

## Constructive Empiricism

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*Bas van Fraassen*

Bas van Fraassen's (b. 1941) book *The Scientific Image* (from which the selection below has been excerpted) has done much to keep the realism debate vigorous by presenting and defending a version of anti-realist empiricism that is immune to many of the criticisms of the received view. According to van Fraassen's position, called "constructive empiricism," contemporary scientific theories are to be interpreted literally as referring to entities that are incapable of being observed by the naked human senses. But the constructive empiricist limits her commitment to only what those theories claim concerning what is observable. Claims concerning the unobservable are understood to play only a pragmatic role in developing empirically successful theories; the existence of the unobservables themselves is neither affirmed nor denied.

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### 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

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.'<sup>23</sup> 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

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.<sup>5</sup>

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.<sup>6</sup>

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.<sup>7</sup>

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.<sup>8</sup>

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 formulation 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

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

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'.<sup>11</sup>

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

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 thoroughgoing 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.'<sup>14</sup>

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, but the main recent attempts to explain this rule and its uses have been made by Gilbert Harman.<sup>15</sup> 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 infer that a mouse has come to live with me. Not merely that these apparent signs of mousely presence will continue, not merely that all the observable phenomena will be as if there is a mouse; but that there really is a mouse.

Will this pattern of inference also lead us to belief in unobservable entities? Is the scientific realist simply someone who consistently follows the rules of inference that we all follow in more mundane contexts? I have two objections to the idea that this is so.

First of all, what is meant by saying that we all *follow* a certain rule of inference? One meaning might be that we deliberately and consciously 'apply' the rule, like a student doing a logic exercise. That meaning is much too literalistic and restrictive; surely all of mankind follows the

but realist yearnings were born among the mistaken ideals of traditional metaphysics.

In his book *Between Science and Philosophy*, Smart gives two main arguments for realism. One is that only realism can respect the important distinction between *correct* and *merely useful* theories. He calls 'instrumentalist' any view that locates the importance of theories in their use, which requires only empirical adequacy, and not truth. But how can the instrumentalist explain the usefulness of his theories?

Consider a man (in the sixteenth century) who is a realist about the Copernican hypothesis but instrumentalist about the Ptolemaic one. He can explain the instrumental usefulness of the Ptolemaic system of epicycles because he can prove that the Ptolemaic system can produce almost the same predictions about the apparent motions of the planets as does the Copernican hypothesis. Hence the assumption of the realist truth of the Copernican hypothesis explains the instrumental usefulness of the Ptolemaic one. Such an explanation of the instrumental usefulness of certain theories would not be possible if *all* theories were regarded as merely instrumental.<sup>17</sup>

What exactly is meant by 'such an explanation' in the last sentence? If no theory is assumed to be true, then no theory has its usefulness explained as following from the truth of another one – granted. But would we have less of an explanation of the usefulness of the Ptolemaic hypothesis if we began instead with the premiss that the Copernican gives implicitly a very accurate description of the motions of the planets as observed from earth? This would not assume the truth of Copernicus's heliocentric hypothesis, but would still entail that Ptolemy's simpler description was also a close approximation of those motions.

However, Smart would no doubt retort that such a response pushes the question only one step back: what explains the accuracy of predictions based on Copernicus's theory? If I say, the empirical adequacy of that theory, I have merely given a verbal explanation. For of course Smart does not mean to limit his question to actual predictions – it really concerns all actual and possible predictions and retrodictions. To put it quite concretely: what explains the fact that all observable planetary phenomena fit Copernicus's

theory (if they do)? From the medieval debates, we recall the nominalist response that the basic regularities are merely brute regularities, and have no explanation. So here the anti-realist must similarly say: that the observable phenomena exhibit these regularities, because of which they fit the theory, is merely a brute fact, and may or may not have an explanation in terms of unobservable facts 'behind the phenomena' – it really does not matter to the goodness of the theory, nor to our understanding of the world.

