

Introduction

[I]t is easy to indulge the commonplace metaphysical instinct. But a taste for metaphysics may be one of those things which we must renounce, if we mean to mould our lives to artistic perfection. Philosophy serves culture, not by the fancied gift of absolute or transcendental knowledge, but by suggesting questions . . .

Walter Pater, *The Renaissance*

THE opposition between empiricism and realism is old, and can be introduced by illustrations from many episodes in the history of philosophy. The most graphic of these is perhaps provided by the sense of philosophical superiority the participants in the early development of modern science felt toward the Aristotelian tradition. In that tradition, the realists held that regularities in the natural phenomena must have a reason (cause, explanation), and they sought this reason in the causal properties, constituting what they called the substantial forms or natures, of the substances involved in natural processes. The nominalists, who denied the reality of these properties, were in the position of having to reject such requests for explanation.¹

The philosophers engaged in developing the philosophical foundations of modern science had apparently escaped this dilemma. Without postulating such causal properties, forms, or 'occult qualities', they could still explain the regularities that are observed in nature. Thus Robert Boyle writes,

That which I chiefly aim at, is to make it probable to you by experiments, that almost all sorts of qualities, most of which have been by the schools either left unexplicated, or generally referred to I know not what incomprehensible substantial forms, may be produced mechanically; I mean by such corporeal agents as do not appear either to work otherwise than by virtue of the motion, size, figure, and contrivance of their own parts (which attributes I call the mechanical affections of matter).²

To give an account of such phenomena as heat or chemical reactions in terms only of mechanical attributes, they realized quite well,

required at least an atomic theory of matter. But I suppose it is clear that they will face that same dilemma again for the regularities they postulate in the behaviour of the atomic parts. No mechanical explanations are possible there, since the atoms have no further parts. So either they must attribute specific powers, qualities, and causal properties to those atoms to explain why they act and react in the way they actually do; or else they must, like the nominalists before them, reject the request for explanation.

In addition, they have gained a problem. Part of the motivation for the nominalist rejection of the Aristotelian realists' world of powers, properties, dispositions (made famous by Molière's *virtus dormitiva*) was epistemological. The observation of the phenomena did not point unambiguously to the supposed causal connections behind them. This problem exists similarly for the atomic hypotheses: the phenomena do not decide their truth or falsity, though they are perhaps better explained by one hypothesis than by another. Subsequent scientists intent on clarifying the philosophical basis of their discipline found it ever more difficult to reconcile their professed empiricism and antipathy to metaphysics with an unqualified belief in hypotheses that describe a supposed world behind the phenomena.

This led in the nineteenth century to the phenomenalism of Ernst Mach, the conventionalism of Henri Poincaré, and the fictionalism of Pierre Duhem. In the twentieth, the logical empiricism of Hans Reichenbach and logical positivism of Rudolph Carnap were further developments in this radical turn to empiricism.

Today, however, no one can adhere to any of these philosophical positions to any large extent. Logical positivism, especially, even if one is quite charitable about what counts as a development rather than a change of position, had a rather spectacular crash. So let us forget these labels which never do more than impose a momentary order on the shifting sands of philosophical fortune, and let us see what problems are faced by an *aspirant* empiricist today. What sort of philosophical account is possible of the aim and structure of science?

Studies in philosophy of science divide roughly into two sorts. The first, which may be called foundational, concerns the content and structure of theories. The other sort of study deals with the relations of a theory on the one hand, to the world and to the theory-user on the other.

There are deep-going philosophical disagreements about the

general structure of scientific theories, and the general characterization of their content. A current view, not altogether uncontroversial but still generally accepted, is that theories account for the phenomena (which means, the observable processes and structures) by postulating other processes and structures not directly accessible to observation; and that a system of any sort is described by a theory in terms of its possible states. This is a view about the structure of theories shared by many philosophers who nevertheless disagree on the issues concerning a theory's relation to the world and to its users. Opponents of that view will at least say, I think, that this account of what science is like is true 'on the face of it', or correct as a first approximation.

