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A Case for Scientific Realism

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When Galileo argued that the familiar patterns of light and shade on the face of the full moon could best be accounted for by supposing the moon to possess mountains and seas like those of earth, he was employing a joint mode of inference and explanation that was by no means new to natural science but which since then has come to be recognized as central to scientific explanation. In a retroduction, the scientist proposes a model whose properties allow it to account for the phenomena singled out for explanation. Appraisal of the model is a complex affair, involving criteria such as coherence and fertility, as well as adequacy in accounting for the data. The theoretical constructs employed in the model may be of a kind already familiar (such as "mountain" and "sea" in Galileo's moon model) or they may be created by the scientist specifically for the case at hand (such as "galaxy," "gene," or "molecule").

Does a successful retroduction permit an inference to the existence of the entities postulated in the model? The instincts of the working scientist are to respond with a strong affirmative. Galaxies, genes, and molecules exist (he would say) in the straightforward sense in which the mountains and seas of the earth exist. The immense and continuing success of the retroductions employing these constructs is (in the scientist's eyes) a sufficient testimony to this. Scientists are likely to treat with incredulity the suggestion that constructs such as these are no more than convenient ways of organizing the data obtained from sophisticated instruments, or that their enduring success ought not lead us to believe that the world actually contains entities corresponding to them. The near-invincible

belief of scientists is that we come to discover more and more of the entities of which the world is composed through the constructs around which scientific theory is built.[1]

But how reliable *is* this belief? And how is it to be formulated? This is the issue of scientific realism that has once again come to be vigorously debated among philosophers, after a period of relative neglect. The "Kuhnian revolution" in the philosophy of science has had two quite opposite effects in this regard. On the one hand, the new emphasis on the *practice* of science as the proper basis for the philosophy of science led to a more sensitive appreciation of the role played by theoretical constructs in guiding and defining the work of science. The restrictive empiricism of the logical positivists had earlier shown itself in their repeated attempts to "reduce" theoretical terms to the safer language of observation. The abandonment of this program was due not so much to the failure of the reduction techniques as to a growing realization that theoretical terms have a distinctive and indispensable part to play in science.[2] It was only a step from this realization to an acknowledgment that these terms carry with them an ontology, though admittedly an incomplete and tentative one. For a time, it seemed as though realism was coming into its own again.

But there were also new influences in the opposite direction. The focus of attention in the philosophy of science was now on scientific change rather than on the traditional topic of justification, and so the instability of scientific concepts became a problem with which the realist had to wrestle. For the first time, philosophers of language were joining the fray, and puzzles about truth and reference began to build into another challenge for realism. And so antirealism has reemerged, this time, however, much more sophisticated than it was in its earlier positivist dress.

When I say 'antirealism', I make it sound like a single coherent position. But of course, antirealism is at least as far from a single coherent position as realism itself is. Though my concern is to construct a case for realism, it will be helpful first to survey the sources and varieties of antirealism. I will comment on these as I go, noting ambiguities and occasional misunderstandings. This will help to clarify the sort of scientific realism that in the end can be defended.

SOURCES OF ANTIREALISM: SCIENCE

CLASSICAL MECHANICS

It is important to recall that scientists themselves have often been dubious about some of their own theoretical constructs, not because of some general antirealist sentiment, but because of some special features of the particular constructs themselves. Such constructs may seem like extra baggage—additional interpretations imposed on the theories themselves—much as the crystalline spheres seemed to many of the astronomers of the period between Ptolemy and Copernicus. Or it may be very difficult to characterize them in a consistent way, a problem that frequently bedeviled the proponents of ethers and fluids in nineteenth-century mechanics.

The most striking example of this sort of hesitation is surely that of Newton in regard to his primary explanatory construct, *attraction*. Despite the success of the mechanics of the *Principia*, Newton was never comfortable with the implications of the notion of attraction and the more general notion of force. Part of his uneasiness stemmed from his theology; he could not conceive that matter might of itself be active and thus in some sense independent of God's directing power. The apparent implication of action at a distance also distressed him. But then, how were these forces to be understood ontologically? *Where* are they, in what do they reside, and does the postulating of an inverse-square law of force between sun and planet say anything more than that each tends to move in a certain way in the proximity of the other?

The Cartesians, Leibniz, and later Berkeley, charged that the new mechanics did not really *explain* motion, since its central notion, *force*, could not be given an acceptable interpretation. Newton was sensitive to this charge and, in the decades following the publication of *Principia*, kept trying to find an ontology that might satisfy his critics.[3] He tried "active principles" that would somehow operate outside bodies. He even tried to reintroduce an ether with an extraordinary combination of properties—this despite his convincing refutation of mechanical ethers in *Principia*.[4] None of these ideas, however, were satisfactory. There were either problems of coherence and fit (the ether) or of specification (the active principles). After Newton's death, the predictive successes of his mechanics gradually stilled the doubts about the explanatory credentials of its central concept. But these doubts did not entirely vanish; Mach's *Science of Mechanics* (1881) would give them enduring form.

What are the implications of this often-told story for the realist thesis? It might seem that the failure of the attempts to interpret the concept of force in terms of previously familiar causal categories was a failure for realism also, and that the gradual laying aside in mechanics of questions about the underlying ontology was, in effect, an endorsement of antirealism. This would be so, however, only if one were to suppose the realist to be committed to theories that permit interpretation in familiar categories or, at the very least, in categories that are immediately interpretable. Naive realism of *this* sort is, indeed, easily undermined. But this is not the view that scientific realists ordinarily defend, as will be seen.

How should Newton's attempts at "interpretation" be regarded, after the fact? Were they an improper intrusion of 'metaphysics', the sort of thing that science today would bar? The term 'underlying ontology' that I have used might mislead here. A scientist *can* properly attempt to specify the mechanisms that underlie his equations. Newton's ether *might* have worked out; it was a potentially testable hypothesis, prompted by analogies with the basic explanatory paradigm of an earlier mechanical tradition. The metaphor of "active principle" proved a fruitful one; it was the ancestor of the notion of field, which would much later show its worth.[5]

In one of his critiques of "metaphysical realism," Putnam argues that "the whole history of science has been antimetaphysical from the seventeenth century on."[6] Where different "metaphysical" interpretations can be given of the same set of equations (e.g., the action-at-a-distance and the field interpretations of Newtonian gravitation theory), Putnam claims that competent physicists have focused on the equations and have left to philosophers the discussion of which of the empirically equivalent interpretations is "right." But this is not a good reading of the complicated history of Newtonian physics. First and foremost, it does not apply to Newton himself nor to many of his most illustrious successors, such as Faraday and Maxwell.[7]

Scientists have never thought themselves disqualified from pursuing one of a number of physical models that, for the moment, appear empirically equivalent. As metaphors, these models may give rise to quite different lines of inquiry, leading eventually to their empirical separation. Or it may be that one of the alternative models appears undesirable on other grounds than immediate empirical adequacy (as action at a distance did to Newton). If prolonged efforts to separate the models empirically are unsuccessful, or if it comes to be shown that the models are in principle empirically equivalent, scientists will, of course, turn to other matters. But this is not a rejection of realism. It is, rather, an admission that no decision can be made in this case as to what the theory, on a realist reading, commits us to.

What makes mechanics unique (and therefore an improper paradigm for the discussion of realism with regard to the theoretical entities of science generally) is that this kind of barrier occurs so frequently there. This would seem to derive from its status as the "ultimate" natural science, the basic mode of explanation of motions. The realist can afford to be insouciant about his inability to construe, for example, "a force of attraction between sun and earth . . . [as] responsible for the elliptical shape of the earth's orbit" in ontological terms, as long as he *can* construe astrophysics to give at least tentative warrant to his claim that the sun is a sphere of gas emitting light through a process of nuclear fusion. There was no way for Newton to know that attempts to interpret force in terms of the simple ontological alternatives he posed would ultimately fail, whereas the ontology of "insensible corpuscles," which he proposes in *Opticks*, would prosper. Each of these ventures was "metaphysical" in the sense that no evidence then available could determine the likelihood of its ever becoming an empirically decidable issue. But it is of such ventures that science is made.

QUANTUM MECHANICS

In the debates between realists and antirealists, one claim that antirealists constantly make is that quantum mechanics has decided matters in their favor. In particular, the outcome of the famous controversy involving Bohr and Einstein, leading to the defeat (in most physicists' eyes) of Einstein, is taken to be a defeat for realism also. Once again, I want to show that this inference cannot be directed against the realist position proper.

Was the Copenhagen interpretation of quantum mechanics antirealist in its thrust?[8] Did Bohr's "complementary principle" imply that the theoretical entities of the new mechanics do not license any sort of existence claims about the structures of the world? It would seem not, for Bohr argues that the world is much more complex than classical physics supposed, and that the debate as to whether the basic entities of optics and mechanics are waves or particles cannot be resolved because its terms are inadequate. Bohr believes that the wave picture and the particle picture are *both* applicable, that *both* are needed, each in its own proper context. He is not holding that from his interpretation of quantum mechanics nothing can be inferred about the entities of which the world is composed; quite the reverse. He is arguing that what can be inferred is entirely at odds with what the classical world view would have led one to expect.