Smart's main line of argument is addressed to exactly this point. In the same chapter he argues as follows. Suppose that we have a theory *T* which postulates micro-structure directly, and macro-structure indirectly. The statistical and approximate laws about macroscopic phenomena are only partially spelt out perhaps, and in any case derive from the precise (deterministic or statistical) laws about the basic entities. We now consider theory *T'*, which is part of *T*, and says only what *T* says about the macroscopic phenomena. (How *T'* should be characterized I shall leave open, for that does not affect the argument here.) Then he continues:

I would suggest that the realist could (say) . . . that the success of *T'* is explained by the fact that the original theory *T* is true of the things that it is ostensibly about; in other words by the fact that there really are electrons or whatever is postulated by the theory *T*. If there were no such things, and if *T* were not true in a realist way, would not the success of *T'* be quite inexplicable? One would have to suppose that there were innumerable lucky accidents about the behaviour mentioned in the observational vocabulary, so that they behaved miraculously *as if* they were brought about by the non-existent things ostensibly talked about in the theoretical vocabulary.<sup>18</sup>

In other passages, Smart speaks similarly of 'cosmic coincidences'. The regularities in the observable phenomena must be explained in terms of deeper structure, for otherwise we are left with a belief in lucky accidents and coincidences on a cosmic scale.

I submit that if the demand for explanation implicit in these passages were precisely formulated, it would at once lead to absurdity. For if the mere fact of postulating regularities, without

saved when they are exhibited as fragments of a larger unity. For that very reason it would be strange if scientific theories described the phenomena, the observable part, in different terms from the rest of the world they describe. And so an attempt to draw the conceptual line between phenomena and the trans-phenomenal by means of a distinction of vocabulary, must always have looked too simple to be good.

Not all philosophers who have discussed the observable/unobservable distinction, by any means, have done so in terms of vocabulary. But there has been a further assumption common also to critics of that distinction: that the distinction is a philosophical one. To draw it, they seem to assume, is in principle anyway the task of the philosophy of perception. To draw it, in principle anyway, philosophy must mobilize theories of sensing and perceiving, sense data and experiences, *Erlebnisse* and *Protokolsaetze*. If the distinction is a philosophical one, then it is to be drawn, if at all, by philosophical analysis, and to be attacked, if at all, by philosophical arguments.

This attitude needs a Grand Reversal. If there are limits to observation, these are a subject for empirical science, and not for philosophical analysis. Nor can the limits be described once and for all, just as measurement cannot be described once and for all. What goes on in a measurement process is differently described by classical physics and by quantum theory. To find the limits of what is observable in the world described by theory *T* we must inquire into *T* itself, and the theories used as auxiliaries in the testing and application of *T*.

We have now come to the 'hermeneutic circle' in the interpretation of science. I want to spell this out in detail, because one might too easily get a feeling of vicious circularity. And I want to give specific details on how science exhibits clear limits on observability.

Recall the main difference between the realist and anti-realist pictures of scientific activity. When a scientist advances a new theory, the realist sees him as asserting the (truth of the) postulates. But the anti-realist sees him as displaying this theory, holding it up to view, as it were, and claiming certain virtues for it.

This theory draws a picture of the world. But science itself designates certain areas in this picture as observable. The scientist, in accepting the

theory, is asserting the picture to be accurate in those areas. This is, according to the anti-realist, the only virtue claimed which concerns the relation of theory to world alone. Any other virtues to be claimed will either concern the internal structure of the theory (such as logical consistency) or be pragmatic, that is, relate specifically to human concerns.

To accept the theory involves no more belief, therefore, than that what it says about observable phenomena is correct. To delineate what is observable, however, we must look to science – and possibly to that same theory – for that is also an empirical question. This might produce a vicious circle if what is observable were itself not simply a fact disclosed by theory, but rather theory-relative or theory-dependent. It will already be quite clear that I deny this; I regard what is observable as a theory-independent question. It is a function of facts about us *qua* organisms in the world, and these facts may include facts about the psychological states that involve contemplation of theories – but there is not the sort of theory-dependence or relativity that could cause a logical catastrophe here.

Let us consider two concrete examples which have been found puzzling. The first, already mentioned by Grover Maxwell, concerns molecules. Certain crystals, modern science tells us, are single molecules; these crystals are large enough to be seen – so, some molecules are observable. The second was mentioned to me by David Lewis: astronauts reported seeing flashes, and NASA scientists came to the conclusion that what they saw were high-energy electrons.

