

tionism: What Physicalism Does Not Entail"; "Scientific Realism and Naturalistic Epistemology"; "Materialism without Reductionism: Non-Humean Causation"; and *Realism and Scientific Epistemology*, for discussion of these points.

25. Boyd, "Scientific Realism and Naturalistic Epistemology"; see also Quine, "Natural Kinds."

26. H. Feigl, "Some Major Issues and Developments in the Philosophy of Science of Logical Empiricism," in *Minnesota Studies in the Philosophy of Science*, vol. 1, ed. H. Feigl and M. Scriven (Minneapolis: University of Minnesota Press, 1956); S. Kripke, "Naming the Necessity," in *The Semantics of Natural Language*, eds. G. Harman and D. Davidson (Dordrecht: D. Reidel, 1972); H. Putnam, "The Meaning of 'Meaning,'" in *Mind, Language and Reality*, ed. H. Putnam, Philosophical Papers, vol. 2 (Cambridge: Cambridge University Press, 1975).

27. Boyd, "Metaphor and Theory Change"; "Scientific Realism and Naturalistic Epistemology"; *Realism and Scientific Epistemology*.

28. Boyd, "Metaphor and Theory Change"; "Scientific Realism and Naturalistic Epistemology."

29. Boyd, "Metaphor and Theory Change."

30. H. Field, "Theory Change and the Indeterminacy of Reference," *Journal of Philosophy* 70 (1973).

31. E. Nagel, *The Structure of Science* (New York: Harcourt Brace, 1961), 151-152.

32. For a discussion of limitations of this process of successive approximation see Boyd, "Scientific Realism and Naturalistic Epistemology."

33. See *ibid.*, for a discussion of the relation between scientific realism and other recent naturalistic trends in epistemology.

34. See "The Natural Ontological Attitude" in this volume. I am extremely grateful to Professor Fine for the opportunity to read a prepublication copy of this paper.

35. For a more precise discussion, see Boyd, "Scientific Realism and Naturalistic Epistemology."

36. *Ibid.*

37. See Boyd, "Metaphor and Theory Change," for a more carefully qualified formulation of this claim.

4

The Natural Ontological Attitude

Arthur Fine

Let us fix our attention out of ourselves as much as possible; let us chace our imagination to the heavens, or to the utmost limits of the universe; we never really advance a step beyond ourselves, nor can conceive any kind of existence, but those perceptions, which have appear'd in that narrow compass. This is the universe of the imagination, nor have we any idea but what is there produced.

—Hume, *Treatise*, Book 1, Part II, Section VI

Realism is dead. Its death was announced by the neopositivists who realized that they could accept all the results of science, including all the members of the scientific zoo, and still declare that the questions raised by the existence claims of realism were mere pseudoquestions. Its death was hastened by the debates over the interpretation of quantum theory, where Bohr's nonrealist philosophy was seen to win out over Einstein's passionate realism. Its death was certified, finally, as the last two generations of physical scientists turned their backs on realism and have managed, nevertheless, to do science successfully without it. To be sure, some recent philosophical literature, and some of the best of it represented by contributors to this book, has appeared to pump up the ghostly shell and to give it new life. But I think these efforts will eventually be seen and understood as the first stage in the process of mourning, the stage of denial. This volume contains some further expressions of this

denial. But I think we shall pass through this first stage and into that of acceptance, for realism is well and truly dead, and we have work to get on with, in identifying a suitable successor. To aid that work I want to do three things in this essay. First, I want to show that the arguments in favor of realism are not sound, and that they provide no rational support for belief in realism. Then, I want to recount the essential role of nonrealist attitudes for the development of science in this century, and thereby (I hope) to loosen the grip of the idea that only realism provides a progressive philosophy of science. Finally, I want to sketch out what seems to me a viable nonrealist position, one that is slowly gathering support and that seems a decent philosophy for postrealist times.¹

ARGUMENTS FOR REALISM

Recent philosophical argument in support of realism tries to move from the success of the scientific enterprise to the necessity for a realist account of its practice. As I see it, the arguments here fall on two distinct levels. On the ground level, as it were, one attends to particular successes; such as novel, confirmed predictions, striking unifications of disparate-seeming phenomena (or fields), successful piggybacking from one theoretical model to another, and the like. Then, we are challenged to account for such success, and told that the best and, it is slyly suggested, perhaps, the *only* way of doing so is on a realist basis. I do not find the details of these ground-level arguments at all convincing. Larry Laudan has provided a forceful and detailed analysis which shows that not even with a lot of hand waving (to shield the gaps in the argument) and charity (to excuse them) can realism itself be used to explain the very successes to which it invites our attention.² But there is a second level of realist argument, the methodological level, that derives from Popper's attack on instrumentalism as inadequate to account for the details of his own, falsificationist methodology. Arguments on this methodological level have been skillfully developed by Richard Boyd,³ and by one of the earlier Hilary Putnams.⁴ These arguments focus on the methods embedded in scientific practice, methods teased out in ways that seem to me accurate and perceptive about ongoing science. We are then challenged to account for why these methods lead to scientific success and told that the best, and (again) perhaps, the only truly adequate way of explaining the matter is on the basis of realism.

