Introduction

Any major development in the philosophy of science has implications for many other philosophical concerns. The theory of knowledge, ethics and the philosophies of art and religion, for example, have been profoundly affected by changes in our understanding of science. Of course, the influence is not simply in one direction; the philosophy of science has itself been moulded by ideas from these other domains.

Some people might find this claim of reciprocal influence between distinctive philosophical areas surprising. One proposition that would be uncontroversial, however, is that the philosophy of social science has depended on developments in the philosophy of science. Each philosophy of science sets narrow limits for what is acceptable as a philosophy of social science. Yet a philosophy of social science is formulated normally after the main ideas of the parent philosophy of science have been exhaustively studied; and often after it has become evident to specialists in the philosophy of science that there is something wrong with the model of science in question.

The philosophy of social science, then, typically has the fate of being behind the times and on the point of change, a fate which is depressing only if we want cut and dried intellectual answers. This means that this book's central question is both familiar and novel. I am concerned with whether the study of human society can satisfy natural scientific methodology. Throughout I pose the question in the following terms. Can social study conform to a *naturalistic* methodology, that is replicate the methodology of natural science? Is *naturalism*, the doctrine that there can be a natural scientific study of society, correct? *Anti-naturalism* is the view that there cannot be a study of society modelled on natural science. *Non-naturalistic* social studies are based on methodologies other than that of natural science, but are not necessarily committed on the issue of whether there can also be a naturalistic social science. Hermeneutic methodologies claim to sustain types of non-naturalistic study.

These issues are familiar in that they have always been fundamental

to the philosophy of social study. They are novel because there are many naturalisms, since there are many philosophies of natural science which might serve as models for the study of society. Those who ask whether social study can model itself on natural science use an identical set of words to pose a changing question. Failure to understand this leads repeatedly to the plea that we should forget the whole issue, a plea which is always ignored.

I cannot hope to set out in full the philosophy of science which I assume in my discussion of naturalism. To do so would not only require a separate book, but also involve much superfluous discussion, because the philosophy of science disputes issues, such as whether or not it is essential for theories to be interpreted by means of models, that do not have to be settled for my purpose.

My strategy is to borrow, as the content of the naturalism I will discuss, the key ideas from recent philosophy of science. This philosophy of science contributed to and developed from the demise of the strongly empiricist philosophy of science which held sway roughly until the mid-1950s. As a term of convenience, we can call this new philosophy of science the 'post-empiricist' philosophy of science.[1]

The key ideas I borrow are uncontroversial ones within post-empiricist philosophy of science. They are uncontroversial in that most disputants within contemporary philosophy of science accept them in some form, and in that they could be abandoned only in the context of a movement to an entirely different philosophy of science. Feyerabend's more Feyerabendian statements, for example, are uncontroversial in neither of these senses.

The two following points indicate the nature of the background philosophy of science I will assume in my discussion of naturalism. First, to call a study a science implies that there is an empirical constraint on the acceptability of its statements, that the testing of its statements against the world is at least one strong criterion for the acceptance or rejection of those statements. Secondly, scientific theories are much more holistic structures than was previously realised, in a sense which includes the point that the meaning of a term is partly determined by its relations with other terms in its theory; and, partly in consequence of this holism, there is no absolutely pre-theoretical observation language relevant to the conduct of science. In saying that a term's meaning is partly determined by its relations with other terms in its theory, we mean that limitations are placed on the sense and reference of terms used within a theory. The limitations occur because the term's sense and reference are modified as a result of

[1] Dispassionate accounts of recent developments in the philosophy of science are to be found in Achinstein (1968), Shapere (1966), Sheffler (1967) and Williams (1977).

the term coming into logical contact with other terms in the theory. These modifications provide, for example, new criteria for the identification of and new expectations concerning an object. This contextual view of meaning leads to a holistic interpretation of theoretical structure and then to the denial of existence to a pre-theoretical observation language, once it becomes apparent that there is no basis for holding that some terms derive their meaning in isolation from other terms. In particular, there is no basis for believing that any term is meaningful in virtue of an immediate relationship with the extra-linguistic world in abstraction from other terms in the language. It follows that there is no statement concerning which the decision about truth-status is independent of assumptions about other statements' truth-statuses.