Is there anything puzzling about these examples? Only to those who think there is an intimate link between theoretical terms and unobservable entities or events. Compare the examples with Eddington's famous table: that table is an aggregate of interacting electrons, protons, and neutrons, he said; but that table is easily seen. If a crystal or table is classified by a theory as a theoretically described entity, does the presence of this observable object become evidence for the reality of other, different but similarly classified entities? Everything in the world has a proper classification within the conceptual framework of modern science. And it is this conceptual framework which we bring to bear when we describe any event, including an observation. This does not



when the detour pays off; it is reserved for those detours which yield no practical gain. Even the useless metaphysical baggage may be intriguing, however, because of its potentialities for future use. An example may yet be offered by hidden variable theories in quantum mechanics.<sup>22</sup> The 'no hidden variables' proofs, as I have already mentioned, rest on various assumptions which may be denied. Mathematically speaking there exist hidden variable theories equivalent to orthodox quantum theory in the following sense: the algebra of observables, reduced *modulo* statistical equivalence, in a model of the one is isomorphic to that in a model of the other. It appears to be generally agreed that such theories confront the phenomena exactly by way of these algebras of statistical quantities. On that assumption, theories equivalent in this sense are therefore empirically equivalent. Such hidden variable models have much extra structure, now looked upon as 'metaphysical baggage', but capable of being mobilized should radically new phenomena come to light.

With this new picture of theories in mind, we can distinguish between two epistemic attitudes we can take up toward a theory. We can assert it to be true (i.e. to have a model which is a faithful replica, in all detail, of our world), and call for belief; or we can simply assert its empirical adequacy, calling for acceptance as such. In either case we stick our necks out: empirical adequacy goes far beyond what we can know at any given time. (All the results of measurement are not in; they will never all be in; and in any case, we won't measure everything that can be measured.) Nevertheless there is a difference: the assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics.

[...]

### Empiricism and Scientific Methodology

[...]

#### §2.4 Phenomenology of scientific activity

The working scientist is totally immersed in the scientific world-picture. And not only he – to varying degrees, so are we all. If I call a certain box

a VHF receiver, if I call a fork electro-plated, if I so much as decide to turn on the microwave oven to heat my sandwich in the cafeteria, I am immersed in a language which is thoroughly theory-infected, living in a world my ancestors of two centuries ago could not enter.

In the language-oriented philosophy of science developed by the logical positivists, one could not say this while remaining empiricist. For the empirical import of a theory was defined via a division of its (*sic*) language into a theoretical and a non-theoretical part. This division was a philosophical one, that is, imposed from outside. And you could not limit your endorsement to the empirical import of the theory unless your language remained in principle limited to the non-theoretical part of the theory's language. To immerse yourself fully in the theoretical world-picture, hence to use the full theoretical language without qualm, branded one (once the irreducibility of theoretical terms was realized) with complete commitment to the veracity of that picture.

In the constructive empiricist alternative I have been developing, nothing is more natural, or more to be recommended than this total immersion. For the empirical import of the theory is now defined from within science, by means of a distinction between what is observable and what is not observable drawn by science itself. The epistemic commitment to the empirical import of the theory above (its empirical adequacy) can be stated using the language of science – and indeed, in no other way. It may be the case that I have no adequate way to describe this box, and the role it plays in my world, except as a VHF receiver. From this it does not follow that I believe that the concept of very high frequency electromagnetic waves corresponds to an individually identifiable element of reality. Concepts involve theories and are inconceivable without them, to paraphrase Sellars. But immersion in the theoretical world-picture does not preclude 'bracketing' its ontological implications.

After all, what is this world in which I live, breathe and have my being, and which my ancestors of two centuries ago could not enter? It is the intentional correlate of the conceptual framework through which I perceive and conceive the world. But our conceptual framework changes, hence the intentional correlate of our conceptual framework changes – but the real world is the same world.