One relation a theory may have to the world is that of being true, of giving a true account of the facts. It may at first seem trivial to assert that science aims to find true theories. But coupled with the preceding view of what theories are like, the triviality disappears. Together they imply that science aims to find a true description of unobservable processes that explain the observable ones, and also of what are possible states of affairs, not just of what is actual. Empiricism has always been a main philosophical guide in the study of nature. But empiricism requires theories only to give a true account of *what is observable*, counting further postulated structure as a means to that end. In addition empiricists have always eschewed the reification of possibility (or its dual, necessity). Possibility and necessity they relegate to relations among ideas, or among words, as devices to facilitate the description of what is actual. So from an empiricist point of view, to serve the aims of science, the postulates need not be true, except in what they say about what is actual and empirically attestable.

When this empiricist point of view was represented by logical positivism, it had added to it a theory of meaning and language, and generally a linguistic orientation. Today that form of empiricism is opposed by scientific realism, which rejects not only the views on meaning of the positivists, but also those empiricist tenets which I outlined in the preceding paragraph. My own view is that empiricism is correct, but could not live in the linguistic form the positivists gave it. They were right to think in some cases that various philosophical perplexities, misconceived as problems in ontology and epistemology, were really at bottom problems about language. This opinion is correct especially, I think, about problems concerning

possibility and necessity. The language of science, being a proper part of natural language, is clearly part of the subject of general philosophy of logic and language. But this only means that *certain* problems can be set aside when we are doing philosophy of science, and emphatically does *not* mean that philosophical concepts must be one and all linguistically explicated. The logical positivists, and their heirs, went much too far in this attempt to turn philosophical problems into problems about language. In some cases their linguistic orientation had disastrous effects in philosophy of science. Scientific realism, however, pursues the antithetical error of reifying whatever cannot be defined away.

Correlative to discussions of the relation between a theory and the world, is the question what it is to accept a scientific theory. This question has an epistemic dimension (how much belief is involved in theory acceptance?) and also a pragmatic one (what else is involved besides belief?). On the view I shall develop, the belief involved in accepting a scientific theory is only that it 'saves the phenomena', that is, correctly describes what is observable. But acceptance is not merely belief. We never have the option of accepting an all-encompassing theory, complete in every detail. So to accept one theory rather than another one involves also a commitment to a research programme, to continuing the dialogue with nature in the framework of one conceptual scheme rather than another. Even if two theories are empirically equivalent, and acceptance of a theory involves as belief only that it is empirically adequate, it may still make a great difference which one is accepted. The difference is pragmatic, and I shall argue that pragmatic virtues do not give us any reason over and above the evidence of the empirical data, for thinking that a theory is true.

So I shall argue for an empiricist position, and against scientific realism. In some ways, philosophy is a subject of fashions—not more so than other intellectual disciplines, I suppose, but at least to the extent that almost any philosopher will begin by explaining that he opposes the 'dominant' or 'received' view, and present his own as revolutionary. It would be quite suspicious therefore if I were to say at this point that scientific realism has become dominant in philosophy of science. Others have certainly characterized it as the emerging victor: Isaac Levi recently wrote, 'My own view is that the coffin of empiricism is already sealed tight.'³ And Arthur Fine, in a reply to Richard Healey:

The objections that he raises to a realist understanding of [quantum mechanics] are ... supposed to move my philosophical colleagues to the same anti-realist convictions that Mr. Healey thinks are held by many physicists. I am not sure how many physicists do hold such anti-realist convictions these days ... I suspect ... that most physicists who do shy away from realism are influenced more by the tradition in which they are schooled than they are by these rather recent and sophisticated arguments. That tradition is the deeply positivist legacy of Bohr and Heisenberg ... I am not much worried that my philosophical colleagues will be seduced by positivist considerations coupled with insubstantial reasons, for we are differently schooled.⁴

There is therefore at least already considerable sentiment on the side of realists that they have replaced the ametaphysical empiricism of the positivists. The empiricist position I mean to advocate will be strongly dissociated from both. (See Chapter 2 §1.2 and Chapter 3 §6 for some remarks on positivism.)

In part my argument will be destructive, countering the arguments brought forward by scientific realists against the empiricist point of view. I shall give a momentary name, 'constructive empiricism', to the specific philosophical position I shall advocate. The main part of that advocacy will be the development of a constructive alternative to scientific realism, on the main issues that divide us: the relation of theory to world, the analysis of scientific explanation, and the meaning of probability statements when they are part of a physical theory. I use the adjective 'constructive' to indicate my view that scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena, and not discovery of truth concerning the unobservable. The baptism of this philosophical position as a specific 'ism' is not meant to imply the desire for a school of thought; only to reflect that scientific realists have appropriated a most persuasive name for themselves (aren't we all scientific, and realists, nowadays?), and that there is after all something in a name.