With this insight, the issue that dominated empiricist philosophy of science – the analysis of the observation terms and statements which were held to be in an immediate relationship with the world – has to be re-stated. For since we no longer acknowledge pre-theoretical observation statements, we cannot study the nature of observation statements or their relations with theoretical statements. The resolution of this issue in any case looked increasingly unlikely from within empiricist philosophy of science, because many different grounds were adduced for distinguishing observation and theoretical statements, all of which were vague and most of which were in conflict with one another. In contrast to empiricist approaches, it became clear that what were considered absolutely empirical terms have their meanings partly determined by their relations with other terms in the theory, including terms in the most abstract reaches of the theory. What relatively empirical statements claim is, likewise, in part a function of the other statements in the theory.

There is thus a real sense in which the statements in a theory stand or fall together. In considering any statement within a theory, we must consider the relations of that statement with other of the theory's statements and, in principle, with the whole theory. How precisely we are to analyse the sense in which statements are dependent on one another is, however, a problem for current philosophy of science; it turns, in part, on our initial formulation of the critique of pre-theoretical observation items. This is a large area which I cannot enter here.

We started from the empirical point that a scientific theory is about the world. We then criticised the empiricist interpretation of this claim, namely that there are some terms and statements whose meaning is given immediately by reference to the extra-linguistic world in abstraction from all other terms and statements. These two starting points for post-empiricist philosophy of science encapsulate its fundamental

promise and dilemma: it seeks to be empirical without being empiricist. As a first approximation, the claims that a scientific theory is about the world and that it should be considered as a whole unit, can be combined in the view that it is the theory as a whole that is assessed for correspondence with the world. This view indicates the right direction, but does little more than that. The problem of how post-empiricist models of science can be empirical is the most basic issue facing current philosophy of science. It is an unresolved issue and it is one which we will continuously run up against in this book.

The term 'theory' has already occurred a great deal in these opening pages. It is necessary to say something about it, because it is a key term in this work. Three separate points ought to be noted in arriving at an understanding of what a theory is. First, there is much to be learnt from empiricist philosophy of science in the analysis of the internal structure of a scientific theory. It depicted a scientific theory as a layered structure, with the higher level statements incorporating and explaining those on the lower levels. There is a reciprocal evidential relation between the layers, in that higher level statements might stand as support for lower level statements and vice versa. As an ideal, the language of a theory should be formalised and the links between the layers deductive.

If we keep in mind the ideal typical nature of this account – that, for example, no empirical theory is fully axiomatic or deductive – we can put it to good use in understanding scientific theory. What we must and can discard, for reasons already indicated, is the idea that there is an absolute break in a theory's structure between the theoretical and pre-theoretical terms and statements. This idea is not essential to analyses of different levels in a scientific theory's structure. All such accounts presuppose is a notion of *relatively* theoretical and *relatively* empirical terms and statements.

This leads to the second point about theories, which I have already discussed. The notion of theoretical terms and statements cannot be invoked in absolute contradistinction to the empirical. The meanings of *prima facie* empirical terms and statements are partly determined by the meanings of all the other terms and statements in a theory. There is no absolutely pre-theoretical observation language; in analysing the meanings or judging the claims of relatively empirical terms and statements, we constantly return to the avowedly theoretical.

A tension exists between the first, layered view of theories and the second, holistic view. In contrast to the layer model, the holistic view lends itself to a network model for scientific theory, in which there are no hierarchical relations between terms and statements in the theory. Investigations into the possibilities, if any, of reconciling layered and

network accounts form a major part of current philosophy of science. Suffice it here to say that there is a chance of reconciliation if we accept that any designated hierarchical relations are not absolute. That is, which items are designated 'high level' and which 'low level' depends on what we are studying and when we analyse the theory in question. Nevertheless, as long as we accept these qualifications on delineations of levels within a theory, it remains possible and useful to talk of higher and lower level items.