Of course, Einstein was a realist in regard to science. But he was also much more than a realist. He maintained a quite specific view about the nature of the world and about its relationship to observation; namely, that dynamic variables have unique real values at all times, that measurement reveals (or should reveal) these values as they exist prior to the measurement, and that there is a deterministic relationship between successive sets of these values. It was this further specification of realism that Bohr disputed.[9]

It is important to note that Einstein *might* have been right here. There is nothing about the nature of science per se that, in retrospect, allows us to say that Bohr *had* to be right. There could well be a world of which

Einstein's version of realism would hold true. And in the 1930s, it was not yet clear that it might not just be our world. We now know that it is not and, furthermore, that this was implicit from the beginning in certain features of the quantum formalism itself, once this formalism was shown to predict correctly. (J. S. Bell's theorem could, in principle, have been proved as easily in 1934 as in 1964; no new empirical results were needed for it.)

What we have discovered as a result of this controversy is, in the first instance, something about the kind of world we live in.[10] The dynamical variables associated with its macro- and microconstituents are measurement-dependent in an unexpected way. (E. Wigner tried to show more specifically that they are *observer*-dependent, in the sense of being affected by the consciousness of the observer, but few have followed him in this direction.) Does the fact that quantum systems are partially indeterminate in this way affect the realist thesis? Not as far as I can see, unless a confusion is first made between scientific realism and the "realism" that is opposed to idealism, and then the measurement-dependence is somehow read as idealist in its implications. It *does* mean, of course, that the quantum formalism is incomplete by the standards of classical mechanics and that a quantum system lacks some kinds of ontological determinacy that classical systems possessed.

This was what Einstein objected to. This was why he sought an "underlying reality" (specifiable ultimately in terms of "hidden parameters" or the like) which would restore determinism of the classical sort. But to search for a completeness of the classical sort was no more "realist" than to maintain (as Bohr did) that the old completeness could never be regained. Recall that realism has to do with the existence-implications of the theoretical entities of successful theories. Einstein's ideal of physics would have the world entirely determinate against the mapping of variables of a broadly Newtonian type; Bohr's would not. The implications for the realist of Bohr's science are, it is true, more difficult to grasp. But why should we have expected the ontology of the microworld to be like that of the macroworld? Newton's third rule of philosophizing (which decreed that the macroworld should resemble the microworld in all essential details) was never more than a pious hope.

ELEMENTARY-PARTICLE PHYSICS

And this dissimilarity of the macrolevel and microlevel is even plainer when one turns from dynamic variables to the entities which these variables characterize. In the plate tectonic model that has had such striking success in recent geology, the continents are postulated to be carried on large plates of rocky material which underlie the continents as well as the oceans and which move very slowly relative to one another. There is no problem as to what an existence-claim means in this case. But problems do arise when we consider such microentities as electrons. For one thing, these are not particles strictly speaking, though custom dies hard and the label 'elementary-particle physics' is still widely used. Electrons do not obey classical (Boltzman) statistics, as the familiar enduring individuals of our middle-sized world do.

The use of namelike terms, such as 'electron', and the apparent causal simplicity of oil-drop or cloud-track experiments, could easily mislead one into supposing that electrons are very small localized individual entities with the standard mechanical properties of mass and momentum. Yet a bound electron might more accurately be thought of as a state of the system in which it is bound than as a separate discriminable entity. It is only because the charge it carries (which is a measure of the proton coupling to the electron) happens to be small that the free electron can be represented as a independent entity. When the coupling strength is greater, as it is between such nuclear entities as protons and neutrons, the matter becomes even more problematic. According to relativistic quantum theory, the forces between these entities are produced by the exchange of mesons. What is meant by 'particle' in this instance reduces to the expression of a force characteristic of a particular field, a far cry from the hard massy points of classical mechanics. And the situation is still more complicated if one turns to the quark hypothesis in quantum field theory. Though quarks are supposed to "constitute" such entities as protons, they cannot be regarded as "constituents" in the ordinary physical sense; that is, they cannot be dissociated nor can they exist in the free state.

The moral is not that elementary-particle physics makes no sort of realist claim, but that the claim it makes must be construed with caution. The denizens of the microworld with their "strangeness" and "charm" can hardly be said to be imaginable in the ordinary sense. At that level, we have lost many of the familiar bearings (such as individuality, sharp location, and measurement-independent properties) that allow us to anchor the reference of existence-claims in such macrotheories as geology or astrophysics. But imaginability must not be made the test for ontology. The realist claim is that the scientist is discovering the structures of the world; it is not required in addition that these structures be imaginable in the categories of the macroworld.

The form of the successful retroductive argument is the same at the micro- as at the macrolevel. If the success of the argument at the macrolevel is to be explained by postulating that something like the entities of the theory exist, the same ought to be true of arguments at the microlevel. Are there electrons? Yes, there are, just as there are stars and slowly moving geological plates bearing the continents of earth. What are electrons? Just what the theory of electrons says they are, no more, no less, always allowing for the likelihood that the theory is open to further refinement. If we cannot quite imagine what they are, this is due to the distance of the microworld from the world in which our imaginations were formed, not to the existential shortcomings of electrons (if I may so express the doubts of the antirealist).

A STRATEGY FOR SCIENTISTS?

Some of the critics of realism assume that defenders of realism are prescribing a strategy for scientists, a kind of regulative principle that will separate the good from the bad among proposed explanatory models. Since the critics believe this strategy to be defective, they have an additional argument against realism. In their view, nonrealist strategies as often as not work out. Indeed, two such episodes might be said to be foundational for modern science: Einstein's laying aside of ontological scruples in his rejection of classical space and time when formulating his general theory of relativity, and Heisenberg's restriction of matrix mechanics to observable quantities only.[11]

A contemporary example of a similarly non-realist strategy can be found among the proponents of S-matrix theory. Geoffrey Chew defends this approach against its rival, quantum field theory with its horde of theoretical entities, by claiming as an advantage that it has no "implication of physical meaning" and that its ability to dispense with an equation of motion allows it also to dispense with any sort of fundamental entities, such as particles or fields.[12] In some of his later essays, Heisenberg (the original proponent of the S-matrix formalism) dwelt on the choice facing quantum physicists of whether to opt for the Democritean approach, utilizing constituent entities, which has been canonical since the seventeenth century, or the Pythagorean approach, which relies on the resources of pure mathematics alone.[13] Heisenberg argued that the Pythagorean approach is now coming into its own, as the resources of the Democritean physical models are close to exhaustion at the quantum level.[14]

It is important to see why a realist could have supported Chew's effort and why the success of Heisenberg's early matrix mechanics must not be credited to antirealism. The realism/antirealism debate has to do with the assessment of the existential implications of successful theories *already in place.* It is not directed to strategies for *further* development, for deciding among alternative formalisms with respect to their likely future potential. A scientist who is persuaded of the truth of realism *might* very well decide that a fresh start is needed when he cannot find a coherent physical model around which to build a new theory. Positivism of this sort may well be called for in some situations, and the realist need not oppose it.

A realist might even decide that at some point the program of Heisenberg and Chew offers more promise, without repudiating his confidence in constructs that have been validated by earlier work. It is true, of course, that a realist will be less likely to turn in this direction than a non-realist would; the extended successes of the Democritean approach and the knowledge of physical structure it has made possible might weigh more heavily, as a sort of inductive argument, with the realist.

Nevertheless, there is no necessary connection; the defender of realism must not be saddled with a normative doctrine of the kind attributed here. One reason, perhaps, why this sort of confusion occurs is that Einstein's stand against Bohr is so often taken to be the paradigm of realism. And it did, indeed, involve a strongly normative doctrine in regard to the proper strategy for quantum physics. But Einstein's world view included, as I have shown, much more than realism; where it failed was not in its realistic component, but in the conservative constraints on future inquiry that Einstein felt the success of classical physics warranted.

As a footnote to this discussion, it may be worth emphasizing that the realist of whom I speak here is, in the first instance, a philosopher. The qualifier 'scientific' in front of 'realist' should not be allowed to mislead. It is used to distinguish the realism I am discussing from the many others that dot the history of philosophy. The realisms that philosophers in the past opposed to nominalism and to idealism are very different doctrines, and neither is connected, in any straightforward way at least, with the realism being referred to here. In the past, the realism I am speaking of has been most often contrasted with fictionalism or with 'instrumentalism'; but at this point the term is almost hopelessly equivocal.

