Introduction

Philosophers of science have always been exercised by the problem of the extent to which scientific theories may be said to describe things that exist. We can characterize this problem as one of *reference*. It might be posed as a question: what can we say, on the basis of our scientific theories, about what there is in the world? The fundamental tenet of the realist's view of science is that those things referred to in a theory of an established science do exist. There are gravitational fields, muons and black holes, just as there are planets, mammals and bacteria. A second traditional problem has been that of whether it makes sense to describe scientific theories as true or false. This problem can be characterized as one of *predication*. Again the realist's answer is clear: the statements contained in a scientific theory are either true or false. For any given statement we may not now be in a position to say which it is, but we can at least conceive of what it would be for it to be true or false.

Though distinct, the problems of reference and predication are not divorced, for one way of explaining what it is for a statement to be true is by showing how things in the world can be as it says they are. According to such an account, what it means to say that the statement "Some bacteria require oxygen to survive" is true, is that there are such things as bacteria and oxygen and that some of the former require the latter if they are to survive. This form of relation between words and the world is often called a correspondence relation, and a theory of truth based on it a correspondence theory of truth. Although I shall not discuss this theory in detail in the book, the reader should note that it is often thought of as inseparable from, or even a corollary of, the realist's views on the problems of reference and predication.

Of much more interest to us is a third problem, closely related to those of reference and predication though perhaps of more recent concern: that of how we can explain the growth of science. By this is meant how we can explain what makes progress possible in science

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and how such progress comes about. For convenience we might describe this as the problem of *scientific progress*. On the basis of his answers to the problems of reference and predication, a realist is able to offer a plausible explanation. Theories of a science characteristically describe the behaviour of things that actually exist. Scientists, says the realist, act in accordance with this view, for they attempt to provide theories which better explain the behaviour of the same kinds of things already referred to by earlier theories. The progress of science is evidence that such a methodology works. In keeping with this the realist wants to say such things as that what Bohr identified as electrons earlier in this century, modern physicists do too; that most of the things Dalton classified as acids are similarly classified by our chemists; that temperature is nothing more than the measure of the mean kinetic energy of molecules; and that the planets Kepler recognized through Tycho Brahe's observations and to which he applied his laws of motion are just those to which Newton applied his laws of dynamics. The realist's central claim with respect to the problem of scientific progress is that there are many cases where competing or successive theories are about the same things.

The aim of this book is to defend such an account of the growth of science. In recent times it has been attacked from different quarters. Some philosophers of science have maintained that, far from the realist giving a plausible account of scientific progress, it does not even make sense to talk of a cumulative growth of true scientific belief. Such a relativist view has arisen from criticism of the explanations of scientific progress given by positivists and falsificationists. At the beginning of Chapter 1, I shall give a brief outline of this debate. Essentially the relativist holds that there is no one external reality which might be invoked to explain the truth or falsity of statements by members of linguistic communities more or less distant, in space or time, from our own. A correspondence theory of truth is denied. My objective will not be to assess the strength of this position. I shall regard it as a challenge to the realist to explain and defend his view of scientific progress. This I shall begin to do in the second section of the chapter where I pose four interconnected questions which a realist has to answer before his account of scientific progress is vindicated. These set the stage for the rest of the book.

Besides the considerations arising from the historiography of science, realism has been under attack in the philosophy of language.

The theory of reference is of central importance here. In fact, the complex of issues surrounding it constitute the core of this book, for I believe that the realist's account of scientific progress stands or falls with his account of what it is for a term to refer. The middle three chapters are therefore devoted to these matters. Chapter 2 deals with a general pragmatic argument which, if correct, would show that we cannot make sense of the notion of reference as classically conceived. Chapter 3 is concerned with another general argument which, in the present context, suggests that there are severe limitations to our understanding of previous scientific theories. If this argument were correct it would also cast doubt on any so-called "descriptive" theory of reference. This is important because, given my characterization of realism, a theory of reference is necessary for a realist account of scientific progress. In Chapter 4, I shall propose a descriptive theory of reference for scientific terms. It is here that my position directly challenges certain current orthodoxy in the philosophy of language, for descriptive theories of reference are generally thought of as non-realist or even anti-realist. I shall try to show that this need not be so.

In Chapter 5, the realist account which I shall defend is illustrated using three actual cases of theory change from the history of science. These add credence to the initial plausibility of the realist view. This is not to say, however, that every change of theory can be shown to reflect a cumulative growth in true scientific belief about what there is. Each case has to be looked at separately and in the end the truth of the realist view as a whole depends on certain contingent features associated with the interpretation of the theories of previous scientists.