Yet, and this is the third point, we need a clear way of distinguishing the relatively empirical from the relatively theoretical. To rest with the claim that all terms and statements are equally theoretical creates more problems than it solves. We need to be able to distinguish a relatively empirical layer as a way of accounting for the fact that scientific theory makes statements about the world. The sorts of question to be posed in deciding which terms and statements are relatively theoretical are as follows: how systematic are they, in the sense of which layers do they subsume and explain? How general and idealised are they? What relations of testability do they bear to other items in the theory? Can they be abandoned without fundamentally revising the theory? What are their relations to the terms and statements with which the theory appears to confront the world more or less directly? These different criteria may often yield different judgments as to the relative theoretical load of an item in a theory. One criterion applied to an item at different times for different purposes might even produce different answers. This is to acknowledge that the designation of one item as 'theoretical' and another as 'empirical' is relative, provisional and shifting within the context of a theory, as are even the most gross functional differentiations between, for example, explanation and classification.

This interpretation of theory allows us to talk sensibly of everyday actors' theories, commonsense theories and implicit theories. They comprise a network of terms and statements, internally related, not in an isolatedly immediate relationship with the world. Moreover, commonsense statements which are relatively systematic and general can be loosely delineated. Where commonsense theory differs from scientific theory is in terms of the first point I made about theories. Virtually by definition, commonsense theories are not rigidly structured, formalised or deductive. Nor do commonsense theories adopt movement towards more rigid structure as a controlling methodological ideal; in many ways, this prescriptive formulation is more important than a descriptive account of the point about lack of structure, because most natural scientific theories also, as a matter of fact, lack rigid structure. These ideas are central to chapters two and three.

A key aspect of the assumptions I use is that they rest on a notion of a unified, holistic theory. Just as it is difficult to give identity criteria for natural languages, so too the decision as to where one theory ends and another begins is not governed by explicit criteria and is often fairly arbitrary. The philosophy of science I adopt requires theoretical holism, because it stresses how terms and statements, previously considered to be common between theories, are structured by specific theoretical options. Theoretical holism might be held more plausible of natural than social science, since natural scientific theories tend to be more explicit and formalised. However, for reasons that should become apparent in chapter four, theoretical holism is relevant to social science because of the extreme difficulty in giving an account of a relatively empirical layer which might be common between two social scientific theories.

I do not claim that natural scientific methodology is the only possible model for social study. Various types of study of society are feasible. History, the study of literature or journalism might be taken as its model disciplines. This suggestion is clearly complicated, however, by the fact that there is methodological uncertainty about the proposed alternative model disciplines, as is evident from the debate that followed Hempel's (1959) attempted extension of a naturalistic model of explanation to history. Even despised journalism sustains its methodological controversies: Wolfe (1973) describes how a 'new journalism' arose out of, *inter alia*, a critique of the accepted notions of the given and of an objective point of view.

More generally, different studies of man could be constructed on the basis of different aims, methodologies and epistemologies. Different conceptions of the status of the student of society might be crucial in this respect. There is a broad division between those studies where the student is seen as an observer and those where he is seen as a participant. Most naturalistic methodologies assume the observer model of the student, while some hermeneutic methodologies use the participant model. This distinction cannot be made in terms of the distinctive objects of social study: for instance, the naturalistic student can 'observe', in the sense relevant to science, 'meaningful' social entities as well as physical objects (*pace* Earle, 1952–3), and a naturalistic social science can admit 'intentional categories' (*pace* Dunn, 1978). The observer and the participant are differentiated, not by the objects they can recognise, but by their different methodologies, aims and criteria of knowledge. The participant's distinctive aims are, first, to acquire the practical and cognitive social skills of the people of his study and, secondly, to enter into full interaction with these people, modifying himself and them in the process.