'Scientific realism' is scientific because it proposes a thesis *in regard to* science. Though the case to be made for it may employ the inference-to-best-explanation technique also used in science, the doctrine itself is still a philosophic one. The scientist qua scientist is not called on to take a stand on it one way or another. Most scientists *do* have views on the issue, sometimes on the basis of much reflection but more often of a spontaneous kind. Indeed, it could be argued that worrying about whether or not their constructs approximate the real is more apt to hinder than to help their work as scientists!

SOURCES OF ANTIREALISM: HISTORY OF SCIENCE

The most obvious source of antirealism in recent decades is the new concern for the history of science on the part of philosophers of science. Thomas Kuhn's emphasis on the discontinuity that, according to him, characterizes the "revolutionary" transitions in the history of science also led him to a rejection of realism: "I can see [in the systems of Aristotle, Newton and Einstein] no coherent direction of ontological development."[15] Kuhn is willing to attribute a cumulative character to the low-level empirical laws of science. But he denies any cumulative character to theory: theories come and go, and many leave little of themselves behind.

Among the critics of realism, Larry Laudan is perhaps the one who sets most store in considerations drawn from the history of science. He displays an impressive list of once-respected theories that now have been discarded, and guesses that "for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring."[16]

To meet this challenge adequately, it would be necessary to look closely at Laudan's list of discarded theories, and that would require an essay in its own right. But a few remarks are in order. The sort of theory on which the realist grounds his argument is one in which an increasingly finer specification of internal structure has been obtained over a long period, in which the theoretical entities function *essentially* in the argument and are not simply intuitive postulations of an "underlying reality," and in which the original metaphor has proved continuously fertile and capable of increasingly further extension. (More on this will follow.)

This excludes most of Laudan's examples right away. The crystalline spheres of ancient astronomy, the universal Deluge of catastrophist geology, theories of spontaneous generation—none of these would qualify. That is not to say that the entities or events they postulated were not firmly believed in by their proponents. But realism is not a blanket approval for all the entities postulated by long-supported theories of the past. Ethers and fluids are a special category, and one which Laudan stresses. I would argue that these were often, though not always, interpretive additions, that is, attempts to specify what "underlay" the equations of the scientist in a way which the equations (as we now see) did not really sanction. The optical ether, for example, in whose existence Maxwell had such confidence, was no more than a carrier for variations in the electromagnetic potentials. It seemed obvious that a vehicle of some sort *was* necessary; undulations cannot occur (as it was often pointed out) unless there is something to undulate! Yet nothing could be inferred about the carrier itself; it was an "I-know-not-what," precisely the sort of unknowable "underlying reality" that the antirealist so rightly distrusts.

The theory of circular inertia and the effluvial theory of static electricity were first approximations, crude it is true, but effective in that the metaphors they suggested gradually were winnowed through, and something of the original was retained. Phlogiston left its antiself, oxygen, behind. The view that the continents were static, which preceded the plate-tectonic model of contemporary geology, was not a theory; it was simply an assumption, one that is correct to a fairly high approximation. The early theories of the nucleus, which assumed it to be homogeneous, were simply idealizations; it was not known whether the nucleus was homogeneous or not, but a decision on that could be put off until first the notion of the nuclear atom itself could be fully explored. These are all examples given by Laudan. Clearly, they need more scrutiny than I have given them. But equally clearly, Laudan's examples may not be taken without further examination to count on the antirealist side. The value of this sort of reminder, however, is that it warns the realist that the ontological claim he makes is at best tentative, for surprising reversals *have* happened in the history of science. But the nonreversals (and a long list is easy to construct here also) still require some form of (philosophic) explanation, or so I shall argue.

SOURCES OF ANTIREALISM: PHILOSOPHY

According to the classic ideal of science as demonstration which dominated Western thought from Aristotle down to Descartes, hypothesis can be no more than a temporary device in science. Of course, one can find an abundance of retroductive reasoning in Aristotle's science as in Descartes', a tentative working back from observed effect to unobserved cause. But there was an elaborate attempt to ensure that *real* science, *scientia propter quid*, would not contain theoretical constructs of a hypothetical kind. And there was a tendency to treat these latter constructs as fictions, in particular the constructs of mathematical astronomy. Duhem has left us a chronicle of the antirealism with which the medieval philosophers regarded the epicycles and eccentrics of the Ptolemaic astronomer.

EMPIRICISM

As the bar to hypothesis gradually came to be dropped in the seventeenth century, another source of opposition to theoretical constructs began to

appear. The new empiricism was distrustful of unobserved entities, particularly those that were unobservable in principle. One finds this sort of skepticism already foreshadowed in some well-known chapters of Locke's *Essay Concerning Human Understanding*. Locke concluded there (Book IV) that a "science of bodies" may well be forever out of reach because there is no way to reason securely from the observed secondary qualities of things to the primary qualities of the minute parts on which those secondary qualities are supposed to depend. Hume went much further and restricted science to the patterning of sense impressions. He simply rejects the notion of cause according to which one could try to infer from these impressions to the unobserved entities causing them.

Kant tried to counter this challenge to the realistic understanding of Newtonian physics. He argued that entities such as the "magnetic matter pervading all bodies" need not be perceivable by the unaided senses in order to qualify as real.[17] He established a notion of cause sufficiently large to warrant causal inference from sense-knowledge to such unobservables as the "magnetic matter." Even though the transcendental deductions of the first *Critique* bear on the prerequisites of possible experience, 'experience' must be interpreted here as extending to all spatiotemporal entities that can be causally connected with the deliverances of our senses.[18]

Despite Kant's efforts, the skeptical empiricism of Hume has continued to find admirers. The logical positivists were attracted by it but were sufficiently impressed by the central role of theoretical constructs in science not to be quite so emphatic in their rejection of the reality of unobservable theoretical entities. The issue itself tended to be pushed aside and to be treated by them as undecidable; E. Nagel's *The Structure of Science* gives classical expression to this view. This sort of agnosticism alternated with a more definitely skeptical view in logical positivist writings. If one takes empiricism as a starting point, it is tempting to push it (as Hume did) to yield the demand not just that every claim about the world must ultimately rest on sense experience but that every admissible entity must be directly certifiable by sense experience.

This is the position taken by Bas van Fraassen. His antirealism is restricted to those theoretical entities that are in principle unobservable. He has no objection to allowing the reality of such theoretical entities as stars (interpreted as large glowing masses of gas) because these are, in his view, observable in principle since we could approach them by spaceship, for example. It is part of what he calls the "empirical adequacy" of a stellar theory that it should predict what we would observe should we come to a star. This criterion, which he makes the single aim of science, is sufficiently broad, therefore, to allow reality-claims for any theoretical entity that, though at present unobserved, is at least in principle directly observable by us. His antirealism has more than a tinge of old-fashioned nominalism about it, the rejection of what he calls an "inflationary metaphysics" of redundant entities.[19] Since neither of the two main arguments he lists for realism, inference to the best explanation and the common cause argument, are (in his view) logically compelling, this is taken to justify his application of Occam's razor.

One immediate difficulty with this position is, of course, the distinction drawn between the observable and the unobservable. Since entities on one side of the line are ontologically respectable and those on the other are not, it is altogether crucial that there be some way not only to draw the distinction but also to confer on it the significance that van Fraassen attributes to it. In one of the classic papers in defense of scientific realism, Grover Maxwell argued in 1962 that there is a continuum in the spectrum of observation from ordinary unaided seeing down to the operation of a high-power microscope.[20] Van Fraassen concedes that the distinction is not a sharp one, that 'observe' is a vague predicate, but insists that it is sufficient if the ends of the spectrum be clearly distinct, that is, that there be at least some clear cases of supposed interaction with theoretical entities which would not count as "observing."[21] He takes the operation of a cloud chamber, with its ionized tracks allegedly indicating the presence of charged entities such as electrons, to be a case where "observe" clearly ought not be used. One must not say, on noting such a track: I observed an electron.

To lay as much weight as this on the contingencies of the human sense organs is obviously problematic, as van Fraassen recognizes. There are organisms with sense-organs very different from ours that can perceive phenomena such as ultraviolet light or the direction of optical polarization. Why could there not, in principle, be organisms much smaller than we, able to perceive microentities that for us are theoretical and able also to communicate with us? Is not the notion 'observable in principle' hopelessly vague in the face of this sort of objection? How can it be used to draw a usable distinction between theoretical entities that do have ontological status and those that do not? Van Fraassen's response is cautious:

> 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*).[22]

So 'observable' means here "observable in principle by us with the sense organs we presently have." But once again, why would 'unobservable' in this sense be allowed the implications for epistemology and

ontology that van Fraassen wants to attach to it?[23] The question is not whether the aim of science ought to be broadened to include the search for unobservable but real entities, though something could be said in favor of such a proposal. It is sufficient for the purposes of the realist to ask whether theories that are in van Fraassen's sense empirically adequate can also be shown under certain circumstances to have likely ontological implications.

