thrown on it by the following suggestion that may be made by a realist who tries to tackle the problem of descriptive adequacy. The suggestion is that, although reticular theories are of considerable importance in science, they do not provide a form of explanation acceptable in its own right as an alternative to causal explanation. Instead, such theories must always be replaced eventually by causal theories, which both explain the regular relations represented by the reticular theory, and provide properly causal explanations of the phenomena that can be shown to be instances of those relations. It may also be suggested that a significant theme in the historical development of science is well represented by this account of the philosophical relation between causal and reticular theories: namely, the successive attempts to discover causal theories which explain reticular theories, often motivated by the dissatisfaction felt by scientists about the explanatory power of the latter.¹⁸

However, the success of this suggestion depends partly on the adequacy of the realist's alternative to the Humean view of causation. Were this to be inadequate, the distinction between the two types of theory would lose much of its force, since if causality is a matter of regularities, the distinctive nature of *causal* theories becomes highly problematic. Further, the positivist might then replace the realist account of the relations between reticular and causal theories by one in terms of the successive D-N explanations of one reticular theory by

another. So let us now consider a positivist objection to the realist view of causation. The objection is that the realist has provided no adequate analysis of causal, or natural, necessity; and that what are presented by the realist as descriptions of structures and mechanisms that causally necessitate observable phenomena, are no more than descriptions of Humean regularities of high generality.

A realist reply to this can be stated only briefly. It is true that the concept of natural necessity has not yet been satisfactorily analysed. Indeed, some philosophers argue that it is essentially unanalysable. However, it is increasingly widely accepted that scientific laws cannot themselves be adequately characterized without some reference to this possibly unanalysable concept of necessity.¹⁹ Further, the realist may accept that, in talking about describing structures and mechanisms, no direct analysis is being given of the concept of necessity; and also that such descriptions are, at least partly, of regularities, such as the behaviour of the molecules that constitute gases. But it does not follow from this that the explanatory power of causal theories derives only from the deducibility of one set of regularities from another, as the positivist maintains. Finally, the discovery of one set of structural regularities is not regarded by the realist as ruling out any further explanatory enquiries. Rather, the existence of such regularities must itself be causally explained, by the positing of further structures and mechanisms, and so on.

This last point raises an important question, and one which enables us to state a somewhat attenuated, and perhaps more defensible, version of the positivist view of causation and theories. Are there any ultimate explanations (that is, those for which no further explanations can be given), or is the process of successive causal explanations, and thus of scientific enquiry itself, in principle infinite and unending? The answer to this question depends basically upon that of the finitude of nature, for there can be no ultimate explanations of an objectively infinite nature. But suppose we accept its finitude, and thus the possibility of ultimate explanations and theories. We can then characterize the positivist view as this: such ultimate explanations must be in terms of 'brute regularities', that simply happen to be those that obtain in our physical world.²⁰

Let us now consider a rather different objection to realism, that it is *essentialist*. The terms 'essence' and 'essentialism' have been used in a large variety of different ways, and we will only examine two of these here. Popper, a well-known opponent of essentialism in science, characterizes the essentialist as making two claims. First, that it is possible to establish the truth of a scientific theory beyond all reasonable doubt. Second, that the scientist can provide ultimate explanations by discovering the essence or 'reality' that lies behind the 'appearances'. Does the realist position, as we have presented it,

entail either of these claims? It would seem not. There is no commitment to the first claim. The realist is concerned primarily with the nature of those theories which the scientist should be trying to construct, and has no inclination to underestimate the degree of difficulty, and uncertainty, involved in such projects. On the question of ultimate explanations, the realist can remain agnostic: as we have already suggested, this turns on the finitude of nature, and there is nothing in the realist position which counts for or against finitude. As for the distinction between reality and appearance, this may often be invoked by the realist, but not in a way that implies belief in either certainty or ultimacy.²¹

The second sense of 'essentialism' is mainly concerned with a theory of definition, and of the function of such definitions in scientific explanation. According to this, definitions are descriptions of the essences, or essential properties, of things, and we can assess various attempts at definition in terms of the truth or falsity of the descriptions given by them. Scientific explanation requires the discovery of such essences, and thus of correct definitions. These views are often ascribed to Aristotle, together with the belief in a world that is objectively divided into 'natural kinds', to which correct definitions must correspond. But, whether or not this ascription is correct, the realist is not committed to this theory of definition, to the view that explanations can be discovered by definitions, or to a belief in natural kinds.²²

Historically, the term 'essence' is often associated with others, such as 'occult quality' or 'substantial form', which are seen by positivists as denoting mysterious entities that do not belong to genuinely scientific theories. The concept of phlogiston is typically regarded by positivists in this way, as we noted in the last section of the previous chapter. By considering this example, we can examine the claim that the realist position inevitably leads to the postulation of unintelligible entities, and thus retards the development of science. The phlogiston theory is often criticized on the following grounds: that it had no explanatory value; that phlogiston was unobservable; that absurd properties, such as negative weight, had to be ascribed to this entity to explain away the increased weight of metals after combustion; and that only when Lavoisier subjected the theory to the scrutiny of experimental data was chemistry at last established on a scientific basis.

But such criticisms are mistaken. Instead, the postulation of phlogiston was a reasonable attempt to explain, in a unified manner, many apparently disparate phenomena. In its earlier stages, the theory was extremely fruitful. It was recognized that the precise nature of the postulated entity required further specification, and several suggestions were made, such as its identification with

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'inflammable air' (hydrogen), or the idea that soot was almost pure phlogiston. The negative weight hypothesis was not essential to the theory of phlogiston, since other explanations of weight increase were proposed, especially in terms of the absorption, during combustion, of matter from the fire and heat. However, Lavoisier's experiments showed this last explanation was highly improbable; and the fact that metallic mercury could be produced by heating mercury calx without the presence of carbon also made the phlogiston theory implausible. Lavoisier's theory enabled most of the same phenomena to be explained without reference to phlogiston: it thus became reasonable to deny its existence. Yet we can find many examples in the history of science (such as the postulation of the 'gene' to explain Mendel's laws of inherited characteristics), where similar postulations were followed by successful identifications of the hypothetical entity (such as the DNA molecule). For the realist, these attempts to explain the regularities of observable phenomena are both necessary and legitimate. The failure of specific hypotheses, which were reasonable at the time they were proposed, should not lead us to eliminate all such hypotheses as, in principle, unscientific. On the contrary, it is only in this way that science can develop so that it deepens our understanding of the physical world.²³

We conclude this chapter by outlining some of the features common to realism and positivism. Both share a general conception of science as an objective, rational enquiry which aims at true explanatory and predictive knowledge of an external reality. Let us consider the concepts of objectivity and rationality that are involved here. There are two different aspects of the former concept. First, the idea that scientific theories must be objectively assessed by reference to empirical evidence. This evidence is such that all scientists who are competent, honest, and lacking in perceptual deficiencies can agree upon it, though not necessarily with total certainty. Second, there is the idea that there are 'objects', in the broadest sense of the term, which exist independently of our beliefs and theories about them. In other words, there is some commitment to a theory of truth in which there is a clear dichotomy between 'the world', and the various attempts that we may make to describe and explain it correctly. This means a rejection of the view that scientific theories determine that reality, rather than make genuine discoveries about it. Science is descriptive, and not constructive, of the nature of that which exists.

What concept of rationality is regarded by realists and positivists as characterizing science? Both hold that there are general standards of scientificity, of what counts as an adequate explanation, of what it is that we must try to achieve by scientific theories, of the manner in which empirical evidence should be used to assess their truth or

falsity, and so on. Whilst disagreeing about what these standards are, both believe that they exist, and can properly be used to evaluate specific cases of scientific practice. They are external and universal standards, independent of particular, substantive theories and explanations, and applicable to all periods in the historical development of science.

It is these shared assumptions of objectivity and rationality which, stated here in a simplified manner, are rejected to varying degrees, and for varying reasons, by the philosophers of science we will consider in the following chapter.

3 Forms of conventionalism

1 Problems of falsification

If empirical evidence is to be regarded as an adequate objective control for the acceptance and rejection of scientific theories, it is essential to specify precisely the logical relations involved in the testing of those theories by observation. We have noted already the impossibility of conclusively verifying scientific theories: the best that can be hoped for is the establishment of degrees of positive confirmation. But it is often maintained, by writers such as Popper, that there is an important asymmetry between verification and falsification. If we deduce predictions from a theory which are then shown by observation to fail, this must entail the falsity of that theory, whether it is a reticular or causal one.¹

At first sight, this claim seems to be justified as a matter of simple logic. Let 'T' stand for a scientific theory, and 'O' for a statement of the observable results of some test-procedure. If we can deduce O from T, and show that O is false, it follows logically that T is also false. If, however, O is shown to be true, it does not follow that T is true: thus the impossibility of conclusive verification. But several problems arise when this simplified logical model of falsification is applied to the actual process of testing a scientific theory. The degree of certainty with which we can assert the falsity of T depends upon the corresponding degree of certainty of the falsity of O. Logically, all that can be shown is that if O is false, then so is T: any uncertainty about the former is necessarily reflected in the latter. Further, what is in fact deducible from a theory is not a statement describing the observable results of a test-procedure, but a hypothetical, or conditional statement, asserting that if the relevant procedure is carried out, such results will occur. Failure to get the predicted results does not, therefore, directly falsify the theory, since it is always possible that the test-procedures have not been carried out satisfactorily.