Note. Throughout this book double quotation marks are used for shudder quotes and for mentioning terms while single quotation marks are used for quotations proper.

1

A realist account of scientific progress

1 THE CHALLENGE OF RELATIVISM

During this century, positivist and falsificationist explanations of how scientific progress comes about have given rise to considerable debate. According to the former there are two ways in which a well-confirmed theory T, i.e., one which explains a large number of observed regularities and has led to many successful predictions, comes to be replaced. Firstly, we may extend T in order to predict new phenomena. This, said the positivists, necessitates either the introduction of new "correspondence rules" which define the "theoretical terms" of T, or the supplementing of the theoretical postulates and basic laws; in either case we obtain a closely related new theory T′. We can then test T′ against its predictions: if it proves incorrect we reject it although we are free to retain T; if it proves correct we accept T′ and we are then free to press on to T″, etc. Once we have established a theory like T we can work from it and so increase our stock of scientific knowledge. An example of successful theory extension is the development of the theory of mechanics. Originally it was formulated to describe the motions of point-masses, and was later extended to encompass the motions of rigid bodies.

The second, and more complex, way in which the positivists thought a well-confirmed theory comes to be replaced is when it is reduced to, or subsumed under, a second theory which was originally formulated in a different area. Frequently cited examples were the reduction of thermodynamics to statistical mechanics, the subsumption of the laws of physical optics under quantum mechanics, and the reduction of Kepler's planetary laws and Galileo's terrestrial laws to Newtonian dynamics. Since the theories come from different areas, the problem arises of relating their different terms. "Heat", "temperature" and "entropy", for example, all occur in thermodynamics whereas none of them occurs in statistical mechanics. The positivists' approach to this problem was to require that, with the help of assumptions of some kind relating the "theoretical terms" of the theory being reduced to properties recognized by the theory to which it is being reduced, the laws of the former theory must be logically derivable from the body of the subsequent theory (Nagel 1961: 353 ff.). In terms of our example, thermodynamic laws concerning heat, temperature and entropy must, given certain assumptions, be logically derivable from the theory of statistical mechanics.

This idea of progress via theory reduction has been strongly criticized by Paul Feyerabend. His central claim is that the conditions laid down by the positivists are inapplicable to actual cases of theory change. One example he discusses in detail is the replacement of Galileo's terrestrial physics by Newtonian dynamics (Feyerabend 1962). If the positivists' condition of logical derivability is to be fulfilled, the laws of the former should be logically derivable from the latter. A basic assumption of Galileo's theory is that vertical accelerations in free fall near the earth's surface are constant over any finite (vertical) interval. Given Newton's theory, however, vertical accelerations in free fall are inversely proportional to distance from the earth. Admittedly, the difference may be experimentally indistinguishable, but that has no bearing on the fact that, strictly speaking, the two theories are logically inconsistent.

Feyerabend also challenges the condition, which he says is an immediate consequence of that of logical derivability, that the meanings of primitive descriptive terms remain the same through reduction. As a prime example he takes the reduction of classical mechanics to relativity theory. The classical theory assumes that the mass of a particle is constant and is conserved in all reactions in a closed system. According to relativity theory, however, the mass of a particle is proportional to its velocity relative to a co-ordinate system in which the observations are carried out. To appreciate that what is at issue here is a change in the meaning of the term "mass", one has to examine the structures of the theories and the roles played by the term in both. In the first place, different and (apparently) incompatible equations about mass hold in the two theories. Secondly, relativistic mass is a relation, involving relative velocities, between an object and a co-ordinate system, whereas classical mass is a property of the object itself and independent of its behaviour in co-ordinate systems. Nor will it do to identify the classical mass with

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the relativistic rest mass, for although both may have the same numerical value, they cannot be represented by the same concept (Feyerabend 1965: 169).

Feyerabend's criticisms are not confined to the positivists' attempts to explain scientific progress; he also levels them at those of Sir Karl Popper and others who, for convenience, we might term falsificationists. Popper's conception of the nature of scientific theories and scientific progress was quite different from that of the positivists. According to him a theory or hypothesis is scientific if and only if it can be refuted or falsified by observational evidence. Theories are conjectures, or highly informative guesses about the world, and as such can be submitted to severe critical tests. In practice, however, it is never the case that a theory is rejected simply on the basis of its failing one or more empirical tests. For one theory to be rejected there has to be another which takes its place. The ideal situation is where we have two theories which both explain a large number of observed regularities but which predict different outcomes given the same experimental situation. In such cases we may appeal to 'crucial experiments' to decide between them. Some frequently cited examples of crucial experiments are: the behaviour of Mercury's perihelion, which was used as a crucial piece of evidence in favour of Einstein's theory and against Newton's; Young's two-slit experiment, which supported Huyghens' wave optics against Newton's semi-corpuscular theory; and Michelson and Morley's experiment which was used to discount the theory of the luminiferous ether in favour of the theory of relativity.