I want to examine some of these methodological arguments in detail to display the flaws that seem to be inherent in them. But first I want to point out a deep and, I think, insurmountable problem with this entire

strategy of defending realism, as I have laid it out above. To set up the problem, let me review the debates in the early part of this century over the foundations of mathematics, the debates that followed G. Cantor's introduction of set theory. There were two central worries here, one over the meaningfulness of Cantor's hierarchy of sets insofar as it outstripped the number-theoretic content required by L. Kronecker (and others); the second worry, certainly deriving in good part from the first, was for the consistency (or not) of the whole business. In this context, D. Hilbert devised a quite brilliant program to try to show the consistency of a mathematical theory by using only the most stringent and secure means. In particular, if one were concerned over the consistency of set theory, then clearly a set-theoretic proof of consistency would be of no avail. For if set theory were inconsistent, then such a consistency proof would be both possible and of no significance. Thus, Hilbert suggested that finite constructivist means, satisfactory even to Kronecker (or L. Brouwer) ought to be employed in metamathematics. Of course, Hilbert's program was brought to an end in 1931, when K. Gödel showed the impossibility of such a stringent consistency proof. But Hilbert's idea was, I think, correct even though it proved to be unworkable. Metatheoretic arguments must satisfy more stringent requirements than those placed on the arguments used by the theory in question, for otherwise the significance of reasoning about the theory is simply moot. I think this maxim applies with particular force to the discussion of realism.

Those suspicious of realism, from A. Oslander to H. Poincaré and P. Duhem to the 'constructive empiricism' of van Fraassen,⁵ have been worried about the significance of the explanatory apparatus in scientific investigations. While they appreciate the systematization and coherence brought about by scientific explanation, they question whether acceptable explanations need to be true and, hence, whether the entities mentioned in explanatory principles need to exist.⁶ Suppose they are right. Suppose, that is, that the usual explanation-inferring devices in scientific practice do not lead to principles that are reliably true (or nearly so), nor to entities whose existence (or near-existence) is reliable. In that case, the usual abductive methods that lead us to good explanations (even to 'the best explanation') cannot be counted on to yield results even approximately true. But the strategy that leads to realism, as I have indicated, is just such an ordinary sort of abductive inference. Hence, if the nonrealists were correct in his doubts, then such an inference to realism as the best explanation (or the like), while possible, would be of no significance—exactly as in the case of a consistency proof using the methods of an inconsistent system. It seems, then, that Hilbert's maxim applies to the debate over realism: to argue for realism one must employ methods more

stringent than those in ordinary scientific practice. In particular, one must not beg the question as to the significance of explanatory hypotheses by assuming that they carry truth as well as explanatory efficacy.

There is a second way of seeing the same result. Notice that the issue over realism is precisely the issue as to whether we should believe in the reality of those individuals, properties, relations, processes, and so forth, used in well-supported explanatory hypotheses. Now what is the hypothesis of realism, as it arises as an explanation of scientific practice? It is just the hypothesis that our accepted scientific theories are approximately true, where "being approximately true" is taken to denote an extratheoretical relation between theories and the world. Thus, to address doubts over the reality of relations posited by explanatory hypotheses, the realist proceeds to introduce a further explanatory hypothesis (realism), itself positing such a relation (approximate truth). Surely anyone serious about the issue of realism, and with an open mind about it, would have to behave inconsistently if he were to accept the realist move as satisfactory.

Thus, both at the ground level and at the level of methodology, no support accrues to realism by showing that realism is a good hypothesis for explaining scientific practice. If we are open-minded about realism to begin with, then such a demonstration (even if successful) merely begs the question that we have left open ("need we take good explanatory hypotheses as true?"). Thus, Hilbert's maxim applies, and we must employ patterns of argument more stringent than the usual abductive ones. What might they be? Well, the obvious candidates are patterns of induction leading to empirical generalizations. But, to frame empirical generalizations, we must first have some observable connections between observables. For realism, this must connect theories with the world by way of approximate truth. But no such connections are observable and, hence, suitable as the basis for an inductive inference. I do not want to labor the points at issue here. They amount to the well-known idea that realism commits one to an unverifiable correspondence with the world. So far as I am aware, no recent defender of realism has tried to make a case based on a Hilbert strategy of using suitably stringent grounds and, given the problems over correspondence, it is probably just as well.

The strategy of arguments to realism as a good explanatory hypothesis, then, *cannot* (logically speaking) be effective for an open-minded nonbeliever. But what of the believer? Might he not, at least, show a kind of internal coherence about realism as an overriding philosophy of science, and should that not be of some solace, at least for the realist? Recall, however, the analogue with consistency proofs for inconsistent systems. That sort of harmony should be of no solace to anyone. But for realism, I fear, the verdict is even harsher. For, so far as I can see, the

arguments in question just do not work, and the reason for that has to do with the same question-begging procedures that I have already identified. Let me look closely at some methodological arguments in order to display the problems.