However, we will ignore these two difficulties in what follows, and concentrate on a more fundamental problem. This arises from the fact that, in order to derive testable consequences from a theory, it is usually necessary to make assumptions additional to those involved in the theory itself. For example, if an optical microscope is employed in making observations to test a theory, we have to assume the theoretical principles upon which the construction and use of the microscope are based. The general problem raised by such examples was clearly articulated by Pierre Duhem (1954, p. 187):

The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting the group is unacceptable and ought to be modified; but the experiment does not indicate which one ought to be changed.

If we represent the additional assumptions, sometimes called 'auxiliary hypotheses', by 'A_{1-n}', then the problem can be described in this way: if O can be deduced only from T together with A_{1-n}, then the falsity of O does not entail the falsity of T, but the falsity of either T or one of the assumptions A_{1-n}. We know that at least one of these must be false, but not which one. In particular, the falsity of O is logically compatible with the truth of T. (Symbolically: [(T & A_{1-n} → O) & -O] → -(T & A_{1-n}). This contrasts with the simplified logical model of falsification, where we have: [(T → O) & -O] → -T.)

A simple illustration of this problem is provided by Hempel.² The Hungarian physician Semmelweiss, investigating the high rates of women's mortality in childbirth, put forward the theory that these were due to blood poisoning, caused by infectious matter carried on the hands of doctors who examined the women after performing dissections in the autopsy room. From this hypothesis, he argued that the mortality rates should decrease if the doctors washed their hands in a solution of chlorinated lime, so destroying the infectious matter. Appropriate experiments were carried out, and the rates decreased as predicted, thus confirming the theory, though not, of course, conclusively verifying it. Suppose, however, that the predicted decrease had not occurred: would this have falsified the theory? Not conclusively, for it might have been an additional assumption, that chlorinated lime solution destroys infectious matter, that was false, and not the theory itself.

W. V. Quine has raised a more radical objection to the possibility of conclusively falsifying scientific theories, in the course of a general critique of the orthodox empiricist distinction between analytic and synthetic statements. He says (1961, p. 43 - our insertion):

Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery [i.e. one apparently directly testable by sensory experience] can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision.

If we apply these remarks specifically to the empirical testing of scientific theories, it would seem that Quine's claim is that it is always possible to rescue a theory from apparently falsifying evidence, by making some further assumption that maintains the consistency of the theory with this evidence. In other words, if we derive O from T, together with assumptions A_{1-n} , and find that O is false, we can always make some further assumption, A_o , which is such that the falsity of O is consistent with the truth of T and A_o . (Note that A_o may sometimes be the denial of one of A_{1-n} .)

It is unclear quite how this claim could be shown to be true. We will confine ourselves to making three points about it. First, the scope of the 'adjustments' that might have to be made to preserve a theory is extremely large: thus Quine explicitly mentions changes in logical laws, and the plea of hallucination. Second, and related to this, the claim involves the idea that it is our whole system of knowledge that is being put to the test at any one time. This is a considerable extension of Duhem's objection to the possibility of testing single, isolated hypotheses. Third, we must distinguish the view that it is always possible to make the necessary adjustments, or additional assumptions, from the view that it is always reasonable to do so. Thus, even if no apparently falsifying evidence can constitute a logically conclusive falsification of a scientific theory, it does not follow that it is always, or normally, more reasonable to make the necessary adjustments than to abandon the theory.³

This suggests that the problem of theory-falsification should be seen primarily as that of finding criteria for the reasonable abandonment of scientific theories, not for their logically conclusive disproof. Looking at the problem in this way, the simplest solution would be to adopt the following rule: always abandon a theory when it is apparently contradicted by observation, even though it is logically possible to save the theory by adding further assumptions. However, this rule (sometimes called 'naive methodological falsificationism', and ascribed to Popper in his earlier writings) has some obvious disadvantages. In particular, it can be shown that had scientists consistently followed it, many major scientific achievements would never have occurred, since theories which eventually turned out to be highly successful would have been abandoned too soon.⁴

Partly in response to this difficulty, Imre Lakatos has proposed an alternative doctrine, which he calls 'sophisticated methodological falsificationism'. He suggests that we cannot rationally decide to abandon any one theory, T, unless there exists some alternative theory, T', which is in certain important respects preferable to T. The decision to abandon T rests not only upon its relation to the empirical evidence, but also upon its relation to alternative theories. The problem, then, is to define what makes T' preferable to T. Lakatos's solution to this is given in the following passage (1970, p. 116):

The sophisticated falsificationist regards a scientific theory T as falsified if and only if another theory T' has been proposed with the following characteristics: (1) T' has excess empirical content over T: that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden by, T; (2) T' explains the previous success of T, that is, all the unrefuted content of T is contained (within the limits of observational error) in the content of T'; and (3) some of the excess content of T' is corroborated.

Simplifying this slightly: it is reasonable to abandon T if and only if there is an alternative theory T' which explains everything that T explains, and generates predictions not derivable from T, some of which have been confirmed by empirical testing.

Several objections can be made to this rule. It will sometimes be too stringent, since an alternative theory may be preferable, despite its not, at least initially, explaining everything that its predecessor explained. And some theories may become so hopelessly at odds with experimental results that it is reasonable to abandon them, even if there are no viable alternatives. Further, there is no analysis of the stage at which it becomes rational to develop alternative theories to the one in trouble. One possibility here, suggested by Paul Feyerabend, is that scientists should adopt a 'principle of proliferation'. According to this, a wide range of competing theories should always be formulated, and scientists should never be concerned solely with the development and testing of any one theory. Indeed Feyerabend argues that, in many cases, it is only by means of this proliferation that counter-evidence to the dominant theory can emerge, or be recognized as such.⁵

However, the most fundamental objection concerns a necessary assumption of both Lakatos's and any similarly 'comparative' account of theory abandonment. The assumption is that it is possible to describe the observational consequences of two or more theories in such a way that we can intelligibly talk of one theory explaining or predicting more than another. In other words, it must be assumed

that we can make observation statements 'in the same language' for each theory. If this is not possible, it makes no sense to talk of comparing their predictive and explanatory powers, since there would be no common, theory-neutral language in which these comparisons could be stated. But it has been argued by several philosophers of science that such a language does not, and cannot, exist.

2 The problem of theory-neutrality

Recent attacks on the theory-neutrality of observation, and thus, implicitly or explicitly, on the possibility of an objective, observationally controlled science, have taken two main forms. First, it is claimed that sensory perception is somehow influenced by theoretical beliefs and expectations. Second, that the meanings of observational terms are in some way dependent upon those of theoretical terms or of the various statements that constitute a scientific theory. Both raise difficult issues in epistemology and the philosophy of language. We will concentrate here on the relation between theories and perception, and on the account of this given by Norwood Hanson.⁶

Hanson's account is based upon an analysis of the concept of seeing, or visual perception. He is concerned to rebut the orthodox distinction between visual *data* and their *interpretation*. By using this distinction, many philosophers have claimed that the proponents of rival theories can be said to agree on what they see, or observe, whilst disagreeing about the theoretical interpretation of such data. Further, agreed observations can be used to resolve disagreements over interpretation: competing theories can be assessed by reference to uninterpreted data. Against this, Hanson argues that, in the sense of seeing relevant to scientific enquiry, the proponents of different theories do not agree on what they see. They see different things just because of their different theoretical beliefs. Each scientific theory provides its own way of describing what we see. It is not that they give different ways of interpreting the data which are what we all, whatever our theories, in fact see. Rather, our theories determine what we see, and there are no separable components of data and interpretation.⁷

The issues involved here are examined by Hanson with reference to a number of examples. The one he discusses most frequently is introduced as follows (1958, p. 5):

Let us consider Johannes Kepler: imagine him on a hill watching the dawn. With him is Tycho Brahe. Kepler regarded the sun as fixed: it was the earth that moved. But Tycho followed Ptolemy and Aristotle in this much at least: the earth was fixed and all other celestial bodies moved around it. *Do Kepler and Tycho see the same thing in the east at dawn?*

Hanson argues that, although there are ways of answering this question in the affirmative, these do not provide us with the sense of 'seeing' that is primarily involved in scientific observation: in this sense, Kepler sees a static sun, and Brahe a mobile one.

He rejects the view that the similarity of the retinal stimulation received by the two astronomers could justify the claim that they saw the same thing. For it is mistaken to claim that these physical stimulations are *seen* by their recipients. He also rejects any recourse to the idea of pure visual experience, and thus to the claim that both Kepler and Brahe had the same such experience, namely of 'a brilliant yellow-white disc centred between green and blue colour-patches' (1958, p. 8). Hanson argues that no general distinction can be drawn between uninterpreted visual experiences, and the different interpretations that may be made of them. To show this, he considers a series of examples from Gestalt psychology, such as the Necker cube, Köhler's goblet-and-faces figuration, and the antelope-bird. He describes these as 'shift-of-aspect phenomena'. In these cases, we cannot talk intelligibly of some uninterpreted visual experience: in seeing the figures first as one thing, and then as another, we cannot be described as giving different interpretations to the same visual data. The interpretation is, as it were, *constitutive* of the seeing, and not a separable component. Or, rather, the concept of interpretation has no place here, and so neither does the distinction between interpretation and data.