Van Fraassen allows that the moons of Jupiter can be observed through a telescope; this counts as observation proper "since astronauts will no doubt be able to see them as well from close up."[24] But one cannot be said to "observe" by means of a high-power microscope (he alleges) because no such direct alternative is available to us in this case. What matters here is not so much the way the instrument works, the precise physical or theoretical principles involved. It is whether there is also, in principle, a direct unmediated alternative mode of observation available to us. The entity need not be observable *in practice*. The iron core that geologists tell us lies at the center of the earth is certainly not observable in practice; it is a theoretical entity since its existence is known only through a successful theory, but it may nonetheless be regarded as real, van Fraassen would say, because *in principle* we could go down there and check it out.

The quality of the evidence for this geological entity might, however, seem no better than that available for the chromosome viewed by microscope. Van Fraassen rests his case on an analysis of the aims of science, in an abstract sense of the term 'aim', on the "epistemic attitude" (as he calls it) proper to science as an activity. And he thinks that reality-claims in the case of the chromosomes, but not the iron core, lie outside the permissible aims of science. Is there any way to make this distinction more plausible?

REFERENCE

Some theoretical entities (such as the iron core or the star) are of a kind that is relatively familiar from other contexts. We do not need a theory to tell us that iron exists or how it may be distinguished. But electrons are what quantum theory says they are, and our only warrant for knowing that they exist is the success of that theory. So there is a special class of theoretical entities whose *entire* warrant lies in the theory built around them. They correspond more or less to the unobservables of van Fraassen.

What makes them vulnerable is that the theory postulating them may itself change or even be dropped. This is where the problems of meaning change and of theory replacement so much discussed in recent philosophy of science become relevant. The antirealist might object to a reality-claim for electrons or genes not so much because they are unobservable but because the reference of the term 'electron' may shift as theory changes. To counter this objection, it sounds as though the realist will have to provide a theory of reference that is able to secure a constancy of reference in regard to such theoretical terms. R. Rorty puts it this way:

> The need to pick out objects without the help of definitions, essences, and meanings of terms, produced (philosophers thought) a need for a "theory of reference" which would not employ the Fregean machinery which Quine had rendered dubious. This call for a theory of reference became assimilated to the demand for a "realistic" philosophy of science which would reinstate the pre-Kuhnian and pre-Feyerabendian notion that scientific inquiry made progress by finding out more and more about the same objects.[25]

Rorty is, of course, skeptical of theories of reference generally, and derides the idea that the problems of realism could be handled by such a theory. He chides Putnam, in particular, for leading philosophers to believe that they could be. Recall the celebrated realist's nightmare conjured up by Putnam:

> What if all the theoretical entities postulated by one generation (molecules, genes, etc. as well as electrons) invariably "don't exist" from the standpoint of later science? . . . One reason this is a serious worry is that eventually the following meta-induction becomes compelling: just as no term used in the science of more than 50 (or whatever) years ago referred, so it will turn out that no term used now (except maybe observation-terms if there are such) refers.[26]

This is the "disastrous meta-induction" which at that time Putnam felt had to be blocked at all costs. But, of course, if the theoretical entities of one generation really did *not* have any existential claim on the next, realism simply would be false. It is, in part at least, because the history of science testifies to a substantial continuity in theoretical structures that we are led to the doctrine of scientific realism at all. Were the history of science *not* to do so, then we would have no logical or metaphysical grounds for believing in scientific realism in the first place. But this is to get ahead of the story. I introduced the issue of reference here not to argue its relevance one way or the other, but to note that one form of antirealism can be directed against the subset of theoretical entities which derive their definition entirely from a particular theory.

One way for a realist to evade objections of this kind is to focus on the manner in which theoretical entities can be causally connected with our measurement apparatus. An electron may be defined as the entity that is causally responsible for, among other things, certain kinds of cloud tracks. A small number of parameters, such as mass and charge, can be

associated with it. Such an entity will be said to exist, that is, not to be an artifact of the apparatus, if a number of convergent sorts of causal lines lead to it. There would still have to be a theory of some sort to enable the causal tracking to be carried out. But the reason to affirm the entity's existence lies not in the success of the theory in which it plays an explanatory role, but in the operation of traceable causal lines. Ian Hacking urges that this defense of realism, which relies on experiential interactions, avoids the problems of meaning-change that beset arguments based on inference to the best explanation.[27]

TRUTH AS CORRESPONDENCE

The most energetic criticisms of realism, of late, have been coming from those who see it as the embodiment of an old-fashioned, and now (in their view) thoroughly discredited, attachment to the notion of truth as some sort of "correspondence" with an "external world." These criticisms take quite different forms, and it is impossible to do them justice in a short space. The rejected doctrine is one that would hold that even in the ideal limit, the best scientific theory, one that has all the proper methodological virtues, could be false. This embodies what the critics have come to call the "God's eye view," the view that there may be more to the world than our language and our sciences can, even in principle, express. They concede that the doctrine has been a persuasive one ("it is impossible to find a philosopher before Kant who was *not* a metaphysical realist");[28] its denial seems, indeed, shockingly anthropomorphic. But they are in agreement that no philosophic sense can be made of the central metaphor of correspondence: "To single out a correspondence between two domains, one needs some independent access to both domains."[29] And, of course, an independent "access to the noumenal objects" is impossible.

The two main protagonists of this view are, perhaps, Rorty and Putnam. Rorty is the more emphatic of the two. He defends a form of pragmatism that discounts the traditional preoccupations of the philosopher with such Platonic notions as truth and goodness. He sees the Greek attempt to separate *doxa* and *epistēmē* as misguided; he equally refuses the modern trap of trying to analyze the meaning of 'true', because it would involve an "impossible attempt to step outside our skins."[30] The pragmatist

> drops the notion of truth as correspondence with reality altogether, and says that modern science does not enable us to cope because it corresponds, it just plain enables us to cope. His argument for the view is that several hundred years of effort have failed to make interesting sense of the notion of "correspondence," either of thoughts to things or of words to things.[31]

Does Rorty deny scientific realism, that is, the view that the long-term success of a scientific theory gives us a warrant to believe that the entities it postulates do exist? It is not clear. What is clear, first, is that he rejects any kind of argument for scientific realism that would explain the success of a theory in terms of a correspondence with the real. And second, he denies that scientific claims have a privileged status, that the scientists' table (in Eddington's famous story) is the only real table. Science, he retorts, is just "one genre of literature," a way "to cope with various bits of the universe," just as ethics helps us cope with other bits.[32]

Putnam, in contrast, is willing to ask the traditional philosophic questions. His patron is Kant rather than James.[33] 'Truth' he defines as "an idealization of rational acceptability."[34] He has more specific objections to urge against the offending metaphysical version of realism than does Rorty, whose argument amounts to claiming that it has failed to make "interesting sense."[35] Does he link this rejection with a rejection of scientific realism? Certainly not in his *Meaning and the Moral Sciences* (1978), where he defends scientific realism by urging that it permits the best explanation of the success of science. It is somewhat more difficult to be sure where his allegiances lie in his more recent pieces; his earlier enthusiasm for scientific realism seems, at the least, to be waning.[36] He attacks materialism with its assumption of mind-independent things,[37] as well as reductionism.

> We are too realistic about physics . . . [because] we see physics (or some hypothetical future physics) as the One True Theory, and not simply as a rationally acceptable description suited for certain problems and purposes.[38]

This does not sound like scientific realism. Be this as it may, however, it seems clear that scientific realism is not the main target in this debate. The target is a set of metaphysical views, views (it is true) that scientific realists have in the past usually taken for granted. I suspect that Rorty would allow that genes exist and that dinosaurs once roamed the earth, as long as these claims are not given a status that is denied to more mundane statements about chairs and goldfish. But can we allow him this position so easily?

Recall that the original motivation for the doctrine of scientific realism was not a perverse philosopher's desire to inquire into the unknowable or to show that only the scientist's entities are "really real." It was a response to the challenges of fictionalism and instrumentalism, which over and over again in the history of science asserted that the entities of the scientist are fictional, that they do not exist in the everyday sense in which chairs and goldfish do. Now, how does Rorty respond to this? Has he an argument to offer? If he has, it would be an argument for scientific realism. It would also (as far as I can see) be a return to philosophy in the "old style" that he thinks we ought to have outgrown.

My own inclinations are to defend a form of metaphysical realism, though not necessarily under all the diverse specifications Putnam offers of it.[39] But that is not to the point here. What is to the point is that scientific realism is not immediately undermined by the rejection of metaphysical realism, though the character of the claim scientific realism makes obviously depends on whether or not it is joined to a concept of truth in which the embattled notion of "correspondence" plays a part. Further, the type of argument most often alleged in its support *does* use the language of correspondence: it is the approximate correspondence between the physical structure of the world and postulated theoretical entities that is held to explain why a theory succeeds as well as it does.[40] Readers will have to decide for themselves whether my argument below does "make interesting sense" or not.