Arguments Concerning Scientific Realism

The rigour of science requires that we distinguish well the undraped figure of nature itself from the gay-coloured vesture with which we clothe it at our pleasure.

Heinrich Hertz, quoted by Ludwig Boltzmann, letter to *Nature*, 28 February 1895

In our century, the first dominant philosophy of science was developed as part of logical positivism. Even today, such an expression as 'the received view of theories' refers to the views developed by the logical positivists, although their heyday preceded the Second World War.

In this chapter I shall examine, and criticize, the main arguments that have been offered for scientific realism. These arguments occurred frequently as part of a critique of logical positivism. But it is surely fair to discuss them in isolation, for even if scientific realism is most easily understood as a reaction against positivism, it should be able to stand alone. The alternative view which I advocate—for lack of a traditional name I shall call it *constructive empiricism*—is equally at odds with positivist doctrine.

§1. *Scientific Realism and Constructive Empiricism*

In philosophy of science, the term 'scientific realism' denotes a precise position on the question of how a scientific theory is to be understood, and what scientific activity really is. I shall attempt to define this position, and to canvass its possible alternatives. Then I shall indicate, roughly and briefly, the specific alternative which I shall advocate and develop in later chapters.

§1.1 *Statement of Scientific Realism*

What exactly is scientific realism? A naïve statement of the position would be this: the picture which science gives us of the world is a

true one, faithful in its details, and the entities postulated in science really exist: the advances of science are discoveries, not inventions. That statement is too naïve; it attributes to the scientific realist the belief that today's theories are correct. It would mean that the philosophical position of an earlier scientific realist such as C. S. Peirce had been refuted by empirical findings. I do not suppose that scientific realists wish to be committed, as such, even to the claim that science will arrive in due time at theories true in all respects—for the growth of science might be an endless self-correction; or worse, Armageddon might occur too soon.

But the naïve statement has the right flavour. It answers two main questions: it characterizes a scientific theory as a story about what there really is, and scientific activity as an enterprise of discovery, as opposed to invention. The two questions of what a scientific theory is, and what a scientific theory does, must be answered by any philosophy of science. The task we have at this point is to find a statement of scientific realism that shares these features with the naïve statement, but does not saddle the realists with unacceptably strong consequences. It is especially important to make the statement as weak as possible if we wish to argue against it, so as not to charge at windmills.

As clues I shall cite some passages most of which will also be examined below in the contexts of the authors' arguments. A statement of Wilfrid Sellars is this:

to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist.¹

This addresses a question of epistemology, but also throws some indirect light on what it is, in Sellars's opinion, to hold a theory. Brian Ellis, who calls himself a scientific entity realist rather than a scientific realist, appears to agree with that statement of Sellars, but gives the following formulation of a stronger view:

I understand scientific realism to be the view that the theoretical statements of science are, or purport to be, true generalized descriptions of reality.¹

This formulation has two advantages: It focuses on the understanding of the theories without reference to reasons for belief, and it avoids the suggestion that to be a realist you must believe current scientific theories to be true. But it gains the latter advantage by use of the word 'purport', which may generate its own puzzles.

Hilary Putnam, in a passage which I shall cite again in Section 7, gives a formulation which he says he learned from Michael Dummett:

A realist (with respect to a given theory or discourse) holds that (1) the sentences of that theory are true or false; and (2) that what makes them true or false is something external—that is to say, it is not (in general) our sense data, actual or potential, or the structure of our minds, or our language, etc.²⁹

He follows this soon afterwards with a further formulation which he credits to Richard Boyd:

That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories—these statements are viewed by the scientific realist ... as part of any adequate scientific description of science and its relations to its objects.³³

None of these were intended as definitions. But they show I think that truth must play an important role in the formulation of the basic realist position. They also show that the formulation must incorporate an answer to the question what it is to *accept* or *hold* a theory. I shall now propose such a formulation, which seems to me to make sense of the above remarks, and also renders intelligible the reasoning by realists which I shall examine below—without burdening them with more than the minimum required for this.

Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true. This is the correct statement of scientific realism.

Let me defend this formulation by showing that it is quite minimal, and can be agreed to by anyone who considers himself a scientific realist. The naïve statement said that science tells a true story; the correct statement says only that it is the aim of science to do so. The aim of science is of course not to be identified with individual scientists' motives. The aim of the game of chess is to checkmate your opponent; but the motive for playing may be fame, gold, and glory. What the aim is determines what counts as success in the enterprise as such; and this aim may be pursued for any number of reasons. Also, in calling something *the* aim, I do not deny that there are other subsidiary aims which may or may not be means to that end: everyone will readily agree that simplicity, informativeness, predictive power, explanation are (also) virtues. Perhaps my for-

mulation can even be accepted by any philosopher who considers the most important aim of science to be something which only *requires* the finding of true theories—given that I wish to give the weakest formulation of the doctrine that is generally acceptable.

I have added 'literally' to rule out as realist such positions as imply that science is true if 'properly understood' but literally false or meaningless. For that would be consistent with conventionalism, logical positivism, and instrumentalism. I will say more about this below; and also in Section 7 where I shall consider Dummett's views further.

The second part of the statement touches on epistemology. But it only equates acceptance of a theory with belief in its truth.² It does not imply that anyone is ever rationally warranted in forming such a belief. We have to make room for the epistemological position, today the subject of considerable debate, that a rational person never assigns personal probability 1 to any proposition except a tautology. It would, I think, be rare for a scientific realist to take this stand in epistemology, but it is certainly possible.³

To understand qualified acceptance we must first understand acceptance *tout court*. If acceptance of a theory involves the belief that it is true, then tentative acceptance involves the tentative adoption of the belief that it is true. If belief comes in degrees, so does acceptance, and we may then speak of a degree of acceptance involving a certain degree of belief that the theory is true. This must of course be distinguished from belief that the theory is approximately true, which seems to mean belief that some member of a class centring on the mentioned theory is (exactly) true. In this way the proposed formulation of realism can be used regardless of one's epistemological persuasion.

§1.2 *Alternatives to Realism*

Scientific realism is the position that scientific theory construction aims to give us a literally true story of what the world is like, and that acceptance of a scientific theory involves the belief that it is true. Accordingly, anti-realism is a position according to which the aim of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true.

What does a scientist do then, according to these different positions? According to the realist, when someone proposes a theory, he

is asserting it to be true. But according to the anti-realist, the proposer does not assert the theory to be true; *he displays it*, and claims certain virtues for it. These virtues may fall short of truth: empirical adequacy, perhaps; comprehensiveness, acceptability for various purposes. This will have to be spelt out, for the details here are not determined by the denial of realism. For now we must concentrate on the key notions that allow the generic division.

The idea of a literally true account has two aspects: the language is to be literally construed; and so construed, the account is true. This divides the anti-realists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. The anti-realism I shall advocate belongs to the second sort.

It is not so easy to say what is meant by a literal construal. The idea comes perhaps from theology, where fundamentalists construe the Bible literally, and liberals have a variety of allegorical, metaphorical, and analogical interpretations, which 'demythologize'. The problem of explicating 'literal construal' belongs to the philosophy of language. In Section 7 below, where I briefly examine some of Michael Dummett's views, I shall emphasize that 'literal' does not mean 'truth-valued'. The term 'literal' is well enough understood for general philosophical use, but if we try to explicate it we find ourselves in the midst of the problem of giving an adequate account of natural language. It would be bad tactics to link an inquiry into science to a commitment to some solution to that problem. The following remarks, and those in Section 7, should fix the usage of 'literal' sufficiently for present purposes.