The fashionable view that a naturalistic methodology based on the student as observer has to 'objectify' the actor under study in some pernicious epistemological-cum-moral sense is mistaken. Certainly, the actor becomes an 'object' of social science in the trivial sense that he is studied and discussed in social scientific theories. But the actor is not necessarily reduced to an 'object' by naturalistic social science in two more interesting senses which are fundamental to the vague protest of 'objectification'. First, the social scientist need not (though he might) conclude that the actions he studies are out of the control of their supposed agents; Hollis's claim, in *Models of Man*, that a naturalistic social science cannot have a theory of the active subject is wrong in this respect. Secondly, the social scientist need not treat the actors' conceptual systems as different in principle from his own. Both these points are themes of this book. They will be discussed mainly in chapters two and three; in chapter three, I will also say a little more about the observer and participant models of the student of society.

It is also a reasonable view that the tradition of modelling social study on natural science has been an unhelpful one. This issue has to be argued again for each new philosophy of science. I wish simply to defend naturalism, which is the doctrine of the possibility of a natural scientific study of society, and to draw out some of the implications for social science if it were to adopt certain views in current philosophy of science as its naturalistic methodology. Only in drawing out these implications do I set forth prescriptions and even then the prescriptions are conditional, not categorical. That is, some of my arguments are of the form: one must hold certain views, *if* one wishes to adopt from the philosophy of science the naturalistic methodology in question. In defending naturalism, I am bound to criticise those elements of anti-naturalism which imply the impossibility of naturalism. But how one might compare the various methodologies of the study of man directly, if at all, is something I do not discuss. Suffice it here to make two points.

First, an advocate of a naturalistic social science would emphasise features such as the unification of knowledge (by means of generalisations, explanations, predictions, etc.) which would be yielded by a powerful naturalistic theory, as against the fragmented insights of non-naturalistic approaches. I suspect that non-naturalistic approaches are not merely fragmented. They are non-existent, in that no satisfactory account of a non-naturalistic social study has been given and no substantive body of non-naturalistic social studies exists. But it would take another book, on non-naturalism and social study, to support this suspicion properly.

Secondly, an element in the comparison of the various

methodologies would be the elucidation of the different interests on which the types of social study are based. That would be to show, for the level of methodology, what will be a central thesis of my discussion of values in social science: namely, values have a foundational role in the formation of the human studies. In *Knowledge and Human Interests*, Habermas analyses the different sorts of interests that may underlie naturalistic and non-naturalistic methodologies. From this book's point of view, Habermas's empirical-analytic, historical-hermeneutic and critical sciences are three *alternative* possible ways of conceptualising the study of man. On a more empirical level, Hawthorn's *Enlightenment and Despair* advances hypotheses about the political conditions of the presence of sociology in France and Germany, where there was a need for a moral science of social progress to mediate between anarchy and reaction; and of the absence of sociology in England, where liberal reformism had practical strength. Hawthorn appears, however, to make two doubtful assumptions: first, that sociology's basic value commitments are always liberal and reformist, and, secondly, that sociology experiences rapid development only when these value commitments are threatened.

This work, then, is doubly conditional. I argue that *if* my background philosophy of science is accepted and *if* the project of a naturalistic social science is deemed possible and fruitful, then social science should take the lines I sketch out. In fact, a certain tension is revealed between the body of this work and an antecedent of the condition governing it. For, as I have already indicated, I repeatedly run into a problem which is shown to be highly intractable, namely the key issue facing modern philosophy of science, that of giving an account of science's claim to be empirical. It may be, as I moot in the Conclusion, that setting out modern philosophy of science's implications for social science helps us to see that modern philosophy of science is itself ultimately untenable.

I focus, within the philosophical framework I have just described, on the problems posed by meaning and value for naturalistic social science. This focus arises from my belief that meaning and value are the areas where the most serious arguments against the possibility of a naturalistic social science, and where the most interesting implications for social science of the naturalistic methodology I adopt are located. My concentration on issues of meaning and value results in several traditional problems of the philosophy of social science (e.g. methodological individualism, the psychology–sociology and history–sociology relations, functionalism) being tackled only indirectly or in passing. Besides repeating the positive reasons for the focus I have chosen, I would justify these omissions by arguing that the

problems concerned are not central to the question of the possibility of a naturalistic social science. They deal directly with the distinctive subject matter of social study (e.g. whether it is directed to social or individualistic entities), not with the philosophical status of that study. However, I will discuss these problems where I consider them to have *prima facie* damaging implications for the project of a naturalistic social science.