Hanson regards these shift-of-aspect phenomena as providing relevant analogies for what is involved in the relation between different scientific theories and what their proponents can be said to see. Thus (1958, p. 17):

You see a bird, I see an antelope; the physicist sees an X-ray tube, the child a complicated lamp-bulb; the microscopist sees coelenterate mesoglea, his new student sees only a gooey, formless stuff. Tycho and Simplicius see a mobile sun, Kepler and Galileo see a static sun.

Further, Hanson argues that the concept of seeing appropriate to scientific observation, is such that what it is that scientists see is essentially related to their knowledge and beliefs. To the extent that these beliefs differ, what they can be said to see also differs⁸ (1958, pp. 23-4 - our italics):

Tycho sees the sun beginning its journey from horizon to horizon. He sees that from some celestial vantage point the sun (carrying with it the moon and the planets) could be watched circling our fixed earth. Watching the sun at dawn through Tychonic spectacles would be to see it in something like this way.

. . . But Kepler will see the horizon dipping, or turning away, from our local fixed star. The shift from sunrise to horizon-turn is analogous to the shift-of-aspect phenomena already considered; *it is occasioned by differences between what Tycho and Kepler think they know.*

We can begin to assess Hanson's account of seeing by showing its implications for the use of observation in testing and comparing scientific theories. If what we see or observe is determined by our beliefs or knowledge, then we cannot, without circularity, test the truth or falsity of those beliefs by means of observation. This radical consequence of Hanson's position can be brought out in the following way. If what we observe depends upon our beliefs, and these beliefs are expressed in our theories, then the truth of our claims about what we observe must depend upon the truth of those theories. This means that if our observation-statements are true, our theories must also be true. But if this is so, we cannot falsify our theories by means of true observation-statements, since if the latter are true, the former cannot be false. Furthermore, we cannot judge between two competing theories on the basis of what is observed since, in the crucial cases, the proponents of these theories will not be able to agree on the truth or falsity of the relevant observation-statements. If their differences of belief result in differences of observation, they cannot settle their theoretical differences by reference to agreed observations.

Such conclusions seem counter-intuitive: is it possible to avoid them, without completely rejecting some of Hanson's insights into the concept of scientific observation? At least two of his claims can certainly be accepted. First, that when asked to describe what they see or observe, the proponents of rival theories will typically give different answers, which reflect their theoretical differences. Second, that it is not possible to provide descriptions of what we observe that are totally free of any conceptual and theoretical implications and assumptions. However, it does not follow from these claims that there is no way of correctly describing observations which is independent of the specific theory, or theories, that are being tested and assessed. If we believe some theory to be true, we will tend to describe our observations in terms of it. But this does not mean that, when the truth of this theory is in dispute, we cannot describe those observations in a manner that does not presuppose its truth. Similarly, though rival theorists will often disagree in their accounts of what they observe, they may be able to find agreed descriptions by reference to which the relative merits of their theories can be compared. Such descriptions will not be totally 'theory-free', but free of the particular theoretical beliefs at issue.

Let us consider how the position just outlined might be applied to

Hanson's example about Kepler and Brahe. Although, when asked what they see at dawn, they may give different answers – such as 'horizon-turn' and 'sunrise', or 'static sun' and 'mobile sun' – there are several other answers they could agree upon. One of these would be in terms of the 'visual experience' of 'a brilliant yellow-white disc centred between green and blue colour patches'. Another might be 'a brilliant heavenly body', a suggestion which Hanson makes at one point. Notice that this latter answer might itself be disputed by scientists whose theoretical disagreements were not those of Kepler and Brahe, but instead concerned some other issue, such as whether it was legitimate to apply the term 'heavenly' to the sun. In this (perhaps unlikely) case, Hanson's 'yellow-white disc . . .' description might nonetheless be agreed upon.

It must not be thought that this 'visual experience' description is an example of some uniquely privileged observational language, to be used in all cases of theoretical disagreement, and providing an epistemologically incorrigible foundation for science.⁹ Hanson's arguments about shift-of-aspect phenomena, together with other philosophical arguments, and results from the psychological study of perception, make any such view untenable. These results are summarized by C. A. Hooker (1973, p. 65):

A perceptual judgment then may be a function of our past experiences, our current emotional state, our current beliefs, our current foci of interests and so on, as well as the actual patterns of stimulation of our sensory receptors.

But, as Hooker goes on to argue, there is little evidence that the kinds of theoretical beliefs normally involved in scientific disputes figure significantly amongst the determinants of perceptual judgments. In other words, although the ideal of science as based upon some pure, uninterpreted realm of perceptual experience is untenable, the opposite view, that theoretical disagreements typically result in perceptual differences, is equally so.

Hanson, as we have noted, presents his own account in terms of an analysis of the sense of 'seeing' that is 'relevant for science'. We can now see an important ambiguity in this phrase. The sense that he regards as relevant is that involved in the way that scientists typically talk about what they see or observe. But we may also be interested in the sense that is relevant to the problems of theory testing and comparison, and there is no good reason to believe that an account of seeing that is relevant to the former concern is equally relevant to the latter. Further, although Hanson may be right in regarding shift-of-aspect phenomena as vitiating any general distinction between data and interpretation, the analogy he suggests between such phenomena, and what occurs in cases of theoretical disagreement, is

too tenuous to establish his conclusions. For Hanson, scientific observations are, as it were, 'shift-of-theory phenomena'. But whereas, in examples such as the antelope-bird figure, it seems that we necessarily see them either one way or the other, this is not so in the kinds of observational situations relevant to the assessment of scientific theories.

Let us conclude by suggesting one of the implications of the position we have been developing in opposition to Hanson's. Much of the language used to describe what we see – the results of experiments, and so on – is theory-laden, in that it assumes the truth of various scientific theories and beliefs. Thus it will often seem that alternative theories to those presently accepted are contradicted by established facts. The ways in which we describe observations will be such that they appear to constitute fairly conclusive counter-evidence to theories that are opposed to those assumed by these observational descriptions. It follows that, in the formulation and defence of alternative theories, an important element will be the detection and challenging of the theoretical assumptions made in the existent observational language. Otherwise, many such alternatives will be prematurely dismissed, through a failure to see that the evidence against them is 'loaded' by theories that may themselves be defective.

It is partly for these reasons that Feyerabend, as we noted in the previous section, defends the principle of proliferation, and argues that without this principle, much evidence that is inconsistent with existing theories may never be revealed as so. Not only does the incorporation of such theories in the observational language distort the apparent relations between observations and alternative theories; it may also conceal what is counter-evidence to accepted theories.¹⁰ For Feyerabend, the proliferation principle forms part of what he calls 'an anarchistic theory of knowledge', which involves a denial of the possibility of formulating rational standards for the assessment of scientific theories. We will examine this in the next section. But we begin by considering an account of science in some respects similar to Feyerabend's, that of Thomas Kuhn.

3 Paradigms and anarchy: problems of rationality

In *The Structure of Scientific Revolutions* (Kuhn, 1970a), Kuhn's central concern is to characterize the way in which science historically develops. He regards this characterization as having important consequences for the philosophy of science. His conclusions challenge not only several features of the conception of science as a rational and objective enquiry, but also the view that philosophical theories cannot be undermined by recourse to historical, sociological and psychological studies.

For Kuhn, the development of each branch of science alternates between two recurrent stages: normal, and revolutionary science. In the former stage, scientific activity is governed by a 'paradigm'. Paradigms are initially defined by Kuhn as 'universally recognized scientific achievements that ... provide model problems and solutions to a community of practitioners' (1970a, p. viii). In normal science, the scientist's work is devoted to the articulation and wider application of the accepted paradigm, which is not itself questioned or criticized. Scientific problems are regarded as *puzzles*, as problems which are known to have a solution within the framework of assumptions implicitly or explicitly embodied in the paradigm. If a puzzle is not solved, the fault lies in the scientist, and not in the paradigm.

By contrast, in the relatively brief periods of revolutionary science, the scientist is confronted by increasingly perplexing *anomalies*, which call into question the paradigm itself. Scientific revolution occurs when a new paradigm emerges, and becomes accepted by the scientific community. But the acceptance of the new paradigm does not take place solely as a result of a process of critical argument and assessment between the proponents of the competing paradigms. For individual scientists, the change of allegiance from one paradigm to another is often a 'conversion experience', akin to gestalt-switches or changes of religious faith. And for the scientific profession as a whole, Kuhn quotes with approval Max Planck's famous dictum: 'A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it' (1970a, p. 150). Such changes require sociological and psychological categories for their explanation: not a rational reconstruction in terms of the standard conceptions of philosophers of science, according to which theories are accepted and rejected by reference to observations, subject to the agreed universally applicable standards of scientific rationality.