The decision to rule out all but literal construals of the language of science, rules out those forms of anti-realism known as *positivism* and *instrumentalism*. First, on a literal construal, the apparent statements of science really are statements, *capable of* being true or false. Secondly, although a literal construal can elaborate, it cannot change logical relationships. (It is possible to elaborate, for instance, by identifying what the terms designate. The 'reduction' of the language of phenomenological thermodynamics to that of statistical mechanics is like that: bodies of gas are identified as aggregates of molecules, temperature as mean kinetic energy, and so on.) On the positivists' interpretation of science, theoretical terms have meaning only through their connection with the observable. Hence they hold

that two theories may in fact *say the same thing* although in form they contradict each other. (Perhaps the one says that all matter consists of atoms, while the other postulates instead a universal continuous medium; they will say the same thing nevertheless if they agree in their observable consequences, according to the positivists.) But two theories which contradict each other in such a way can 'really' be saying the same thing only if they are not literally construed. Most specifically, if a theory says that something exists, then a literal construal may elaborate on what that something is, but will not remove the implication of existence.

There have been many critiques of positivist interpretations of science, and there is no need to repeat them. I shall add some specific criticisms of the positivist approach in the next chapter.

§1.3 Constructive Empiricism

To insist on a literal construal of the language of science is to rule out the construal of a theory as a metaphor or simile, or as intelligible only after it is 'demythologized' or subjected to some other sort of 'translation' that does not preserve logical form. If the theory's statements include 'There are electrons', then the theory says that there are electrons. If in addition they include 'Electrons are not planets', then the theory says, in part, that there are entities other than planets.

But this does not settle very much. It is often not at all obvious whether a theoretical term refers to a concrete entity or a mathematical entity. Perhaps one tenable interpretation of classical physics is that there are no concrete entities which are forces—that 'there are forces such that . . .' can always be understood as a mathematical statement asserting the existence of certain functions. That is debatable.

Not every philosophical position concerning science which insists on a literal construal of the language of science is a realist position. For this insistence relates not at all to our epistemic attitudes toward theories, nor to the aim we pursue in constructing theories, but only to the correct understanding of *what a theory says*. (The fundamentalist theist, the agnostic, and the atheist presumably agree with each other (though not with liberal theologians) in their understanding of the statement that God, or gods, or angels exist.) After deciding that the language of science must be literally understood, we can still say that there is no need to believe good theories to be

true, nor to believe *ipso facto* that the entities they postulate are real.

Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. This is the statement of the anti-realist position I advocate; I shall call it *constructive empiricism*.

This formulation is subject to the same qualifying remarks as that of scientific realism in Section 1.1 above. In addition it requires an explication of 'empirically adequate'. For now, I shall leave that with the preliminary explication that a theory is empirically adequate exactly if what it says about the observable things and events in this world, is true—exactly if it 'saves the phenomena'. A little more precisely: such a theory has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to *all* the phenomena; these are not exhausted by those actually observed, nor even by those observed at some time, whether past, present, or future. The whole of the next chapter will be devoted to the explication of this term, which is intimately bound up with our conception of the structure of a scientific theory.

The distinction I have drawn between realism and anti-realism, in so far as it pertains to acceptance, concerns only how much belief is involved therein. Acceptance of theories (whether full, tentative, to a degree, etc.) is a phenomenon of scientific activity which clearly involves more than belief. One main reason for this is that we are never confronted with a complete theory. So if a scientist accepts a theory, he thereby involves himself in a certain sort of research programme. That programme could well be different from the one acceptance of another theory would have given him, even if those two (very incomplete) theories are equivalent to each other with respect to everything that is observable—in so far as they go.

Thus acceptance involves not only belief but a certain commitment. Even for those of us who are not working scientists, the acceptance involves a commitment to confront any future phenomena by means of the conceptual resources of this theory. It determines the terms in which we shall seek explanations. If the acceptance is at all strong, it is exhibited in the person's assumption of the role of explainer, in his willingness to answer questions *ex cathedra*. Even if you do not accept a theory, you can engage in discourse in a context in which language use is guided by that theory—but acceptance produces such contexts. There are simi-

larities in all of this to ideological commitment. A commitment is of course not true or false: The confidence exhibited is that it will be vindicated.