Another point of divergence between Popper and the positivists is that Popper rests no weight on the by now rather discredited claim that we can distinguish theoretical from observational scientific terms. For Popper, since any scientific term can support law-like statements, all scientific terms are to some extent theoretical. This naturally leads to an obvious and crucial question: a theory is refuted as a result of observations contrary to those it predicts, but if all terms are theoretical to some degree, what guarantee do we have that an observation report of a crucial experiment will enable us to decide between two *different* theories? Suppose we have two competing theories T^1 and T^2, and a crucial experiment E is proposed to decide between them. The terms used in reporting observations of E will have, according to Popper, some theoretical content; perhaps this content will be derived from T^1. But in that case why should we even suppose that it *could* support T^2 rather than T^1? We are on the brink of what Feyerabend foresees:

> Each theory will possess its own experience, and there will be no overlap between these experiences. Clearly, a crucial experiment is now impossible. It is impossible not because the *experimental device* would be too complex or expensive, but because there is no universally accepted *statement* capable of expressing whatever emerges from observation. (1965: 214)

In outline, then, this is the substance of the attacks on positivist and falsificationist accounts of theory change. For our purposes the most important feature is the emphasis that Feyerabend places on the idea which constitutes perhaps the only thread common to both accounts: that in some way the meaning or import of a scientific term depends upon the theoretical context in which it occurs. The radical conclusion which appears to follow, and which Feyerabend hastens to draw, is that competing or successive theories may use the same terms but in principle there is no way of deciding whether or not they mean the same things by them. Such a situation has been described as one in which the theories concerned are incommensurable.

What bearing does this have on a realist's view of scientific progress? Fundamental to this view is the claim that competing or successive theories are often about the same things. Now Feyerabend defends the view that the ontology or domain of an earlier theory is completely replaced by that of a subsequent one. Strictly speaking, the only entities there are, are the ones contained in the ontologies of the theories we currently accept. To quote Feyerabend, 'introducing a new theory involves changes in the meanings of even the most "fundamental" terms of the language employed' (1962: 29). As a consequence of a rather different analysis of the history of science, Thomas Kuhn has reached a similar view with respect to theories separated by a 'scientific revolution'. An old scientific 'paradigm' is occasionally displaced by a new one, with the result that, in some senses at least, the post-revolutionary scientist finds himself working in a 'different world' (Kuhn 1970: Ch. 10). In short, for the relativist there is no one external reality by which we may measure the truth of our theories.

On the basis of a claim that the meanings of terms are different for different theories, the conclusion is drawn that what those terms refer to is different too. If this argument is valid it amounts to a flat contradiction of the realist's explanation of scientific progress. It is at precisely this point, however, where a claim about meaning becomes a claim about reference, that we must look at recent work in the philosophy of language. In the first instance, at least, the realist's explanation seems only to rely on the concept of reference, for what he wants to show is that different theories are often about the same things, not that the terms they use have the same meaning. Consequently, if he can make good the distinction, it might be possible for him to concede that while we may not be able to decide whether terms mean the same intertheoretically it does not follow from this that we cannot decide whether they have the same reference.[1] This is a thesis which I shall refine in the coming chapters.

Before moving on it is to be noted that Feyerabend's position is not without its critics. It has been pointed out that in discussing the thesis of meaning variance, Feyerabend holds that apparently competing or rival theories may contain incompatible statements. But by maintaining that all the terms of such theories are incommensurable, i.e., that there is in principle no way of showing that they mean the same, it becomes obscure how certain of the statements contained in the theories could be shown to be incompatible (Shapere 1966). If there is no way of telling whether by 'mass' a Newtonian means the same as an Einsteinian, then when the former assents to the sentence "Mass is a constant property of an object" and the latter dissents from it there is no way of telling whether they are even assenting to and dissenting from the same statement; they are talking past each other. Showing that statements from different theories are incompatible appears to involve at least the possibility of *translating* from one to the other. The possibility of translation, however, is precisely what incommensurability denies.