Some philosophers of science might accept these claims as historically correct, but deny their philosophical relevance. However, such a response not only rests upon too restrictive a view of the relation between philosophical and historical studies, but also ignores the distinctively philosophical basis of many elements in Kuhn's account. For example, one reason which Kuhn has for rejecting 'the methodological stereotype of falsification by direct comparison with nature' (1970a, p. 77) is his rejection of the theory-neutrality of observation. This rejection is partly based on the same grounds that we discussed in the previous section, and leads him frequently to make assertions of the following kind (1970a, p. 117 – our italics):

During the seventeenth century, when their research was guided by one or another effluvia theory, electricians repeatedly *saw* chaff particles rebound from, or fall off, the electrified bodies that had attracted them. At least that is what seventeenth-century observers *said they saw*, and we have no more reason to doubt their reports of perception than our own. Placed before repulsion (rather than mechanical or gravitational rebounding). . . .

We have already criticized the use of this concept of seeing; and we can also note that many of the examples that Kuhn cites against the 'methodological stereotype of falsification' could be accommodated within the sophisticated methodological falsification of Lakatos, discussed in section 1.¹¹ But there is another reason for Kuhn's rejection of the possibility of rationally assessing competing paradigms, which raises different issues. He argues that the relevant standards of assessment are typically *internal* to each paradigm, and vary from one to another. To understand this argument, we must examine further his conception of paradigms. Kuhn's initial definition rapidly broadens into the idea of a set of shared assumptions, often implicit, that govern the activity of a scientific community. In later writings, he distinguishes two concepts of a paradigm, termed 'exemplars' and 'disciplinary matrices' (1970a, pp. 181-90), and it is the latter we will consider here. This consists of a 'strong network of commitments, theoretical, instrumental, and methodological' (1970a, p. 42). An example is the corpuscularian paradigm in seventeenth-century science. This included what Kuhn calls the 'methodological' principle that ultimate explanations must be given by means of the laws governing the interactions between corpuscles, the imperceptible particles of matter of which all objects in the physical world were thought to be composed. Principles of this kind, says Kuhn, provide criteria for the selection, evaluation, and criticism of the problems and solutions arising within any one scientific community.

What happens, then, at times of paradigm-conflict and change? Paradigms (1970a, p. 103) are

the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science . . . And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.

In other words, such changes will often involve 'changes in the standards governing permissible problems, concepts and explanations' (1970a, p. 106). Thus scientific revolutions are partly constituted by changes in the standards governing legitimate explanations, and even in those that define what it is to be scientific, as opposed to, say, metaphysical. So it is not surprising that disputes between proponents of rival paradigms cannot be settled in a rational and objective manner. For, not only are there no mutually agreed, theory-neutral observational facts, but there are also no agreed standards of assessment to apply to them. In both these respects, competing paradigms are 'incommensurable'.¹²

Let us now examine this claim that standards are relative to paradigms. First, Kuhn fails to show that such standards differ from one paradigm to another. This failure is partly due to an ambiguity in expressions such as 'methodological principles' or 'standards governing permissible explanations'. The ambiguity can be illustrated in Kuhn's example of the corpuscularian paradigm. In one sense, it is true that corpuscularians were committed to the 'methodological principle' of explaining all phenomena by reference to interactions between corpuscles. But in another sense, this would not count as a methodological principle at all, since it clearly involves substantive, scientific claims about what kinds of entities exist, what relations obtain between them, and so on. In this latter sense, the D-N model of explanation would be an example of a methodological principle, which lays down standards for permissible explanations, but in a manner independent of substantive issues such as the existence and nature of corpuscles.

Thus, whilst paradigms, and scientific communities, have different methodological principles in the first sense, they may often not differ with respect to principles or standards in the second sense. Both corpuscularians and their opponents might agree on what, in general, constitutes an adequate scientific explanation, and be prepared to argue out their substantive disagreements by reference to such criteria. And to a considerable extent, this was true in the seventeenth-century. For example, the disputes between proponents of wave and corpuscular theories of light did not always involve disagreements about general criteria for scientific explanation. Conversely, although there were many methodological arguments between seventeenth century scientists, these were often conducted *within* what Kuhn would, on other criteria, regard as a single paradigm.¹³ Finally, even if Kuhn were correct in his claims about the historical relationships between paradigms and standards, it would not follow that differences between such standards are rationally unresolvable. For at least some of these differences are open to philosophical argument, of the kind we have already engaged in, in our

assessment of realist and positivist views about explanation, theories, and scientificity.

We turn now to another writer who has attacked the image of science as a rational and objective enterprise, Feyerabend. For him, this image is both mythical and undesirable. It is mythical, because it misrepresents the historical realities of scientific development, and because it is, in any case, impossible to justify any methodological rules and standards for science: 'There is only *one* principle that can be defended under all circumstances, and in *all* stages of human development. It is the principle: *anything goes*' (1970, p. 26). And the image is an undesirable one, since it directs scientists to pursue an ideal that is in total conflict with the 'humanitarian attitude'. The ideal of rationality is, says Feyerabend, 'unworthy of a free man'; and he quotes with approval J. S. Mill's endorsement of 'that cultivation of individuality which [alone] produces, or can produce well developed human beings' (1970, pp. 20-1).

Like Kuhn, Feyerabend denies the existence of a theory-neutral observational language, and of universal methodological rules and standards. But he also denies, both as reality and ideal, Kuhn's picture of normal science. We noted earlier Feyerabend's defence of a 'principle of proliferation', clearly opposed to Kuhn's puzzle-solving normality.¹⁴ Here, we will consider the nature and rationale of his views about rules and standards in scientific practice. Feyerabend argues that there are no rules whose breach cannot be shown to be justified. Indeed, it is often best to act in a way directly contrary to that required by any rule (1970, p. 22):

there are circumstances when it is advisable to introduce, elaborate and defend ad hoc hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternatives, or self-inconsistent hypotheses, and so on.

There are even circumstances – and they occur rather frequently – when *argument* loses its forward-looking aspect and becomes a hindrance to progress.

Further, in the absence of universal rules and standards, we must rely upon 'esthetic judgments, judgments of taste, and our own subjective wishes' (1970, p. 90). We can, if we wish, *choose* to conduct science in an apparently rational, objective, and 'professional' manner. But such a choice should be resisted by all who value human freedom and individuality (1970, p. 21):

Without universally enforced standards of truth and rationality we can no longer speak of universal error. We can only speak of

what does, or does not, seem appropriate when viewed from a particular and restricted point of view, different views, temperaments, attitudes giving rise to different judgments and different methods of approach. Such an *anarchistic epistemology* . . . is not only a better means for improving knowledge, of understanding history. It is also more appropriate for a free man to use than are its rigorous and 'scientific' alternatives.

Thus one significant and distinctive feature of Feyerabend's position, as compared with most Anglo-American philosophers of science, is his insistence upon the essential function of 'human values' in science. First, there is his claim that the choice between one conception of science and another is partly an *ethical* choice, and not one that can be decided by reference to the kinds of limited, 'professionally' philosophical criteria that are normally invoked. Second, in Feyerabend's preferred image of science, there is an important part to be played by the individual values, wishes and tastes of the scientist: science must be a fully *human* activity, not one which engages only the cerebral and academic elements of human beings. In suggesting the first of these connections between science and values, Feyerabend is perhaps closest in his concerns to Popper, who persistently emphasizes the links between the critical, rational nature of scientific activity, and the political values of an 'open', liberal-democratic society. But, whereas Popper often argues from the 'correct' view of scientific method to the 'correct' social values, Feyerabend's argument proceeds in the opposite direction. And, in any case, he dismisses both Popperian method and Popperian values as 'ratiomania'.¹⁵

We will make two critical comments on Feyerabend's position. First, his use of phrases such as 'methodological rule' or 'standards of rationality' is insufficiently discriminating. Most of his arguments against the existence of universal rules and standards are addressed specifically to the practical problems of how scientists should proceed in the actual construction, testing, and rejection of theories. He concludes that, at best, we can only formulate a number of 'rules of thumb', and there will always be circumstances in which it is right to ignore these. But it is possible to accept this, whilst denying that this entails the rejection of rational standards concerning the general nature of scientific theories, explanations, the relations between theories and observations, and so on. We may, for example, know what it is that would constitute an adequate explanation of some phenomenon, without this giving us any practical guides that will guarantee success in discovering one, or tell us when to abandon or continue any particular approach to the problem. But a defence of the rationality and objectivity of science does not require the formulation of such guides.¹⁶

Second, it is difficult to see how Feyerabend can both dismiss the existence of universal standards, and continue to talk, as he does, of scientific 'progress'. For example, he defends his anarchistic epistemology on the grounds that '[this] liberal practice . . . is not just a *fact* of the history of science . . . [and] not merely a manifestation of human inconstancy and ignorance . . . [but] reasonable and *absolutely necessary* for the growth of knowledge' (1970, p. 22). But it is also necessary to adopt standards and criteria for what it is that constitutes such scientific knowledge, if one is to defend or reject various means towards its growth and progress.

4 Conventionalism and instrumentalism

So far, we have avoided ascribing the term 'conventionalist' to any of the writers discussed in this chapter. The definitions that can, and have been given to this term are highly varied. A fairly standard one is given by Kolakowski (1972, pp. 158-9):

The fundamental idea of conventionalism may be stated as follows: certain scientific propositions, erroneously taken for descriptions of the world based on the recording and generalization of experiments, are in fact artificial creations, and we regard them as true not because we are compelled to do so for empirical reasons, but because they are convenient, useful, or even because they have aesthetic appeal. Conventionalists agree with empiricists on the origin of knowledge, but reject empiricism as a norm that allows us to justify all accepted judgements by appealing to experience, conceived of as a sufficient criterion of their truth. Or, to put the same point somewhat more accurately, the data of experience always leave scope for more than one explanatory hypothesis, and which one is to be chosen cannot be determined by experience.