This is a preliminary sketch of the *pragmatic* dimension of theory acceptance. Unlike the epistemic dimension, it does not figure overtly in the disagreement between realist and anti-realist. But because the amount of belief involved in acceptance is typically less according to anti-realists, they will tend to make more of the pragmatic aspects. It is as well to note here the important difference. Belief that a theory is true, or that it is empirically adequate, does not imply, and is not implied by, belief that full acceptance of the theory will be vindicated. To see this, you need only consider here a person who has quite definite beliefs about the future of the human race, or about the scientific community and the influences thereon and practical limitations we have. It might well be, for instance, that a theory which is empirically adequate will not combine easily with some other theories which we have accepted in fact, or that Armageddon will occur before we succeed. Whether belief that a theory is true, or that it is empirically adequate, can be equated with belief that acceptance of it would, under ideal research conditions, be vindicated in the long run, is another question. It seems to me an irrelevant question within philosophy of science, because an affirmative answer would not obliterate the distinction we have already established by the preceding remarks. (The question may also assume that counterfactual statements are objectively true or false, which I would deny.)

Although it seems to me that realists and anti-realists need not disagree about the pragmatic aspects of theory acceptance, I have mentioned it here because I think that typically they do. We shall find ourselves returning time and again, for example, to requests for explanation to which realists typically attach an objective validity which anti-realists cannot grant.

§2. *The Theory/Observation 'Dichotomy'*

For good reasons, logical positivism dominated the philosophy of science for thirty years. In 1960, the first volume of *Minnesota Studies in the Philosophy of Science* published Rudolf Carnap's 'The Methodological Status of Theoretical Concepts', which is, in many ways, the culmination of the positivist programme. It interprets science by relating it to an observation language (a postulated part

of natural language which is devoid of theoretical terms). Two years later this article was followed in the same series by Grover Maxwell's 'The Ontological Status of Theoretical Entities', in title and theme a direct counter to Carnap's. This is the *locus classicus* for the new realists' contention that the theory/observation distinction cannot be drawn.

I shall examine some of Maxwell's points directly, but first a general remark about the issue. Such expressions as 'theoretical entity' and 'observable-theoretical dichotomy' are, on the face of it, examples of category mistakes. Terms or concepts are theoretical (introduced or adapted for the purposes of theory construction); entities are observable or unobservable. This may seem a little point, but it separates the discussion into two issues. Can we divide our language into a theoretical and non-theoretical part? On the other hand, can we classify objects and events into observable and unobservable ones?

Maxwell answers both questions in the negative, while not distinguishing them too carefully. On the first, where he can draw on well-known supportive essays by Wilfrid Sellars and Paul Feyerabend, I am in total agreement. All our language is thoroughly theory-infected. If we could cleanse our language of theory-laden terms, beginning with the recently introduced ones like 'VHF receiver', continuing through 'mass' and 'impulse' to 'element' and so on into the prehistory of language formation, we would end up with nothing useful. The way we talk, and scientists talk, is guided by the pictures provided by previously accepted theories. This is true also, as Duhem already emphasized, of experimental reports. Hygienic reconstructions of language such as the positivists envisaged are simply not on. I shall return to this criticism of positivism in the next chapter.

But does this mean that we must be scientific realists? We surely have more tolerance of ambiguity than that. The fact that we let our language be guided by a given picture, at some point, does not show how much we believe about that picture. When we speak of the sun coming up in the morning and setting at night, we are guided by a picture now explicitly disavowed. When Milton wrote *Paradise Lost* he deliberately let the old geocentric astronomy guide his poem, although various remarks in passing clearly reveal his interest in the new astronomical discoveries and speculations of his time. These are extreme examples, but show that no immediate

conclusions can be drawn from the theory-ladenness of our language.

However, Maxwell's main arguments are directed against the observable-unobservable distinction. Let us first be clear on what this distinction was supposed to be. The term 'observable' classifies putative entities (entities which may or may not exist). A flying horse is observable—that is why we are so sure that there aren't any—and the number seventeen is not. There is supposed to be a correlate classification of human acts: an unaided act of perception, for instance, is an observation. A calculation of the mass of a particle from the deflection of its trajectory in a known force field, is not an observation of that mass.

It is also important here not to confuse *observing* (an entity, such as a thing, event, or process) and *observing that* (something or other is the case). Suppose one of the Stone Age people recently found in the Philippines is shown a tennis ball or a car crash. From his behaviour, we see that he has noticed them; for example, he picks up the ball and throws it. But he has not seen *that* it is a tennis ball, or *that* some event is a car crash, for he does not even have those concepts. He cannot get that information through perception; he would first have to learn a great deal. To say that he does not see the same things and events as we do, however, is just silly; it is a pun which trades on the ambiguity between seeing and seeing that. (The truth-conditions for our statement '*x* observes *that A*' must be such that what concepts *x* has, presumably related to the language *x* speaks if he is human, enter as a variable into the correct truth definition, in some way. To say that *x* observed the tennis ball, therefore, does not imply at all that *x* observed that it was a tennis ball; that would require some conceptual awareness of the game of tennis.)