VARIETIES OF ANTIREALISM

It may be worthwhile at this point, looking back at the territory we have traversed, to draw two rough distinctions between types of antirealism. *General antirealism* denies ontological status to theoretical entities of science generally, while *limited antirealism* denies it only to certain classes of theoretical entities, such as those that are said to be unobservable in principle. Thus, the arguments of Laudan, based as they are on a supposedly general review of the history of scientific theories, would lead him to a *general* form of antirealism, one that would exclude existence status to *any* theoretical entity whose existence is warranted only by the success of the theory in which it occurs. In contrast, van Fraassen is claiming, as I have shown, only a *limited* form of antirealism.

Second, we might distinguish between *strong antirealism*, which denies any kind of ontological status to all (or part) of the theoretical entities of science, and *weak antirealism* which allows theoretical entities existence of an everyday "chairs and goldfish" kind,[41] but insists that there is some further sense of "really really there," which realists purportedly have in mind, that is to be rejected. Classical instrumentalism would be of the former kind (strong antirealists), whereas many of the more recent critics of scientific realism appear to fall in the latter category (weak antirealists). These (weak antirealist) critics are often, as I have shown, hard to place. They reject any attempt to justify scientific realism as involving dubious metaphysics, but appear to accept a weak (realist) claim of the "everyday" kind without any form of supporting argument.[42] Their rhetoric is antirealist in tone, but their position often seems compatible with the most basic claim of scientific realism, namely that there is reason to believe that the theoretical terms of successful theories refer. This gives the weak antirealists' position a puzzling sort of undeclared status where they appear to have the best of both worlds. I am inclined to think that their effort to have it both ways must in the end fail.

THE CONVERGENCES OF STRUCTURAL EXPLANATION

The basic claim made by scientific realism, once again, is that the long-term success of a scientific theory gives reason to believe that something like the entities and structure postulated by the theory actually exists. There are four important qualifications built into this: (1) the theory must be successful over a significant period of time; (2) the explanatory success of the theory gives some reason, though not a conclusive warrant, to believe it; (3) what is believed is that the theoretical structures are *something like* the structure of the real world; (4) no claim is made for a special, more basic, privileged, form of existence for the postulated entities.[43] These qualifications: "significant period," "some reason," "something like," sound very vague, of course, and vagueness is a challenge to the philosopher. Can they not be made more precise? I am not sure that they can; efforts to strengthen the thesis of scientific realism have, as I have shown, left it open to easy refutation.

The case for scientific realism can be made in a variety of ways. Maxwell, Salmon, Newton-Smith, Boyd, Putnam, and others have argued it in well-known essays. I am not going to comment on their arguments here since my aim is to outline what I think to be the best case for scientific realism. My argument will, of course, bear many resemblances to theirs. What may be the most distinctive feature of my argument is my stress on structural types of explanation, and on the role played by the criterion of fertility in such explanations.

Stage one of the argument will be directed especially against general antirealism. I want to argue that in many parts of natural science there has been, over the last two centuries, a progressive discovery of *structure*. Scientists construct theories which explain the observed features of the physical world by postulating models of the hidden structure of the entities being studied. This structure is taken to account causally for the observable phenomena, and the theoretical model provides an approximation of the phenomena from which the explanatory power of the model derives. This is the standard account of structural explanation, the type of explanation that first began to show its promise in the eighteenth and early nineteenth centuries in such sciences as geology and chemistry.[44]

I want to consider some of the areas where the growth in our knowledge of structure has been relatively steady. Let me begin with geology, a good place for a realist to begin. The visible strata and their fossil contents came to be interpreted as the evidence for an immense stretch of time past in which various processes such as sedimentation and volcanic activity occurred. There was a lively debate about the mechanisms of mountain building and the like, but gradually a more secure knowledge of the past aeons built up. The Carboniferous period succeeded the Devonian and was, in turn, succeeded by the Permian. The length of the periods, the climatic changes, and the dominant life forms were gradually established with increasing accuracy. It should be stressed that a geological period, such as the Devonian, is a theoretical entity. Further, it is, in principle, inaccessible to our direct observation. Yet our theories have allowed us to set up certain temporal boundaries, in this case (the Devonian period) roughly 400 to 350 million years ago, when the dominant life form on earth was fish and a number of important developments in the vertebrate line occurred.

The long-vanished species of the Devonian are theoretical entities about which we have come to know more and more in a relatively steady way. Of course, there have been controversies, particularly over the sudden extinction of life forms such as occurred at the end of the Cretaceous period and over the precise evolutionary relationships among given species. But the very considerable theory changes that have occurred since Hutton's day do not alter the fact that the growth in our knowledge of the sorts of life forms that inhabited the earth aeons ago has been pretty cumulative. The realist would say that the success of this synthesis of geological, physical, and biological theories gives us good reason to believe that species of these kinds did exist at the times and in the conditions proposed. Most antirealists (I suspect) would agree. But if they do, they must concede that this mode of retroductive argument can warrant, at least in some circumstances, a realist implication.

Geologists have also come to know (in the scientists' sense of the term 'know') a good deal about the interior of the earth. There is a discontinuity between the material of the crust and the much denser mantle, the "Moho" as it is called after its Yugoslavian discoverer, about 5 kilometers under the ocean bed and much deeper, around 30 or 40 kilometers, under the continents. There is a further discontinuity between the solid mantle and the molten core at a depth of 2,900 kilometers. All this is inferred from the characteristics of seismic waves at the surface. Does this structural model of the earth simply serve as a device to enable the scientist to predict the seismic findings more accurately, or does it enable an additional ontological claim to be made about the actual hidden structures of earth? The realist would argue that the explanatory power of the geologist's hypothesis, its steadily improving accuracy, gives good ground to suppose that something can be inferred about real structures that lie far beneath us.

An elegant example of a quite different sort would come from cell biology. Here, the techniques of microscopy have interwoven with the theories of genetics to produce an ever more detailed picture of what goes on inside the cell. The chromosome first appeared under a microscope; only gradually was the gene, the theoretical unit of hereditary transmission, linked to it. Later the gene came to be associated with a particular locus on the chromosome. The unraveling by Crick and Watson of the biochemical structure of the chromosome made it possible to define the structure of the gene in a relatively simple way and has allowed at least the beginnings of an understanding of how the gene operates to direct the growth of the organism. In his book, *The Matter of Life,* Michael Simon has traced this story in some detail, and has argued that its progressive character can best be understood in terms of a realist philosophy of science.[45]

One further example of this sort of progression can be found in chemistry. The complex molecules of both inorganic and organic chemistry have been more accurately charted over the past century. The atomic constituents and the spatial relations among them can be specified on the basis both of measurement, using X-ray diffraction patterns, for example, and on the basis of a theory that specifies where each kind of atom *ought* to fit. Indeed, this knowledge has enabled a computer program to be designed that can "invent" molecules, can suggest that certain configurations would yield a new type of complex molecule and can even predict what some of the molecule's properties are likely to be.

To give a realist construal to the molecular models of the chemist is not to imply that the nature of the constituent atoms and of the bonding between them is exhaustively known. It is only to suppose that the elements and spatial relationships of the model disclose, in a partial and tentative way, real structures within complex molecules. These structures are coming to be more exactly charted, using a variety of techniques both experimental and theoretical. The coherence of the outcome of these widely different techniques, and the reliability of the chemist's intuitions as he decides which atom must fit a particular spot in the lattice, are most easily understood in terms of the realist thesis.

These examples may serve to make two points. The first is that the discontinuous replacement account of the history of theories favored by antirealists is seen to be one-sided. If one focuses on global explanatory theories, particularly in mechanics, it can come to seem that theoretical entities are modified beyond recognition as theories change. Dirac's electron has little in common with the original Thomson electron; Einstein's concept of time is a long way from Newton's, and so on. These conventional examples of conceptual change could themselves be scrutinized to see whether they will bear the weight the antirealist gives them. But it may be more effective to turn from explanatory elements such as electrons to explanatory structures such as those of the organic chemist, and note, as a historical fact, the high degree of continuity in the relevant history.

Second, one could note the sort of confidence that scientists have in structural explanations of this sort. It is not merely a confidence in the empirical adequacy of the predictions these models enable them to make. It is a confidence in the model itself as an analysis of complex real structure. Look at any textbook of polymer chemistry to verify this. Of course, the chemists could be wrong to build this sort of realist expectation into their work, but the arguments of philosophers are not likely to convince them of it.