There are several different strands in this definition, each of which may be developed in a number of directions. We will now elaborate on some of these, thereby providing a characterization of conventionalism as a group of three, loosely yet significantly related elements.¹⁷

(1) There is the idea that many scientific statements are not to be seen as true or false descriptions of some external, independently existing 'reality', but rather as creations or constructions of the scientist. This may develop into the more radical claim that, in some sense, the physical world of the scientist is created or constructed by scientific theories, and not described by them. In other words, theories are determinative of what is real, and when they change in a fundamental way, we are not faced with a different conception of the same world, but a different world. (2) There is the claim that the

kinds of considerations that are relevant in accepting or rejecting a scientific theory are somehow 'subjective', in that they are essentially related to the scientist's practical interests, aesthetic or moral values, and so on. The source of this subjectivity may be seen as the individual scientist, or the scientific community. In either case, there is a denial of the existence or adequacy of rational, universally valid criteria and standards for the evaluation of scientific activity. (3) There is the view that the truth or falsity of theories is 'under-determined' by empirical data. Observation cannot provide an objective control for science: the idea that agreed facts can enable us to choose between theories is denied.

Taken together, these three elements of conventionalism constitute a rejection of those common assumptions of objectivity and rationality that we ascribed to realists and positivists at the end of chapter 2. Let us now consider how the arguments we have examined in this chapter are related to them. The Duhemian and Quinean objections to the simple logical model of falsification would give support to, respectively, weaker and stronger forms of (3). Both indicate the difficulties in formulating the kinds of criteria whose existence is denied in (2), though much depends on whether the concept of rationality is restricted to that of logical proof. The attacks on a theory-neutral observational language would support an extreme form of (3) and render inoperative any universal standards which presuppose the existence of theory-free observations. In addition, they could form the basis, together with other philosophical assumptions about the relations between the real and the perceptible, for an ontologically radical version of (1); not only observations, but the world, are theory-dependent. A move of this kind is indicated in these comments by Kuhn (1970a, p. 118):

Lavoisier, we said, saw oxygen where Priestley had seen dephlogisticated air and where others had seen nothing at all. . . . At the very least, as a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he 'saw differently', the principle of economy will urge us to say that after discovering oxygen Lavoisier worked in a different world.

And Kuhn, like Feyerabend, has argued directly in favour of (2). But whereas Feyerabend lays stress on the individual scientist's tastes, attitudes and values, Kuhn is concerned primarily with the shared standards of the scientific community, and their paradigm-relativity.¹⁸

However, the relationships sketched out here are only hypothetical: the actual degree to which these three elements are supported by the arguments we have considered depends upon the validity of those arguments. We have criticized all of them to some extent, and if these

criticisms are correct, either the degree of support is weakened, or less radical versions of conventionalism are established. However, our piecemeal discussion of these arguments has perhaps concealed an important feature of science whose recognition underlies many of the specific claims made by the writers concerned. This is the way in which, during its historical development, there have been recurrent and radical changes in both the theoretical and observational vocabularies of science. It is no accident that most conventionalist-inclined contemporary philosophers have also made contributions to the history of science. In doing so, they have demonstrated the inadequacy of positivist views of its historical development, whether of an inductivist or hypothetico-deductivist nature.¹⁹ But what we will now suggest is that this general feature of science can more readily be accommodated within a realist, than a positivist, conception of the scientific enterprise.

If theories consist either of empirical laws, or of theoretical laws whose non-observational terms can be suitably defined in the doubly-privileged observational language, it is difficult to see why the development of science should be accompanied by major changes in the way that scientists talk about what they observe, and by clearly marked discontinuities in their theoretical conceptions of the physical world. For the realist, these facts are less surprising. The replacement of one theory by another often means the introduction of new ontological classes, and the rejection, as non-existent, of the entities described by the previous theory. An example of this is contained in the last passage from Kuhn that we quoted above: the discovery of oxygen, and the denial of the existence of phlogiston.²⁰ But whereas, for Kuhn, Priestley and Lavoisier are to be seen as 'working in different worlds', the realist maintains that they worked in the same world, whilst their theories made conflicting claims about the nature and existence of some of its constituents.

Let us explore this example in more detail (see the last sections of chapters 2 and 3). Priestley discovered a previously unknown gas by heating mercury calx. He noted some of its properties, especially that it supported rapid combustion, and was respirable. He called the gas 'dephlogisticated air', thereby indicating his explanation of these properties in terms of the phlogiston theory of combustion. Lavoisier, who produced the same gas, having been informed about it by Priestley, rejected this description of it, since he rejected the phlogiston theory. He first called it 'eminently respirable air', and later, 'oxygen'. The latter name derived from Greek terms, and meant 'begetting acid'. This description reflected Lavoisier's belief that oxygen, when combined with, for instance, sulphur, produced sulphuric acid upon combustion. We have here a clear example of how theoretical beliefs are assumed in the descriptive vocabulary of science. But, as we

argued in section 2 of this chapter, when theories conflict, there is normally a way of describing phenomena that is neutral as between the competing theories. Thus, in this case, both Priestley and Lavoisier could agree on some of the gas's properties, whilst disagreeing about their theoretical explanation. There is no reason to regard the two theories, and their 'observational vocabularies', as incommensurable; and only in the most metaphorical sense of 'seeing' is it true that Priestley and Lavoisier 'saw differently', as Kuhn claims. Further, there is no justification for the view that these two scientists 'lived in different worlds'. Lavoisier denied the existence of an entity, phlogiston, that Priestley believed in. But they lived in the same world, and even shared many beliefs about one of its constituents, that 'eminently respirable air' whose properties they explained differently.

We conclude this chapter by considering one other position in the philosophy of science, *instrumentalism*, and its relations with those we have already discussed. We can distinguish two main senses that have been given to this term. First, 'instrumentalism' may refer to a view about the general purpose of scientific enquiry, that it should aim to give us predictive and manipulative power over our physical environment. Thus the justification of scientific activity lies in its practical results, in improving our ability to make useful predictions and changes. Second, the term may denote a view about the logical status of scientific theories, that they are computational devices which generate testable predictions. Theories are instruments, and, as such, only their utility can be assessed, and not their truth or falsity. They do not provide any knowledge of the physical world over and above the predictions that can be derived from them.

Clearly, these two views are closely related. In particular, the first may be seen as providing a strong reason for adopting the second. But either one may be accepted independently of the other, and in what follows we shall be concerned only with the second. Often, a contrast is drawn between instrumentalism and 'realism', as two mutually exclusive and exhaustive views about the nature of science. Here, 'realism' is used in a very broad sense, to denote the view that scientific theories are to be assessed for their truth or falsity, and not merely for their instrumental power: theories are genuine statements, not computational devices. This general contrast has some significance, but it also conceals important similarities and differences. We have already noted the important differences between realism and positivism, which are both termed 'realist' in the instrumentalism-realism dichotomy. But there are also similarities between positivism and instrumentalism.

Consider, for example, the positivist account of scientific explanation. If, as we have argued, this account tends to assimilate predictive

and explanatory knowledge, it may become difficult to distinguish the positivist view of scientific theories as providing true explanations, and the instrumentalist view of theories as devices for the generation of predictions. Of course, the positivist regards the laws in a D-N explanation as either true or false, whilst the instrumentalist considers them only as performing a function in the deductive argument. But it is easy to conflate these two attitudes, or to slide from one to the other. In practice, they tend to generate the same kind of scientific activity, which is distinctly different from that required by the realist position. A similar point can be made about the status of theoretical terms, and the positivists' attempts to reduce these to statements in the observational language. Suppose that these attempts were successful. For the positivist, this would mean that theoretical statements are properly scientific, and can meaningfully be ascribed a truth-value. The instrumentalist, however, could argue that this showed how theories are merely convenient devices, and theoretical terms simply shorthand statements about observables. Alternatively, if these attempts fail, the instrumentalist may continue to accept the use of non-replaceable theoretical terms, on the grounds of their utility in generating testable predictions. Though positivists may be worried by whether theoretical statements are properly scientific, instrumentalists are not concerned by this, since their criterion of scientificity is a purely pragmatic one.

Thus, in this case, the difficulties of the positivist programme for theoretical terms may provide a rationale for the adoption of an instrumentalist view of theories. But whichever of these positions about theoretical terms is adopted, both positivists and instrumentalists have in common their rejection of the realist view, according to which theoretical terms are to be interpreted as referring to actually existent, even if unobservable, items. It is this disagreement which seems to us fundamental.

We can see this further by considering an argument sometimes used to support instrumentalism against realism. It concerns the function of models. The instrumentalist may point out that many models have been used by scientists without regarding them as having the ontological implications typically ascribed to them by the realist. Thus, in the twentieth century, both wave and particle models of light have been employed simultaneously, but they are not viewed, as they were earlier, as competing representations of the nature or constitution of light. Neither model can account for all the relevant phenomena, but each can be used to make successful predictions about different, and restricted ranges of those phenomena. There is no claim that light is composed either of waves or particles: the use of such models has a purely pragmatic, instrumental justification.