The arguments Maxwell gives about observability are of two sorts: one directed against the possibility of drawing such distinctions, the other against the importance that could attach to distinctions that can be drawn.

The first argument is from the continuum of cases that lie between direct observation and inference:

there is, in principle, a continuous series beginning with looking through a vacuum and containing these as members: looking through a windowpane, looking through glasses, looking through binoculars, looking through a low-power microscope, looking through a high-power microscope, etc., in the

order given. The important consequence is that, so far, we are left without criteria which would enable us to draw a non-arbitrary line between 'observation' and 'theory'.⁴

This continuous series of supposed acts of observation does not correspond directly to a continuum in what is supposed observable. For if something can be seen through a window, it can also be seen with the window raised. Similarly, the moons of Jupiter can be seen through a telescope; but they can also be seen without a telescope if you are close enough. That something is observable does not automatically imply that the conditions are right for observing it now. The principle is:

X is observable if there are circumstances which are such that, if X is present to us under those circumstances, then we observe it.

This is not meant as a definition, but only as a rough guide to the avoidance of fallacies.

We may still be able to find a continuum in what is supposed detectable: perhaps some things can only be detected with the aid of an optical microscope, at least; perhaps some require an electron microscope, and so on. Maxwell's problem is: where shall we draw the line between what is observable and what is only detectable in some more roundabout way?

Granted that we cannot answer this question without arbitrariness, what follows? That 'observable' is a *vague predicate*. There are many puzzles about vague predicates, and many sophisms designed to show that, in the presence of vagueness, no distinction can be drawn at all. In Sextus Empiricus, we find the argument that incest is not immoral, for touching your mother's big toe with your little finger is not immoral, and all the rest differs only by degree. But predicates in natural language are almost all vague, and there is no problem in their use; only in formulating the logic that governs them.⁵ A vague predicate is usable provided it has clear cases and clear counter-cases. Seeing with the unaided eye is a clear case of observation. Is Maxwell then perhaps challenging us to present a clear counter-case? Perhaps so, for he says 'I have been trying to support the thesis that any (non-logical) term is a *possible* candidate for an observation term.'

A look through a telescope at the moons of Jupiter seems to me a clear case of observation, since astronauts will no doubt be able to see them as well from close up. But the purported observation

of micro-particles in a cloud chamber seems to me a clearly different case—if our theory about what happens there is right. The theory says that if a charged particle traverses a chamber filled with saturated vapour, some atoms in the neighbourhood of its path are ionized. If this vapour is decompressed, and hence becomes super-saturated, it condenses in droplets on the ions, thus marking the path of the particle. The resulting silver-grey line is similar (physically as well as in appearance) to the vapour trail left in the sky when a jet passes. Suppose I point to such a trail and say: 'Look, there is a jet!'; might you not say: 'I see the vapour trail, but where is the jet?' Then I would answer: 'Look just a bit ahead of the trail ... there! Do you see it?' Now, in the case of the cloud chamber this response is not possible. So while the particle is detected by means of the cloud chamber, and the detection is based on observation, it is clearly not a case of the particle's being observed.

As a second argument, Maxwell directs our attention to the 'can' in 'what is observable is what can be observed.' An object might of course be temporarily unobservable—in a rather different sense: it cannot be observed in the circumstances in which it actually is at the moment, but could be observed if the circumstances were more favourable. In just the same way, I might be temporarily invulnerable or invisible. So we should concentrate on 'observable' *tout court*, or on (as he prefers to say) 'unobservable in principle'. This Maxwell explains as meaning that the relevant scientific theory *entails* that the entities cannot be observed in any circumstances. But this never happens, he says, because the different circumstances could be ones in which we have different sense organs—electron-microscope eyes, for instance.