I discuss meaning and value in chapters two to five. Chapter one asks whether social science can replicate the internal structure and central achievements of natural scientific theories. It thus treats issues such as the possibility of empirical generalisations, theories and predictions in social science. Chapter one is in a sense ground-clearing, allowing me to dismiss certain topics from the discussion of naturalism, before moving to the deeper problems of meaning and value. But in another sense it is a fundamental part of this work. For my analyses of meaning and value are designed to show that, although there are senses in which social science is distinctively concerned with meanings and values, there is no reason why social study should not be scientific, if our model of science is derived from recent philosophy of science. Having established this point, social scientific theories are then free to model themselves on the kinds of aims and internal structure discussed in chapter one.

Some of the analyses of science I appeal to in chapter one were achieved before the more recent work in the philosophy of science which I assume as an overall background. This discrepancy in models of science is not serious for two reasons. First, recent philosophy of science stands in the empirical tradition, so it is not surprising that there are areas of agreement between it and the work of the previous generation. Secondly, perhaps a more precise way of making the same point, the strongly empiricist philosophy of science of the previous generation concentrated on the internal structure of scientific theories, often, moreover, on the structure of the relatively empirical layers of theories. Recent philosophy of science has shifted attention to an overall characterisation of the nature of scientific theory, without having to disagree with all of the detailed analyses of the previous generation. We can expect, for example, to find in Hempel much that is still interesting and sound on explanation.

Writers in the philosophy of social science should be self-conscious about the nature of the philosophy being undertaken and the status of the claims being made. To say that this is a work in the philosophy of social science is in a sense ambiguous, because there are two kinds of philosophy that are relevant to social science. The first might be called 'methodology', by which I mean what typically falls under the heading

of the philosophy of science: namely, reflection on the structure and methods, whether of construction or of validation, of scientific theories in general. For many purposes, methodological reflection can be thought of as second-order, in that most scientists need not engage in it during their scientific work. This may not be the case, however, with natural sciences at the frontiers of research, where, for example, the nature of experimentation and of theory and the aims of explanations may become live issues. It certainly is not the case with social science, which has always been subject to methodological controversy. This work presupposes the relevance of methodology to social science, in the sense that the outcome of methodological debate will substantively affect the conduct of social science. But the very methodology whose relevance to social study I support, the natural scientific one, sets limits to the implications that methodological controversy carries for social scientific conduct. It is characteristic of natural scientific methodology that it is sensitive to the boundaries between methodology and substantive scientific theory, and that it invokes those boundaries in order to dissolve what have been thought to be methodological issues. This is a strategy I will follow at times.

Methodology has always to state whether it is prescriptive or descriptive. Prescriptive methodology lays down standards which scientists are enjoined to follow. Descriptive methodology sets out the methodology adopted by successful science. A satisfactory methodology must, in fact, be both prescriptive and descriptive. This can be achieved by constructing a methodology which plausibly claims to exhibit the structure of successful science, and which then draws the reasonable conclusion that if science wishes success in future, it must continue to follow the methodology in question. Such an approach to methodology, then, rests on views about the history of the area of reasoning being investigated, views which can be supported only by the use of realistic examples. Part of my reluctance to make categorical claims about background philosophies of science stems from the fact that I am not able to make informed judgments about the structure exhibited by successful natural science.

A separate issue is the place of idealised prescriptions *within* the overall descriptive-and-prescriptive methodology described in the last paragraph. A methodology advocating prescriptions about scientific reasoning based on an analysis of the history of science may or may not decide that scientific reasoning is governed by norms, which, though unattainable, still constructively affect scientific conduct. The ideals that scientific theory should be fully formalised and that explanation should be fully deductive are possible examples. This point will recur in chapter one.