But this kind of example does not refute the realist position.

Realists can maintain that the existence of such situations in science normally indicates that no adequate theory has yet been developed. In the absence of an adequate theory, which would embody a model that is not regarded in this pragmatic manner, the realist can accept the use of these stop-gap devices. But this must be seen as an essentially unsatisfactory state of affairs, not one which demonstrates the truth of the instrumentalist position. And the realist may add that, here as elsewhere, the adoption of a realist standpoint injects a strongly dynamic element into scientific practice.²¹

Finally, how should we locate instrumentalism within our classification of positivist, realist, and conventionalist philosophies of science? We can see it best as, in principle, a form of conventionalism, which in practice has several affinities with positivism. It is conventionalist in its denial of the truth or falsity of scientific theories, and its emphasis upon practical interests as a criterion for their acceptance or rejection. But it often comes close to positivism in the kinds of scientific activity that its adoption tends to generate. It is, we suggest, for these reasons that historically, instrumentalist and positivist positions have been so closely intertwined in particular writers.²²

Notes

able, though unlike chapter 1, where we analyse positions that have frequently been maintained by philosophers of science, chapter 2 contains some developments of realist views that have not been advanced in the same form before. Our threefold classification is not intended as complete: see n. 11 to chapter 6, on the *rationalist* conception of science.

1 Positivist philosophy of science

- 1 Thus positivists have analysed scientific theories as 'interpreted formal calculi'. We will not discuss this concept in what follows. For a simple account of its main features, see Introduction to Shapere (1965). A critical assessment by one of its former exponents is in Hempel (1970). For general characterizations and evaluations of twentieth-century positivism, see the papers in Achinstein and Barker (1969). We have tried to avoid the use of symbolic formalizations; but where these occur, they are always preceded by 'translations' in non-symbolic language.
- 2 Hempel's views were first stated in Hempel (1942); then in Hempel and Oppenheim (1948); and more fully, with detailed consideration of objections, in Hempel (1965b). Similar views were proposed in Popper (1959), itself a translation of the 1934 German edition. Important critical discussions include: Scriven (1962), Scheffler, I. (1964), Donagan (1966), and Suchting (1967).
- 3 In the third model, the 'Deductive-Statistical', it is statistical laws that are explained, by deducing them from premisses containing at least one other such law: see Hempel (1965b), pp. 380-1. An example of this, in sociology, will be given in chapter 4, section 4. This contrasts with the I-S model, where it is particular phenomena that are explained.
- 4 We have omitted reference to another popular criterion, that laws 'support counter-factual conditionals', since we believe this has proved fruitless; see Walters (1967) for a useful account. For general discussions of the criteria for scientific laws, see Popper (1959), ch. 3; Nagel (1961) ch. 4; Ayer (1963); Jobe (1967). On the issue of necessity, see n. 19 to chapter 2.
- 5 On the logical problem of induction, see Black (1967). Barker (1957) is a useful analysis of different views about the relations between theories and observations. For discussion of various types of confirmation theory, see Swinburne (1973). Popper's rejection of inductive argument, and defence of hypothetico-deductivism, are in Popper (1959) and (1969a): Medawar (1969) gives very readable support. Chapters 6 and 7 of Achinstein (1971) contain an interesting discussion of several forms of argument from observation to theory, illustrated with case-histories.
- 6 Ashby (1967) is a useful survey of the problems of formulating the positivist criterion; see also Scheffler (1964), pp. 127-62, and the Introduction to Ayer (1946). We have excluded reference to analytic statements, which most positivists regard as non-empirical but meaningful, and important in science. On the difference between criteria of scientificity and meaningfulness, see n. 3 to the Introduction to part 1.

Introduction to part 1

- 1 Losee (1972) is a useful account of the major historical figures and ideas in the philosophy of science. On the realist tradition, see Wallace (1972); on conventionalism in astronomy, Duhem (1969); on Berkeley's positivism, Popper (1969c); on late nineteenth-century writers, Alexander (1964). In this century, philosophy of science has been much influenced by developments in relativity theory and quantum mechanics, but we will not discuss these: Capek (1961) presents most of the issues. Bohm (1957) defends a realist approach to quantum mechanics, in contrast to the positivist interpretation by Heisenberg (1959); Smart (1968), ch. 5, contains a useful discussion from a realist standpoint.
- 2 For a general history of positivist philosophy, see Kolakowski (1972). On logical positivism, and the analytical movement, see Passmore (1968), chs 16 and 18. Examples of scientific realism are Sellars (1963), Smart (1963), and Mandelbaum (1964), though this movement is somewhat heterogeneous.
- 3 Cf. Ayer (1946), whose logical positivism involves a rejection of metaphysics, theology and ethics as spurious, cognitively meaningless forms of 'knowledge'; with Popper (1969a), who argues, against the logical positivists, that what is important is to distinguish between science and non-science, and not between the meaningful and the meaningless. Duhem combines a part-positivist, part-conventionalist view of science with defence of a neo-Aristotelian metaphysics compatible with his Catholicism. He rejects the idea that scientific theories should attempt to *explain* phenomena, on the grounds that explanation means the discovery of unobservable realities beneath the appearances. Explanatory science would therefore be unnecessarily subject to *metaphysical* disputes (see especially (1954), chs 1, 2, and the appendix).
- 4 The term 'realism' is applied to several philosophical doctrines, such as Platonic Realism, that have little connection with our own use of it. In contemporary philosophy of science, the term is sometimes restricted to a particular view about theoretical entities. Our wider use is justifi-

- 7 Strictly speaking, the observation-language contains not only observational terms, but 'logical' terms, such as 'and', 'all', 'if', 'not', etc.: see the Introduction to Shapere (1965) on this. For discussion of strategies not using correspondence rules, such as the use of Craig's Theorem, or Ramsey Sentences, see Hempel (1958); Scheffler (1964), pp. 162-203; and Smart (1968), ch. 5.
- 8 See Spector (1967) for a fuller account of these different attempts to characterize observational terms.
- 9 See the Introduction to Shapere (1965) for an outline of the more technical aspects of these two types of correspondence rule. Our account here, and the examples, are drawn from Shapere. For a critical discussion of the use of operational definitions in psychology, see Taylor, C. (1964), ch. 4: we discuss this in chapter 7, section 3. In Spector (1967), a different but equally common sense is given to 'partial interpretation'.
- 10 For discussion of the many different distinctions that can be drawn, see Spector (1967), and Achinstein (1968), chs 5 and 6. On the problem of distinguishing sets of terms, as against uses of terms, see Suppe (1972).
- 11 See Shapere (1966) pp. 41-4 on these aspects of 'the logic of science', and n. 1 to this chapter on the analysis of theories as interpreted calculi.
- 12 The distinction between logic and psychology is closely related to that between the 'contexts of justification and discovery': on this latter distinction, see Achinstein (1968), pp. 137-41. For support of the logic versus psychology distinction, see Popper (1959), ch. 1; and on his dispute with Kuhn on this issue, see Kuhn (1970b) and Popper (1970). For an extreme dismissal of models and analogies as psychologistic, see Duhem (1954), part I, ch. IV. A fuller discussion of models and analogies occurs in chapter 2, section 2, below.
- 13 See Agassi (1963) for a spirited attack on inductivist historians of science, from a hypothetico-deductivist standpoint. The whole question of the relations between history and philosophy of science is one of the main themes of the papers in Lakatos and Musgrave (1970).
- 14 On the phlogiston and oxygen theories of combustion, see McKie (1952), chs 10-14; Toulmin (1957); Hall (1962), ch. 11; and Freund (1968), ch. 1. Another episode favoured by positivists is the elimination of 'vitalism' from nineteenth-century biology: for a stimulating account of positivist misrepresentation of this, see Benton (1974).
- 15 See Hempel (1969), pp. 185-94, on the several different aspects of the 'unity of science' ideal. On the reduction of scientific theories, see Oppenheim and Putnam (1958), Nagel (1961), and Sklar (1968).
- 2 Realist philosophy of science**
- 1 The contrast here is similar to that drawn by Scheffler, I. (1964) between the 'substantiation' and the 'explanation' of an event's occurrence: see pp. 19-57. It is also closely related to the medieval distinction between 'ratio cognoscendi' and 'ratio essendi': roughly, between a reason for knowing or believing, and a reason for being or existing.
- 2 This account of Hume is a deliberately 'modernized' one, using the concepts of modern empiricism. For a critical discussion of Hume, including this problem of modernization, see Bennett (1971), chs 9-12. Hume's own position is most easily approached in section 7 of 'An Enquiry Concerning Human Understanding', in Hume (1902). The precise relationship between individual causal judgments and regularities is problematic: see ch. 3 of White, M. (1965), and Davidson (1967).
- 3 It is no solution to restrict the concept of causal relations to cases where one kind of event is not only always followed by another, but also always precedes it. For this runs into the problem of causal 'plurality', i.e. of there being more than one possible cause of the same effect. For useful discussions of this generally neglected issue, see Pap (1963), ch. 14 and Gruner (1967a).
- 4 For somewhat different approaches, see Madden (1969), and Maxwell N. (1968). In the latter, it is argued that there is no unintelligibility in causal relations being *logically necessary*.
- 5 See Harré (1964b), and (1970) ch. 4, for this argument about meaning and evidence. Our view of the natural sciences is heavily indebted to Rom Harré's systematic development of the realist position.
- 6 The restriction of science to how questions and descriptions, is sometimes used to protect religion from the claims of science: cf. n. 3 to the Introduction to part 1, and the accompanying text. For a brief discussion of some of these issues, see Edwards (1967).
- 7 For Mill's account of causation, see Mill (1898), book 3. Several refinements have been added in more recent work, such as the requirement of non-redundancy of causal conditions, and the concept of a causal field. On these, and for a seminal discussion of the relation between particular causal conditions and the complete set, see Mackie (1965). M. White's treatment of explanation and causation in history shows the close relation between the D-N model and the Millian view of causes as sufficient conditions: see M. White (1965), especially chs 3-4.
- 8 On the use of models and analogies in science, see Hesse (1963), Spector (1965), Achinstein (1968), chs 7-8, and Harré (1970), ch. 2. Discussion of this issue is beset by terminological differences between writers. Our distinctions between source, subject and model follow those of Harré; on 'representational' models as physical objects, see Achinstein (1968) pp. 209-11.
- 9 On idealizations in science, see Nagel (1961) pp. 129-45, and Shapere (1969). Hempel (1952) compares their use in natural and social science. In chapter 5, section 5, below, Marx's use of abstraction and idealization is examined and compared with Weber's.
- 10 For a lively account of this use of models as hypothetical mechanisms, see Harré (1961). The form of argument involved is often termed 'retroductive inference': on this, see Hanson (1965), ch. 4, and Achinstein (1971), ch. 6.
- 11 The inadequacy of inductive arguments for arriving at hypotheses about unobservables is discussed in Barker (1957), ch. 5 and Mandelbaum (1964), ch. 2. On the nature and status of analogical argument,