A third consequence one might draw from the history of the structural sciences is that there is a single form of retroductive inference involved throughout. As C. S. Peirce stressed in his discussion of retroduction, it is the degree of success of the retroductive hypothesis that warrants the degree of its acceptance as truth. The point is a simple one, and indeed is already implicit in Aristotle's *Posterior Analytics.* Aristotle indicates that what certifies as *demonstrative* a piece of reasoning about the relation between the nearness of planets and the fact that they do not twinkle, is the degree to which the reasoning *explains.* This connection between the explanatory and the epistemic character of scientific reasoning is constantly stressed in Renaissance and early modern discussions of hypothetical reasoning.[46]

What the history of recent science has taught us is not that retroductive inference yields a plausible knowledge of causes. We already knew this on *logical* grounds. What we have learned is that retroductive inference *works* in the world we have and with the senses we have for investigating that world. This is a contingent fact, as far as I can see. This is why realism as I have defined it is in part an empirical thesis. There could well be a universe in which observable regularities would *not* be explainable in terms of hidden structures, that is, a world in which retroduction would not work. Indeed, until the eighteenth century, there was no strong empirical case to be made against that being *our* universe. Scientific realism is not a logical doctrine about the implications of successful retroductive inference. Nor is it a metaphysical claim about how any world *must* be. It has both logical and metaphysical components. It is a quite limited claim that purports to explain why certain ways of proceeding in science have worked out as well as they (contingently) have.

That they have worked out well in such structural sciences as geology, astrophysics, and molecular biology, is apparent. And the presumption in these sciences is that the model-structures provide an increasingly accurate insight into the real structures that are causally responsible for the phenomena being explained. This may be thought to give a reliable presumption in favor of the realist implications of retroductive inference in natural science generally. But one has to be wary here. Much depends on the sort of theoretical entity one is dealing with; I have already noted, for instance, some of the perplexities posed by quantum-mechanical entities. Much depends too on how *well* the theoretical entity has served to explain: How important a part of the theory has it been? Has it been a sort of optional extra feature like the solid spheres of Ptolemaic astronomy? Or has it guided research in the way the Bohr model of the hydrogen atom did? What kind of fertility has the theoretical entity shown?

FERTILITY AND METAPHOR

Kuhn lists five values that scientists look for when evaluating a scientific theory: predictive accuracy, consistency, breadth of scope, simplicity, fertility.[47] It is the last of these that bears most directly on the problem of realism. Fertility is usually equated with the ability to make novel predictions. A good theory is expected to predict novel phenomena, that is, phenomena that were not part of the set to be explained. The further in kind these novel phenomena are from the original set, and thus the more unexpected they are, the better the model is said to be. The display of this sort of fertility reduces the likelihood of the theory's being an ad hoc one, one invented just for the original occasion but with no further scope to it.

There has been much debate about the significance of this notion of ad hoc. Clearly, it will appeal to the realist and will seem arbitrary to the antirealist. The realist takes an ad hoc hypothesis not to be a genuine theory, that is, not to give any insight into real structure and therefore to have no ground for further extension. The fact that it accounts for the original data is accidental and testifies to the ingenuity of the inventor rather than to any deeper fit. When the theory is first proposed, it is often difficult to tell whether or not it is ad hoc on the basis of the other criteria

of theory appraisal. This is why fertility is so important a criterion from the realist standpoint.

The antirealist will insist that the novel facts predicted by the theory simply increase its scope and thus make it more acceptable. They will say that there is no significance to the time order in which predictions are made; if they are successful, they count as evidence whether or not they pertain to the data originally to be explained. A straightforward application of Bayes's theorem shows this, assuming of course the antirealist standpoint. Yet scientists seem to set a lot of store in the notion of ad hoc. Are scientific intuitions sufficiently captured by a translation into antirealist language? Is an ad hoc hypothesis one that just happens not to be further generalizable, or is it one that does not give sufficient insight into real structure to permit any further extension?

Rather than debate this already much-debated issue further, let me turn to a second aspect of fertility which is less often noted but which may be more significant for our problem.[48] The first aspect of fertility, novelty, had to do with what could logically be inferred from the theory, its logical resources, one might put it. But a good model has more resources than these. If an anomaly is encountered or if the theory is unable to predict one way or the other in a domain where it seems it *should* be able to do so, the model itself may serve to suggest possible modifications or extensions. These are *suggested,* not implied. Therefore, a creative move on the part of the scientist is required.

In this case, the model functions somewhat as a metaphor does in language. The poet uses a metaphor not just as decoration but as a means of expressing a complex thought. A good metaphor has its own sort of precision, as any poet will tell you. It can lead the mind in ways that literal language cannot. The poet who is developing a metaphor is led by suggestion, not by implication; the reader of the poem queries the metaphor and searches among its many resonances for the ones that seem best to bear insight. The simplistic "man is a wolf" examples of metaphor have misled philosophers into supposing that what is going on in metaphor is a comparison between two already partly understood things. The only challenge then would be to decide in what respects the analogy holds. In the more complex metaphors of modern poetry, something much more interesting is happening. The metaphor is helping to illuminate something that is not well understood in advance, perhaps, some aspect of human life that we find genuinely puzzling or frightening or mysterious. The manner in which such metaphors work is by tentative suggestion. The minds of poet and reader alike are actively engaged in creating. Obviously, much more would need be said about this, but it would lead me too far afield at this point.[49]

The good model has something of this metaphoric power.[50] Let me recall another one here, from geology once again. It had long been known that the west coast of Africa and the east coast of South America show striking similarities in terms of strata and their fossil contents. In 1915, Alfred Wegener put forward a hypothesis to explain these and other similarities, such as those between the major systems of folds in Europe and North America. The continental drift notion that he developed in *The Origins of Continents and Oceans* was not at first accepted, although it admittedly did explain a great deal. There were too many anomalies: How could the continents cut through the ocean floor, for example, since the material of the ocean floor is considerably harder than that of the continents? In the 1960s, new evidence of seafloor spreading led H. Hess and others to a modification of the original model. The moving elements are not the continents but rather vast plates on which the continents as well as the seafloor are carried. And so the continental drift hypothesis developed into the plate tectonic model.

The story has been developed so ably from the methodological standpoint by Rachel Laudan[51] and Henry Frankel[52] that I can be very brief, and simply refer you to their writings. The original theoretical entity, a floating continent, did not logically entail the plates of the new model. But in the context of anomalies and new evidence, it did *suggest* them. And these plates in turn suggested new modifications. What happens when the plates pull apart are seafloor rifts, with quite specific properties. The upwelling lava will have magnetic directional properties that will depend on its orientation relative to the earth's magnetic field at the time. This allows the lava to be dated, and the gradual pulling apart of the plates to be charted. It was the discovery of such dated strips paralleling the midocean rifts that proved decisive in swinging geologists over to the new model in the mid-1960s. What happens when the plates collide? One is carried down under (subduction); the other may be upthrust to form a mountain ridge. One can see here how the original metaphor is gradually extended and made more specific.

In a recent critical discussion of my views on fertility and metaphor,[53] Michael Bradie has urged as a weakness of my argument that one needs to give a sufficiently precise account of metaphor to allow one to understand what would count as a metaphorical extension, so as to know when two theory stages can be identified as different stages of the same theory. My response is simple and, perhaps, simplistic. If the original model (say, continental drift) suggested the later modification as a plausible way of meeting the known anomalies and of incorporating the new evidence, then I would call this a metaphorical extension. Are continental drift and the plate tectonic model two stages of the same theory or

two different theories? It all depends on how 'theory' is defined and how sharply theories are individuated. I do not see that very much hangs on this decision, one way or the other.

The important thing to note is that there *are* structural continuities from one stage to the next, even though there are also important structural modifications. What provides the continuity is the underlying metaphor of moving continents that had been in contact a long time ago and had very gradually developed over the course of time. One feature of the original theory, that the continents are the units, is eventually dropped; other features, such as what happens when the floating plates collide, are thought through and made specific in ways that allow a whole mass of new data to fall into place.

How does all this bear on the argument for realism? The answer should be obvious. This kind of fertility is a persistent feature of structural explanations in the natural sciences over the last three centuries and especially during the last century. How can it best be understood? It appears to be a contingent feature of the history of science. There seems to be no a priori reason why it *had* to work out that way, as I have already shown. What best explains it is the supposition that the model approximates sufficiently well the structures of the world that are causally responsible for the phenomena to be explained to make it profitable for the scientist to take the model's metaphoric extensions seriously. It is because there is something like a floating plate under our feet that it is proper to ask: What happens when plates collide, and what mechanisms would suffice to keep them in motion? These questions do not arise from the original theory if it is taken as no more than a formalism able to give a reasonably accurate predictive account of the data then at hand. If the continental drift hypothesis had no implications for what is really going on beneath us, for the hidden structures responsible for the phenomena of the earth's surface, then the subsequent history of that hypothesis would be unintelligible. The antirealist cannot, it seems to me, make sense of such sequences, which are pretty numerous in the recent history of all the natural sciences, basic mechanics, as always, constituting a special case.