This strikes me as a trick, a change in the subject of discussion. I have a mortar and pestle made of copper and weighing about a kilo. Should I call it breakable because a giant could break it? Should I call the Empire State Building portable? Is there no distinction between a portable and a console record player? The human organism is, from the point of view of physics, a certain kind of measuring apparatus. As such it has certain inherent limitations—which will be described in detail in the final physics and biology. It is these limitations to which the 'able' in 'observable' refers—our limitations, *qua* human beings.

As I mentioned, however, Maxwell's article also contains a different sort of argument: even if there is a feasible observable/

unobservable distinction, this distinction has no importance. The point at issue for the realist is, after all, the reality of the entities postulated in science. Suppose that these entities could be classified into observables and others; what relevance should that have to the question of their existence?

Logically, none. For the term 'observable' classifies putative entities, and has logically nothing to do with existence. But Maxwell must have more in mind when he says: 'I conclude that the drawing of the observational-theoretical line at any given point is an accident and a function of our physiological make-up, ... and, therefore, that it has no ontological significance whatever.'⁶ No ontological significance if the question is only whether 'observable' and 'exists' imply each other—for they do not; but significance for the question of scientific realism?

Recall that I defined scientific realism in terms of the aim of science, and epistemic attitudes. The question is what aim scientific activity has, and how much we shall believe when we accept a scientific theory. What is the proper form of acceptance: belief that the theory, as a whole, is true; or something else? To this question, what is observable by us seems eminently relevant. Indeed, we may attempt an answer at this point: to accept a theory is (for us) to believe that it is empirically adequate—that what the theory says *about what is observable* (by us) is true.

It will be objected at once that, on this proposal, what the anti-realist decides to believe about the world will depend in part on what he believes to be his, or rather the epistemic community's, accessible range of evidence. At present, we count the human race as the epistemic community to which we belong; but this race may mutate, or that community may be increased by adding other animals (terrestrial or extra-terrestrial) through relevant ideological or moral decisions ('to count them as persons'). Hence the anti-realist would, on my proposal, have to accept conditions of the form

If the epistemic community changes in fashion *Y*, then my beliefs about the world will change in manner *Z*.

To see this as an objection to anti-realism is to voice the requirement that our epistemic policies should give the same results independent of our beliefs about the range of evidence accessible to us. That requirement seems to me in no way rationally compelling; it could be honoured, I should think, only through a thorough-

going scepticism or through a commitment to wholesale leaps of faith. But we cannot settle the major questions of epistemology *en passant* in philosophy of science; so I shall just conclude that it is, on the face of it, not irrational to commit oneself only to a search for theories that are empirically adequate, ones whose models fit the observable phenomena, while recognizing that what counts as an observable phenomenon is a function of what the epistemic community is (that *observable* is *observable-to-us*).

The notion of empirical adequacy in this answer will have to be spelt out very carefully if it is not to bite the dust among hackneyed objections. I shall try to do so in the next chapter. But the point stands: even if observability has nothing to do with existence (is, indeed, too anthropocentric for that), it may still have much to do with the proper epistemic attitude to science.

§3. *Inference to the Best Explanation*

A view advanced in different ways by Wilfrid Sellars, J. J. C. Smart, and Gilbert Harman is that the canons of rational inference require scientific realism. If we are to follow the same patterns of inference with respect to this issue as we do in science itself, we shall find ourselves irrational unless we assert the truth of the scientific theories we accept. Thus Sellars says: 'As I see it, to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist.'⁷

The main rule of inference invoked in arguments of this sort is the rule of *inference to the best explanation*. The idea is perhaps to be credited to C. S. Peirce,⁸ but the main recent attempts to explain this rule and its uses have been made by Gilbert Harman.⁹ I shall only present a simplified version. Let us suppose that we have evidence *E*, and are considering several hypotheses, say *H* and *H'*. The rule then says that we should infer *H* rather than *H'* exactly if *H* is a better explanation of *E* than *H'* is. (Various qualifications are necessary to avoid inconsistency: we should always try to move to the best over-all explanation of all available evidence.)

It is argued that we follow this rule in all 'ordinary' cases; and that if we follow it consistently everywhere, we shall be led to scientific realism, in the way Sellars's dictum suggests. And surely there are many telling 'ordinary' cases: I hear scratching in the wall, the patter of little feet at midnight, my cheese disappears—and I