- see Hesse (1963), and Achinstein (1971) ch. 6. It should be noted that some existential hypotheses have the characteristic of being verifiable, but not falsifiable: unlike general laws, the discovery of positive instances may be conclusive. On this asymmetry, see Watkins (1958). This undermines the rationale for any general commitment to falsification: cf. section 2 of the previous chapter. On the importance of existential hypotheses, see Harré (1961).
- 12 We have omitted any discussion of the sense of 'model' in mathematics, often used by positivists in their accounts of scientific theories, when viewing them as interpreted calculi. On this, see Achinstein (1968) ch. 8.
 - 13 In fact, the restrictive criterion of scientificity that we have ascribed to positivists was abandoned fairly soon in this century, and less restrictive formulations substituted for it. It is arguable that these do not generate the positivist 'problem of theoretical terms': but discussions of theoretical terms long continued as if this problem did exist. Why this is so is an interesting historical question.
 - 14 On this realist interpretation of correspondence rules, see Schaffner (1969), and Harré (1970), ch. 1. For general defences of the realist view of theoretical terms as making ontological commitments, see Maxwell, G. (1962), and Smart (1968), ch. 5. We have used the example of 'magnetic' as a theoretical term for the sake of simplicity. Since, though, it is a *dispositional* term, not directly referring to an entity such as an atom, it might be thought appropriate to give its meaning via hypothetical statements about observations. But even with dispositional terms, we believe a realist analysis is preferable. For defence of realism about dispositions, see Armstrong (1968), ch. 6.
 - 15 For an interesting general discussion of the nature of definitions in science, see Achinstein (1968), chs 1-2. In a recent article which marks a considerable departure from positivist views, Hempel (1970) argues that the demand for precise definitions of theoretical terms is unfounded. On the verificationist theory of meaning, see Passmore (1968), chs 16 and 18.
 - 16 Harré (1964a), pp. 8-20, distinguishes 'reticular' and 'explanatory' theories, denying explanatory power to the former. The distinction is similar to that between 'abstractive' and 'hypothetical' theories; see Nagel (1961), pp. 125-8. Hempel (1965b), pp. 247-54, regards the explanatory power of non-causal laws such as those of pendular motion as good grounds for not restricting the D-N model's laws to *causal* laws: cf. section 1 of the previous chapter, p. 12.
 - 17 It is doubtful, however, whether positivist views of explanation and theory adequately represent even reticular laws and theories.
 - 18 Cf. the comments on descriptive and prescriptive approaches to science in section 4 of the previous chapter. One difficulty with basing philosophical theories on scientific practice is that this practice is itself guided by (often implicit) philosophical views. A good example is quantum mechanics: see n. 1 to the Introduction to part 1.
 - 19 Kneale (1961) contains an influential argument for the need of the concept of necessity to characterize scientific laws. The argument is convincingly re-stated by Molnar (1969).

- 20 Some realists might be willing to accept this attenuated version of positivism whilst emphasizing that, in practice, the realist position generates a significantly different type of theoretical activity in science than the less-attenuated type of positivism leads to. Others might argue that the ultimate level of explanation need not be in terms of brute regularities; Harré, for example, argues for a universe made up of point-powers, in (1970), chs 10-11. On the finitude of nature, and its relevance for the philosophy of science, see Kneale (1968).
- 21 For Popper's rejection of essentialism, see his (1969b), pp. 103-7. More will be said about the reality-appearance distinction in parts 2 and 3, especially in section 1 of chapter 8.
- 22 Another form of essentialism will be discussed in chapter 4, section 3. For an interesting defence of the Aristotelian view of scientific explanation, involving a concept of essence, see Brody (1972): he argues that this avoids the problems of the D-N model. In chapter 9, we return briefly to the issue of natural kinds, in discussing the relations between language and reality.
- 23 On the early development of the gene-concept, see Goodfield (1969). On phlogiston and oxygen theories, see n. 14 to chapter 1.

3 Forms of conventionalism

- 1 Cf. the discussion of verification, confirmation and falsification in chapter 1, section 2, and chapter 2, section 2. On the verification of existential hypotheses, see n. 11 to chapter 2.
- 2 See Hempel (1966), pp. 3-6. Chapters 2 and 3 of this book contain a useful account of the logic of theory-testing, with several scientific examples.
- 3 Duhem's and Quine's positions are sometimes mistakenly conflated in what is called 'the Duhem-Quine Thesis'. For criticism of this conflation, and exegesis of Duhem's position, see Laudan (1965). Grünbaum (1966) argues against both positions, claiming that Quine's view is only true if trivial. Assumptions introduced solely to 'save' a theory are often termed *ad hoc*.
- 4 For examples demonstrating this historical point, see Swinburne (1964). The phrase 'naive methodological falsification' is Lakatos's: see his (1970), where the issue of Popper's changing views is also discussed, pp. 180-4. Notice that 'abandoning' a theory is ambiguous between (a) no longer regarding it as *true*, and (b) no longer *using* it.
- 5 Feyerabend's reasons for this last claim will emerge at the end of the next section. For his defence of proliferation, see his (1963a): his criticisms of Lakatos are in his (1970). It should be noted that Lakatos's main concern is slightly different from the way we have presented his views: he wishes to distinguish 'progressive and degenerating problem shifts'. Also, his Popperian concept of 'corroboration' is not strictly equivalent to that of 'confirmation'.
- 6 For similar accounts to Hanson's, see Toulmin (1961), chs 3-6; Feyerabend (1962), (1963a); Kuhn (1970a), especially ch. 10. Many of their arguments are inspired by the later writings of Wittgenstein, especially (1963), part 2, sec. xi. The theory-dependence of meanings is

- also maintained by these writers, particularly Feyerabend, and the arguments here display an interesting isomorphism to those about perception. For criticism of these views about meaning, see Putnam (1965), Shapere (1966), and Achinstein (1968), pp. 91-105. For criticisms of both forms of attack on theory-neutrality, see Scheffler, I. (1967), and Kordig (1971).
- 7 Our account of Hanson's position is based upon his (1958): he defends a similar view in (1969). Unlike, for example, Kuhn, he is not *explicitly* concerned to challenge the function of observation as an objective control for theory-comparisons. In what follows, the terms 'theory' and 'theoretical belief' are used in a deliberately imprecise and general sense. Cf., with the present section, the arguments against a single theoretical-observational dichotomy, in chapter 1, section 3.
- 8 This argument depends upon his claim that seeing involves seeing *that* something is the case: "Seeing that" threads knowledge into our seeing' (1958, p. 22).
- 9 This is why, at the end of chapter 1, section 3, we claimed that the issues of theory-neutrality, and epistemological certainty, must be clearly separated. The position we have defended here is partly based on that of Hooker (1973). See also Dretske (1969), for defence of a concept of 'non-epistemic seeing', i.e. seeing without presuppositions of knowledge or belief.
- 10 For Feyerabend on proliferation, see his (1963). The necessity for challenging the theoretical assumptions of observational descriptions is emphasized by Mepham (1973a), who talks of 'the theoretical critique of the facts'. A related problem is whether, for the purposes of such 'theoretical critiques', we should regard *ordinary language* as itself a theory, which is open to revision by scientific discoveries. On this, in the context of the mind-body problem, see Feyerabend (1963b), and Rorty (1965).
- 11 But note that the first edition of *The Structure of Scientific Revolutions* appeared in 1962, before Lakatos's position was fully developed. Our account of Kuhn is mainly based on this edition: in the second, a *Postscript* is added, containing several refinements and partial retractions, for example on the concepts of paradigms and of seeing: further 'second thoughts' are in his (1970c). Exegesis of Kuhn is made difficult by his frequent use of weakening qualifications: on this feature, see the important review-article by Shapere (1964). We have kept to the stronger versions of his claims. Useful critical discussions are in Scheffler, I. (1967), Lakatos and Musgrave (eds) (1970), and Kordig (1971). We discuss further aspects of Kuhn's position in sections 2 and 3 of chapter 9.
- 12 For Kuhn, paradigms are also incommensurable because of meaning-changes in the theoretical and observational vocabularies, and because, in some sense, scientists operating within different paradigms inhabit 'different worlds'. On the latter, see Section 4 below: on the former, see n. 6 above. It seems doubtful whether Kuhn can consistently maintain, as he often does, that paradigms are both incommensurable and incompatible: see, e.g. Kordig (1971), pp. 52-9.