What must be recognized, however, is that criticism of one position does not in itself establish the earlier view which was being brought into question. Scepticism about Feyerabend's relativist claims regarding theory change does not in itself tell in favour of positivism, falsificationism or, more importantly for our purpose, realism. In defence of his claim of sameness of reference, the realist has first to make good the distinction between meaning and reference.

As a result of the work of Frege, such a distinction has been the

[1] An early version of this view was advanced by Israel Scheffler (1967: Ch. 3). It should be noted that although I shall maintain that a theory of reference is a necessary condition for making sense of scientific progress, there are some realists, notably Wilfrid Sellars (1968: 81ff.; 1973), who have pursued a different line.

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subject of extensive discussion in recent philosophy of language. In his 1892 paper 'Über Sinn und Bedeutung', he distinguishes the 'Sinn' or "sense" of a proper name from its 'Bedeutung' or "reference". Reference signifies the relation between a word and an object. It is that which we use the name to talk about and so forms no part of what we might ordinarily think of as meaning. Sense is more closely related to meaning. Knowing the sense of a name – understanding it – enables one in principle to decide which object, if any, the name refers to; but there being a relation in the first place is something different. Frege does not actually talk about sense being part of a more general notion. Nevertheless, it is clear that within the intuitive notion of meaning he thought that a distinction could be drawn between sense and two other ingredients: "force", as in the difference between asserting something and asking whether it is true, and "tone", as in the difference between "sweat" and "perspiration". These last two ingredients, however, will not concern us in this book. We shall be concentrating on sentences in the indicative mood and our analysis will scarcely touch the subtleties of tone.

Before suggesting how we might extend Frege's original insight, let me say a few words about terminology. Many scientific terms are predicates, i.e., expressions of the form ". . . is a ψ" or ". . . is φ", where the blank is to be filled by a singular term such as a name or a definite description to yield a complete sentence. In talking of a singular term I shall sometimes say that it *denotes* an object, by which I mean that it refers to the object, and sometimes that such an object is the *referent* of the term. In the case of a predicate I shall simply talk of its *extension*, by which I mean the set of objects to which it can be correctly applied. The notion of reference is to be understood broadly as signifying not only the relation between a singular term and an object but also the relation between a predicate and a set of objects. So when I talk about a theory of reference for scientific terms this is sometimes to be understood as meaning a theory of the extensions of scientific predicates.

Returning to the problem of theory comparability, suppose that theory T^1 has the following empirical consequence:

(i) $(x)\ (Px \supset Qx)$

Suppose also that an apparent rival, T^2, has the empirical consequence:

(ii) $(\exists x)\ (Px\ \&\ {\sim}Qx)$

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Ordinarily we should maintain that, since (i) and (ii) are logical contradictories, T^1 and T^2 are in conflict. In maintaining this we are committed to holding not that both P and Q have the same *sense* in both T^1 and T^2 but that they have the same *extension*. The reason is that, in extensional logic, to say that (i) and (ii) are contradictories is to say that they could not both be true under any uniform interpretation of the predicates, where by 'uniform interpretation of the predicates' is meant: given a non-empty domain of objects D, and an interpretation I which assigns sets of objects to predicates as their extensions, all occurrences of the same predicate letter under an interpretation I are construed as having the same extension. If it can be established that both P and Q have the same extension as they occur in T^1 and T^2, then they can be represented, as in (i) and (ii), by the same predicate letter in logical form. Finally, no uniform interpretation of the predicates could make both (i) and (ii) true. In order to justify his claim that competing or successive theories can be about the same things, then, the realist's main task is to establish what the extensions of predicates from the theories are and to express this in such a way as to permit comparison.

2 FOUR QUESTIONS FOR REALISM

Let me spell out in more detail the realist account of scientific progress which I shall defend in this book. To begin with we need to be more definite about what sort of predicates are to have their extensions compared. If we look at some typical problematic cases of theory change, we come across questions like "Are the formative elements postulated by Mendel the genes studied by modern molecular biologists?", "Were the atomic theories of Dalton and Avogadro about the same things?", and "When Newton used the term 'gravitational field', did it apply to anything recognized in relativity physics?". Less problematic cases, i.e., ones where sameness of extension is more obvious, are Ptolemaic and Copernican theories of the planets, Aristotle's theory of the brain and that of contemporary neurophysiologists, and even the discovery that whales are mammals and not fish. In all of these cases, the relevant predicate – "gene", "atom", "gravitational field", "planet", "brain", and "whale" – is a *natural kind predicate*. Such predicates, which can be correctly applied to objects on the basis of their physical properties, occupy a central place in scientific theorizing. At