- of a theory is not, or might not be true, is reason not to accept it.' The drawback of this alternative is that it leaves open what epistemic attitude acceptance of a theory does involve. This question must also be answered, and as long as we are talking about full acceptance – as opposed to tentative or partial or otherwise qualified acceptance – I cannot see how a realist could do other than equate that attitude with full belief. (That theories believed to be false are used for practical problems, for example, classical mechanics for orbiting satellites, is of course a commonplace.) For if the aim is truth, and acceptance requires belief that the aim is served . . . I should also mention the statement of realism at the beginning of Richard Boyd. 'Realism, Underdetermination, and a Causal Theory of Evidence', *Noûs*, 7 (1973), 1–12. Except for some doubts about his use of the terms 'explanation' and 'causal relation' I intend my statement of realism to be entirely in accordance with his. Finally, see C. A. Hooker, 'Systematic Realism', *Synthese*, 26 (1974), 409–97; esp. pp. 409 and 426.
- 10 More typical of realism, it seems to me, is the sort of epistemology found in Clark Glymour's forthcoming book, *Theory and Evidence* (Princeton: Princeton University Press, 1980), except of course that there it is fully and carefully developed in one specific fashion. (See esp. his chapter 'Why I am not a Bayesian' for the present issue.) But I see no reason why a realist, as such, could not be a Bayesian of the type of Richard Jeffrey, even if the Bayesian position has in the past been linked with anti-realist and even instrumentalist views in philosophy of science.
  - 11 G. Maxwell, 'The Ontological Status of Theoretical Entities', *Minnesota Studies in Philosophy of Science*, III (1962), p. 7.
  - 12 There is a great deal of recent work on the logic of vague predicates; especially important, to my mind, is that of Kit Fine ('Vagueness, Truth, and Logic', *Synthese*, 30 (1975), 265–300) and Hans Kamp. The latter is currently working on a new theory of vagueness that does justice to the 'vagueness of vagueness' and the context-dependence of standards of applicability for predicates.
  - 13 Op. cit., n. 11. In the next chapter I shall discuss further how observability should be understood. At this point, however, I may be suspected of relying on modal distinctions which I criticize elsewhere. After all, I am making a distinction between human limitations, and accidental factors. A certain apple was dropped into the sea in a bag of refuse, which sank; relative to that information it is necessary that no one ever observed the apple's core. That information, however, concerns an accident of history, and so it is not human limitations that rule out observation of the apple core. But unless I assert that some facts about humans are essential, or physically necessary, and others accidental, how can I make sense of this distinction? This question raises the difficulty of a philosophical retrenchment for modal language. This I believe to be possible through an ascent to pragmatics. In the present case, the answer would be, to speak very roughly, that the scientific theories we accept are a determining factor for the set of features of the human organism counted among the limitations to which we refer in using the term 'observable'. The issue of modality will occur explicitly again in the chapter on probability.
  - 14 Op. cit., See n. 5.
  - 15 'The Inference to the Best Explanation', *Philosophical Review*, 74 (1965), 88–95 and 'Knowledge, Inference, and Explanation', *American Philosophical Quarterly*, 5 (1968), 164–73. Harman's views were further developed in subsequent publications (*Noûs*, 1967; *Journal of Philosophy*, 1968; in M. Swain (ed.), *Induction*, 1970; in H.-N. Castañeda (ed.), *Action, Thought, and Reality*, 1975; and in his book *Thought*, Ch. 10). I shall not consider these further developments here.
  - 16 Cf. P. Thagard, doctoral dissertation, University of Toronto, 1977, and 'The Best Explanation: Criteria for Theory Choice', *Journal of Philosophy*, 75 (1978), 76–92.
  - 17 J. J. C. Smart, *Between Science and Philosophy* (New York: Random House, 1968), p. 151.
  - 18 *Ibid.*, pp. 150ff.
  - 19 This point is clearly made by Aristotle, *Physics*, II, Chs. 4–6 (see esp. 196<sup>a</sup> 1–20; 196<sup>b</sup> 20–197<sup>a</sup> 12).
  - 20 Putnam, op. cit., n. 8.
  - 21 Of course, we can ask specifically why the *mouse* is one of the surviving species, how it survives, and answer this, on the basis of whatever scientific theory we accept, in terms of its brain and environment. The analogous question for theories would be why, say, Balmer's formula for the line spectrum of hydrogen survives as a successful hypothesis. In that case too we explain, on the basis of the physics we accept now, why the spacing of those lines satisfies the formula. Both the question and the answer are very different from the global question of the success of science, and the global answer of realism. The realist may now make the *further* objection that the anti-realist cannot answer the question about the mouse specifically, nor the one about Balmer's formula, in this fashion, since the answer is in part an assertion that the scientific theory, used as basis of the explanation, is true. This is a quite different argument . . .