- 13 On seventeenth-century theories of light, and their methodological assumptions, see Sabra (1967): especially chs 6, 8 and 11, on Huygens's wave theory, and Newton's (implicitly) corpuscular position. It should also be noted that there were many distinctive standpoints within a generally corpuscularian framework, which strongly affected disputes about what counted as a *substantively* adequate 'ultimate explanation': but much 'normal science' was carried on despite these differences. See also Dijksterhuis (1961), part IV, and Mandelbaum (1964), chs 1-2.
- 14 The conflict between their views on 'normal science' is apparent if one compares the title of Kuhn (1963), 'The Function of Dogma in Scientific Research', with Feyerabend's defence of an 'anarchistic epistemology' in his (1970). But Feyerabend, like Kuhn, is a strong advocate of meaning-variance: see n. 6 above. For a recent attempt to develop an *evolutionary* view of the growth and development of science and human knowledge, see Toulmin (1972).
- 15 See Feyerabend (1970), p. 72. For this feature of Popper's position, see Magee (1973), chs 6 and 7. Thus, underlying the methodological disagreements between Popper, Kuhn, and Feyerabend are important *political* implications: the three have been taken as providing defences for, respectively, liberal-democracy, totalitarianism, and anarchism - though Feyerabend now prefers the label 'dadaism': see footnote 33 to his (1970).
- 16 For a similar point, see Hooker (1972), a sympathetic review of Feyerabend's work. The contrast we have drawn between 'rational standards' and 'practical guides' is too simple as it stands: for example, Lakatos's 'sophisticated methodological falsificationism' (section 1 above), and the attempts to formulate a 'logic of confirmation' (see n. 5 to chapter 1), fall somewhere between the two.
- 17 Kolakowski's definition is mainly related to nineteenth-century writers, in a chapter significantly entitled 'Conventionalism: Destruction of the Concept of Fact' (1972, ch. 6). Our elaborations on it reflect the changing concerns and assumptions of more recent philosophers of science. Many earlier conventionalists were interested in the extent to which scientific laws involved definitional, rather than empirical, truths: on these writers, see Alexander (1964). There was also concern with the relations between language and reality: see chs 10-11 of Poincaré (1958). 'Conventionalism' is sometimes applied to the view that logical principles are based on conventions: see, e.g., Ayer (1946), ch. 4.
- 18 For a partial withdrawal of his 'different worlds' claim, see *Postscript* to Kuhn (1970a), pp. 192-3: and in his (1970c), pp. 259-66, he emphasizes how individual scientists may apply their shared standards differently. The relation between aesthetic or moral values, and the objectivity or rationality of science, is more complex than we have presented it. For example, one frequently advocated criterion for theory-choice is simplicity: should this be seen as a subjective, or objective standard? This partly depends on which of the numerous criteria of simplicity is used, and for what reason: on these issues, see the symposium in *Philosophy of Science*, 28, pp. 109-71, especially

- Rudner (1961). We return to the issues of values, objectivity, and the theory-dependence of reality, in chapter 9.
- 19 On positivists and the history of science, see section 4 of chapter 1. Historical studies by the writers examined include: Duhem (1917) and (1969), Hanson (1958), (1963), and Kuhn (1959).
 - 20 The argument outlined here is far from conclusive, and positivists might claim that radical changes in theoretical vocabularies could be accounted for by the discovery of unsuspected general laws.
 - 21 Using the distinction made between two senses of 'abandonment' in n. 4 above, the realist sees this use of models as indicating abandonment (a), but not (b). Another argument against realism by instrumentalists concerns the function of idealizations: for the realist view on this, see chapter 2, section 2. On the general contrast between realism and instrumentalism, see Popper (1969b), and Nagel (1961), ch. 6, who argues that the difference between them concerns only 'preferred modes of speech'. This claim is strongly rejected in G. Maxwell (1962).
 - 22 Thus Kolakowski (1972) correctly treats conventionalism as a 'chapter' in the history of positivism; and the tradition of viewing astronomical theories as 'saving the phenomena' can be seen as part-instrumentalist, part-conventionalist: see the Introduction to part 1. But most instrumentalists would be opposed to a denial of the theory-neutrality of the observational facts predicted by theories. The variety of definitions given by philosophers of science to such terms as 'positivism', 'realism', 'instrumentalism' and 'conventionalism' is somewhat confusing, and our own differ from several others: but what matters is the specific content of the positions these terms are used to denote.

Introduction to part 2

- 1 Positivist interpretations of Marx were common at the end of the last century and at the beginning of this; best known are Bukharin, Kautsky and Plekhanov. A set of papers which criticizes this interpretation and support an alternative humanistic one is Fromm (1967).
- 2 A good example of a social scientist who explicitly adopts a single distinctive position in the philosophy of science is Homans. See his (1964) where he adopts a Hempelian positivism which he then uses to criticize sociological orthodoxy.
- 3 The account that would be given by a hypothetico-deductivist of the history of social science would of course be different again; see our discussion of these writers in section 4, of chapter 1. There are no histories of social thought which are based on the types of methodological distinction that we make. On the Scottish Enlightenment see Swingewood (1970).

4 Sociology and positivism

- 1 On the general history of Comtean positivism see Simon (1963). For Comte's own writings the most convenient source is *Positive Philosophy* vols 1 and 2 (Comte, 1853). Two useful and interesting nineteenth-

- century discussions, both relevant to the next section, are Mill (1965) and Spencer (1968). Three modern methodological discussions are in Aron (1968), Kolakowski (1972), and Laudan (1971). On the role of sociology in the Comtean system, see Swingewood (1970). The fact that the following account is only of Comte should not be read as historically minimizing the role and importance of Saint-Simon. We will not enter into the debate as to whether the Comtean system is merely the systematization and elaboration of basically Saint-Simonian arguments. Furthermore, our account is mainly of Comte's theses as set out in the *Cours de Philosophie Positive*, written between 1830 and 1842.
- 2 We will discuss aspects of methodological individualism in chapter 8.
 - 3 In general, see Mill (1898), book VI, 'On the Logic of the Moral Sciences', as well as Mill (1965). The best methodological discussion is in Ryan (1970), especially chapter 1 on deduction, chapter 3 on induction, and chapter 4 on whether or not he is suitably classified as an empiricist, epistemologically. Also see Chapter 2, section 1 above, for a discussion of the Millian approach to causation.
 - 4 Spencer's own views can best be seen in Spencer (1874, 1883, 1893), some of which is included in Andreski (1971). Very useful discussions are in Eisen (1967) and Peel (1971). For other aspects of his work see Barker (1915) and Burrow (1966).
 - 5 This reorientation of social thought is interestingly and helpfully discussed in Hughes (1959), although his use of the term 'positivism' is much broader than ours set out in chapter 1. We discuss positivism and behaviourism in chapter 7, section 3.
 - 6 For Durkheim's own writings see especially *The Rules of Sociological Method* (Durkheim, 1938); also Durkheim (1915, 1933, 1952). We are now fortunate in having available Steven Lukes's work on Durkheim (Lukes, 1973). The other most useful methodological discussions are in Alpert (1961) and Douglas (1967). On Durkheim's possibly changing views on the character of social facts, see Parsons (1968, vol. 1). Useful general discussions are in Giddens (1971, 1972).
 - 7 We will discuss aspects of this issue in chapter 8. There is anyway much controversy. Some writers, such as Alpert (1961) argue that we can only grasp Durkheim's thesis if we distinguish carefully between the different notions of the 'individual' that he uses. Other writers, Giddens (1971) for example, do not think this so important. In general on this topic see Alpert (1961, p. 149f.) and Lukes (1973, pp. 11-15).
 - 8 We should note that *Elementary Forms* is a rather different work from the other two, partly at least because it is based upon a functionalist mode of explanation.
 - 9 Durkheim's method of rejecting alternative explanations apart from his own suffers from two defects. First, he may very well not reject *all* the possible explanations so it does not follow that his own, the sociological, is left as the only alternative. Second, Durkheim's method of presentation excludes the possibility that the alternative explanations are interdependently plausible and only implausible when given as separate and mutually incompatible. We examine his rejection of