One further point is worth stressing in regard to our geological story. Some theoretical features of the model, such as the midocean rifts, could be checked directly and their existence observationally shown. Here, as so often in science, theoretical entities previously unobserved, or in some cases even thought to be unobservable, are in fact observed and the expectations of theory are borne out, to no one's surprise. The separation between observable and unobservable postulated by many antirealists in regard to ontological status does not seem to stand up. The same mode of argument is used in each case; it is not clear why in one case expectations of real existence are accorded to the theoretical entity whereas in other cases, logically similar in explanatory character, these expectations are denied. The ontological inference, let me insist again, must be far more hesitant in some cases than in others. There is no question of according the same ontological status to *all* theoretical entities by virtue of a similar degree of fertility evinced over a significant period of time. Nonetheless, such fertility finds its best explanation in a broadly realist account of science.

Does this form of argument commit the realist to holding that every regularity in the world must be explained in terms of ontological structure? This turns out to be van Fraassen's main line of attack against realism. He takes it that the realist is committed to finding hidden variables in quantum mechanics. Since the odds against this are now quite high, and since, in any event, this would commit the realist to one possible world where the other looks just as possible, van Fraassen takes this to refute realism. But as I have shown, realism is not a regulative principle, and it does not lay down a strategy for scientists. Realism would not be refuted if the decay of individual radioactive atoms turns out to be genuinely undetermined. It does not look to the future; much more modestly, realism looks to quite specific past historical sequences and asks what best explains them. Realism does not look at *all* science, nor at all future science, just at a good deal of past science which (let me say it again) might not have worked out to support realism the way it did. The realist seeks an explanation for the regularities he finds in science, just as the scientist seeks an explanation for regularities he finds in the world. But if in particular cases he cannot find an explanation or cannot even show that there is no explanation, this in no sense shows that his original aim has somehow been discredited.

Thus, what van Fraassen describes as the "nominalist response" of the antirealist must in the end be rejected. He characterizes it in this way:

> 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.[54]

I hope I have shown that the nominalist resolve to leave such regularities as the extraordinary fertility of our scientific theories at the level of brute fact is unphilosophical. Furthermore, I hope I have shown that it makes a very great deal of difference to the explanatory power or goodness of a theory whether it can call on effective metaphors of hidden structure. And I doubt whether it is really necessary to prove that such

metaphors are important to our understanding of the world and of the role of science in achieving such understanding.

EPILOGUE

Finally, I return to the weighty issues of reference and truth which are so dear to the heart of the philosopher. Clearly, my views on metaphor would lead me to reject the premise on which so much of the recent debate on realism has been based. Van Fraassen puts it thus:

> 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.[55]

I do not think that acceptance of a scientific theory involves the belief that it is true. Science aims at fruitful metaphor and at ever more detailed structure. To suppose that a theory is literally true would imply, among other things, that no further anomaly could, in principle, arise from any quarter in regard to it. At best, it is hard to see this as anything more than an idealized "horizon-claim," which would be quite misleading if applied to the actual work of the scientist. The point is that the resources of metaphor are essential to the work of science and that the construction and retention of metaphor must be seen as part of the aim of science.

Scientists in general accept the quantum theory of radiation. Do they believe it to be true? Scientists are very uncomfortable at this use of the word 'true', because it suggests that the theory is definitive in its formulation. As has often been pointed out, the notion of *acceptance* is very complex, indeed ambiguous. It is basically a pragmatic notion: one accepts an explanation as the best one available; one accepts a theory as a good basis for further research, and so forth. In no case would it be correct to say that acceptance of a theory entails belief in its truth.

The realist would not use the term 'true' to describe a good theory. He would suppose that the structures of the theory give some insight into the structures of the world. But he could not, in general, say how good the insight is. He has no independent access to the world, as the antirealist constantly reminds him. His assurance that there is a fit, however rough, between the structures of the theory and the structures of the world comes not from a comparison between them but from the sort of argument I sketched above, which concludes that only this sort of reasoning would explain certain contingent features of the history of recent science. The term 'approximate truth', which has sometimes been used in this debate, is risky because it immediately invites questions such as: *how* approximate?, and how is the degree of approximation to be measured? If I am right in my presentation of realism, these questions are unanswerable because they are inappropriate.

The language of theoretical explanation is of a quite special sort. It is open-ended and ever capable of further development. It is metaphoric in the sense in which the poetry of the symbolists is metaphoric, not because it uses explicit analogy or because it is imprecise, but because it has resources of suggestion that are the most immediate testimony of its ontological worth. Thus, the M. Dummett-Putnam claim that a realist is committed to holding with respect to any given theory, that the sentences of the theory are either true or false,[56] quite misses the mark where scientific realism is concerned. Indeed, I am tempted to say (though this would be a bit too strong) that if they are literally true or false, they are not of much use as the basis for a research program.

Ought the realist be apologetic, as his pragmatist critic thinks he should be, about such vague-sounding formulations as these: that a good model gives an insight into real structure and that the long-term success of a theory, in most cases, gives reason to believe that something like the theoretical entities of that theory actually exist? I do not think so. The temptation to try for a sharper formulation must be resisted by the realist, since it would almost certainly compromise the sources from which his case derives its basic strength. And the antirealist must beware of the opposite temptation to suppose that whatever cannot be said in a semantically definitive way is not worth saying.

NOTES

The first version of this essay was delivered as an invited paper at the Western Division meeting of the American Philosophical Association in April 1981. I am indebted to Larry Laudan for his incisive commentary on that occasion, and to the numerous discussions we have had on this topic.

1. It was the confidence that, as a student of physics, I had developed in this belief that led me, in my first published paper in philosophy, to formulate a defense of scientific realism against the instrumentalism prevalent at the time among philosophers of science. (See "Realism in Modern Cosmology," *Proceedings American Catholic Philosophical Association* 29 [1955]: 137-150.) Much has changed in philosophy of science since that time; a different sort of defense is (as we shall see) now called for.

2. This is the theme of C. G. Hempel's classic essay, "The Theoretician's Dilemma," *Minnesota Studies in the Philosophy of Science* 3 (1958): 37-98.

3. For the details of this story, see E. McMullin, *Newton on Matter and Activity* (Notre Dame: University of Notre Dame Press, 1978), especially chap. 4: "How is Matter Moved?"

4. In a recent critique of "metaphysical realism," Hilary Putnam has Newton defending the view that particles act at a distance across empty space. *Reason, Truth and History* (Cambridge: Cambridge University Press, 1981), 73. Though the *Principia* has often been made to yield that claim, this view is, in fact, the one alternative that Newton at all times steadfastly rejected.

5. Newton's other suggestion, briefly explored in the 1690s, that forces might be nothing other than the manifestations of God's direct involvement in the governance of the universe, *could*, however, be properly described as 'metaphysical'; this is not, of course, to say that it was illegitimate.

6. H. Putnam, "Why There Isn't a Ready-Made World," *Synthese* 51 (1982): 141-168; see 163. Also available in volume 3 of Putnam's Philosophical Papers Series, *Realism and Reason* (Cambridge: Cambridge University Press, 1983).

7. According to Putnam, Newton, though no positivist, "strongly rejected the idea that his theory of universal gravitation could or should be read as a description of metaphysically ultimate fact. *'Hypotheses non fingo'* was a rejection of metaphysical hypotheses, not of scientific ones" (*Reason, Truth and History*, 163). This supposed rejection of metaphysics would, however, place Newton much closer to positivism than he really was. In the *Principia*, Newton shows himself well aware that different interpretations (he calls them "physical," not "metaphysical") can be given of attraction, and he tries to deflect anticipated criticism of this ambiguity by intimating that one can prescind such interpretation by remaining at the "mathematical" level. But he knew perfectly well that he could not *remain* at this level and still claim to have "explained" the planetary motions. In his own later writing, much of it unpublished in his lifetime, he constantly tried out different hypotheses, as I have already noted. He knew, of course, that these were speculative, that none of them was "metaphysically ultimate fact." But I can find nothing in his writing to suggest that he believed that in principle a decision between these alternatives could not be reached. The task of the natural philosopher (he would have said) was to try to adjudicate between them.

8. As Fine argues in "The Natural Ontological Attitude," this volume.

9. Richard Healey calls it "naive realism"; "naive" not in a deprecatory sense, but as connoting the "natural attitude." See "Quantum Realism: Naiveté Is No Excuse," *Synthese* 42 (1979): 121-144.

10. Especially owing to the developments in recent years of the original quantum formalism, associated not only with physicists (Bell, Kochen, Specker, Wigner) but also with philosophers of science (Cartwright, Fine, Gibbins, Glymour, Putnam, Redhead, Shimony, van Fraassen, and others).

11. This argument may be found, for example, in Fine, "Natural Ontological Attitude," sec. II.

12. G. Chew, "Impasse for the Elementary-Particle Concept," *Great Ideas Today* (Chicago: Encyclopedia Britannica, 1973), 367-389; see 387-389. In his more recent, and very speculative combinatorial topology, Chew has managed to construct a formalism in which the various elementary "particles" are replaced

by combinations of triangles (shades of the *Timaeus!*). Though quarks do not appear in his formalism, Chew has hopes of obtaining all the results that quantum field theory does and perhaps even more.

13. See, for example, W. Heisenberg, "Tradition in Science," in *The Nature of Scientific Discovery*, ed. O. Gingerich (Washington: Smithsonian, 1975), 219-236.

14. In the last few years, this claim has come to seem a lot less plausible, in the short run at least, since quantum field theory has been scoring notable successes, while work on the S-matrix formalism has been all but abandoned.

15. T. Kuhn, *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), 206.

16. See, in particular, L. Laudan, "A Confutation of Convergent Realism," this volume. The quotation is from p. 232.

17. E. Kant, *Critique of Pure Reason*, A226/B273.

18. See G. G. Brittan, *Kant's Theory of Science* (Princeton: Princeton University Press, 1978), chap. 5.

19. B. C. van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980), 73.

20. G. Maxwell, "The Ontological Status of Theoretical Entities," *Minnesota Studies in Philosophy of Science* 3 (1962): 3-27.

21. Van Fraassen, *The Scientific Image*, 16.

22. Ibid., 19.

23. Van Fraassen complicates the picture further by also allowing the sense of 'observable' to depend on the theory being tested. "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." Ibid., 57.

24. Ibid., 16.

25. R. Rorty, *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979), 274-275.

26. H. Putnam, "What is Realism?" this volume p. 145.

27. See I. Hacking, "Experimentation and Scientific Realism," this volume. It is not clear to me whether one comes up with the same list of entities using Hacking's way as one does with the more usual form of argument relying on explanatory efficacy.

28. Putnam, *Reason, Truth and History*, 57.

29. Ibid., 74.

30. R. Rorty, *Consequences of Pragmatism* (Minneapolis: University of Minnesota Press, 1982), xix.

31. Ibid., xvii.

32. Ibid., xliii.

33. I must say that I have difficulties in seeing that Kant "all but says that he is giving up the correspondence theory of truth" (Putnam, *Reason, Truth and History*, 63), and that he "is best read as proposing for the first time what I have called the 'internalist' or 'internal realist' view of truth" (ibid., 60).

34. Ibid., 55. This puts him close to Dummett's camp in a different philosophical battle.

35. These are briefly sketched in "Realism and Reason," final chapter of H. Putnam's *Meaning and the Moral Sciences* (London: Routledge, 1978). See also Putnam, "Why There Isn't a Ready-Made World." His main argument is that even if the world did have a "built-in structure" (which he denies), this could not single out *one* correspondence between signs and objects.

36. 'Scientific realism' does not occur in the topic index of Putnam's, *Reason, Truth and History*, even though other 'realisms' are discussed extensively.

37. See Putnam, "Why There Isn't a Ready-Made World."

38. Putnam, *Reason, Truth and History*, 143. It is curious that both he and Rorty (*Consequences of Pragmatism*, xxvi) criticize the realistic tendency to suppose that physics can reach the "one true theory." But they both define the offending sort of realism precisely as the view that supposes that even in the ideal limit such a theory may not be reached. In fact, according to Putnam's own definition, the "one true theory" is, by definition, what physics *does* reach!

39. These become less and less sympathetic as times goes on. I do not see, for example, why a metaphysical realist should defend the claim that "the world consists of some fixed totality of mind-independent objects," or that "there is exactly one true and complete description of the way 'the world is'" (Putnam, *Reason, Truth and History*, 49). Paul Horwich, in an attempt to pin down Putnam's notion, makes it follow from "a more general and fundamental aspect of metaphysical realism," namely, "the view according to which truth is so inexorably separated from our practice of confirmation that we can have no reasonable expectation that our methods of justification are even remotely correct." Horwich claims that Putnam's notion is "committed to an uncomfortable extent to the possibility of unverifiable truth: no truths are verifiable or even inconclusively confirmable" (P. Horwich, "Three Forms of Realism," *Synthese* 51 [1982]: 181-201; see 188, 189). Not only does this go a long way, in my opinion, beyond what Putnam believes metaphysical realism amounts to, but it also makes a straw man of the position. In fact, I know of no philosopher who would defend it in the form in which Horwich states it.

40. Since this was the type of argument that Putnam endorsed in his earlier work, citing Boyd, one can see why he might now have backed away not only from the supporting argument but also from the thesis itself.

41. This is what Horwich calls "epistemological realism." P. Horwich, "Three Forms of Realism," 181. I am not as convinced as he is that this position is "opposed only by the rare skeptic."

42. Fine's essay in this volume appears to fall into this category. The first section of it is devoted to a critique of all the arguments normally brought in support of scientific realism; the second section argues that instrumentalism had a much more salutary influence than realism did on the growth of modern science. But the final section proposes, as the consequence of a "natural ontological attitude," that "there really are molecules and atoms" and rejects the instrumentalist assertion that they are just fictions. But some argument is needed for this, beyond calling this attitude "natural." And to say that the realist adds to this acceptable "core position" an unacceptable "foot-stamping shout of 'Really,'" an "emphasis that all this is really so," leaves me puzzled as to what this difference is supposed to amount to.

43. The issues as to whether these entities *ought* to be attributed privileged

status (as materialism and various forms of reductionism maintain) will not be discussed here.

44. I traced the history and main features of this form of explanation in "Structural Explanation," *American Philosophical Quarterly* 15 (1978): 139-147.

45. M. Simon, *The Matter of Life* (New Haven: Yale University Press, 1971).

46. See the discussion of this in E. McMullin, "The Conception of Science in Galileo's Work," *New Perspectives on Galileo*, ed. R. Butts and J. Pitt (Dordrecht: Reidel, 1978), 209-257.

47. T. Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1977), 321-322. See also E. McMullin, "Values in Science," PSA Presidential Address 1982, in *PSA* 1982, vol. 2.

48. For a fuller discussion of the criterion of fertility, see E. McMullin, "The Fertility of Theory and the Unit for Appraisal in Science," *Boston Studies in the Philosophy of Science*, ed. R. S. Cohen et al., 39 (1976): 395-432.

49. See, for instance, P. Wheelwright, *Metaphor and Reality* (Bloomington: Indiana University Press, 1962), esp. chap. 4, "Two Ways of Metaphor"; and E. McMullin, "The Motive for Metaphor," *Proceedings American Catholic Philosophical Association* 55 (1982): 27-39.

50. I have elsewhere developed one instance of this in some detail, the Bohr model of the H-atom as it guided research from 1911 to 1926. See E. McMullin, "What Do Physical Models Tell Us?" in *Logic, Methodology and Philosophy of Science*, Proceedings Third International Congress, ed. B. van Rootselaar (Amsterdam, 1968), 3: 389-396.

51. See, for example, R. Laudan, "The Recent Revolution in Geology and Kuhn's Theory of Scientific Change," in *Paradigms and Revolutions*, ed. G. Gutting (Notre Dame: University of Notre Dame Press, 1980), 284-296; R. Laudan, "The Method of Multiple Working Hypotheses and the Development of Plate-Tectonic Theory," in press.

52. H. Frankel, "The Reception and Acceptance of Continental Drift Theory as a Rational Episode in the History of Science," in *The Reception of Unconventional Science*, ed. S. Mauskopf (Boulder: Westview Press, 1978), 51-89; H. Frankel, "The Career of Continental Drift Theory," *Studies in the History and Philosophy of Science* 10 (1979): 21-66.

53. M. Bradie, "Models, Metaphors and Scientific Realism," *Nature and System* 2 (1980): 3-20.

54. Van Fraassen, *The Scientific Image*, 24.

55. Ibid., 8.

56. H. Putnam, "What is Mathematic Truth?", *Mathematics, Matter and Method* (Cambridge: Cambridge University Press), 69-70.

3

The Current Status of Scientific Realism

Richard N. Boyd

INTRODUCTION

The aim of this essay is to assess the strengths and weaknesses of the various traditional arguments for and against scientific realism. I conclude that the typical realist rebuttals to empiricist or constructivist arguments against realism are, in important ways, inadequate. I diagnose the source of the inadequacies in these arguments as a failure to appreciate the extent to which scientific realism requires the abandonment of central tenets of modern epistemology, and I offer an outline of a defense of scientific realism that avoids the inadequacies in question.

SCIENTIFIC REALISM DEFINED

By 'scientific realism' philosophers typically understand a doctrine which we may think of as embodying four central theses:

1. Theoretical terms in scientific theories (i.e., nonobservational terms) should be thought of as putatively referring expressions; that is, scientific theories should be interpreted "realistically."

2. Scientific theories, interpreted realistically, are confirmable and in fact are often confirmed as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.

3. The historical progress of mature sciences is largely a matter of suc-