A typical realist argument on the methodological level deals with what I shall call the problem of the "small handful." It goes like this. At any time, in a given scientific area, only a small handful of alternative theories (or hypotheses) are in the field. Only such a small handful are seriously considered as competitors, or as possible successors to some theory requiring revision. Moreover, in general, this handful displays a sort of family resemblance in that none of these live options will be too far from the previously accepted theories in the field, each preserving the well-confirmed features of the earlier theories and deviating only in those aspects less confirmed. Why? Why does this narrowing down of our choices to such a small handful of cousins of our previously accepted theories work to produce good successor theories?

The realist answers this as follows. Suppose that the already existing theories are themselves approximately true descriptions of the domain under consideration. Then surely it is reasonable to restrict one's search for successor theories to those whose ontologies and laws resemble what we already have, especially where what we already have is well confirmed. And if these earlier theories were approximately true, then so will be such conservative successors. Hence, such successors will be good predictive instruments; that is, they will be successful in their own right.

The small-handful problem raises three distinct questions: (1) why only a small handful out of the (theoretically) infinite number of possibilities? (2) why the conservative family resemblance between members of the handful? and (3) why does the strategy of narrowing the choices in this way work so well? The realist response does not seem to address the first issue at all, for even if we restrict ourselves just to successor theories resembling their progenitors, as suggested, there would still, theoretically, always be more than a small handful of these. To answer the second question, as to why conserve the well-confirmed features of ontology and laws, the realist must suppose that such confirmation is a mark of an approximately correct ontology and approximately true laws. But how could the realist possibly justify such an assumption? Surely, there is no valid inference of the form "*T* is well-confirmed; therefore, there exist objects pretty much of the sort required by *T* and satisfying laws approximating to those of *T*." Any of the dramatic shifts of ontology in science will show the invalidity of this schema. For example, the loss of the ether from the turn-of-the-century electrodynamic theories demonstrates this at the level of ontology, and the dynamics of the Rutherford-Bohr atom

vis-à-vis the classical energy principles for rotating systems demonstrates it at the level of laws. Of course, the realist might respond that there is no question of a strict inference between being well confirmed and being approximately true (in the relevant respects), but there is a probable inference of some sort. But of what sort? Certainly there is no probability relation that rests on inductive evidence here. For there is no independent evidence for the relation of approximate truth itself; at least, the realist has yet to produce any evidence that is independent of the argument under examination. But if the probabilities are not grounded inductively, then how else? Here, I think the realist may well try to fall back on his original strategy, and suggest that being approximately true provides the best explanation for being well confirmed. This move throws us back to the ground-level realist argument, the argument from specific success to an approximately true description of reality, which Lauden has criticized. I should point out, before looking at the third question, that if this last move is the one the realist wants to make, then his success at the methodological level can be no better than his success at the ground level. If he fails there, he fails across the board.

The third question, and the one I think the realist puts most weight on, is why does the small-handful strategy work so well. The instrumentalist, for example, is thought to have no answer here. He must just note that it does work well, and be content with that. The realist, however, can explain why it works by citing the transfer of approximate truth from predecessor theories to the successor theories. But what does this explain? At best, it explains why the successor theories cover the same ground as well as their predecessors, for the conservative strategy under consideration assures that. But note that here the instrumentalist can offer the same account: if we insist on preserving the well-confirmed components of earlier theories in later theories, then, of course the later ones will do well over the well-confirmed ground. The difficulty, however, is not here at all but rather is in how to account for the successes of the later theories in new ground or with respect to novel predictions, or in overcoming the anomalies of the earlier theories. And what can the realist possibly say in this area except that the theorist, in proposing a new theory, has happened to make a good guess? For nothing in the approximate truth of the old theory can guarantee (or even make it likely) that modifying the theory in its less-confirmed parts will produce a progressive shift. The history of science shows well enough how such tinkering succeeds only now and again, and fails for the most part. This history of failures can scarcely be adduced to explain the occasional success. The idea that by extending what is approximately true one is likely to bring new approximate truth is a chimera. It finds support neither in the logic of approxi-

mate truth nor in the history of science. The problem for the realist is how to explain the *occasional success* of a strategy that *usually fails*.⁸ I think he has no special resources with which to do this. In particular, his usual fallback onto approximate truth provides nothing more than a gentle pillow. He may rest on it comfortably, but it does not really help to move his cause forward.

The problem of the small handful raises three challenges: why small, why narrowly related, and why does it work. The realist has no answer for the first of these, begs the question as to the truth of explanatory hypotheses on the second, and has no resources for addressing the third. For comparison, it may be useful to see how well his archenemy, the instrumentalist, fares on the same turf. The instrumentalist, I think, has a substantial basis for addressing the questions of smallness and narrowness, for he can point out that it is extremely difficult to come up with alternative theories that satisfy the many empirical constraints posed by the instrumental success of theories already in the field. Often it is hard enough to come up with even one such alternative. Moreover, the common apprenticeship of scientists working in the same area certainly has the effect of narrowing down the range of options by channeling thought into the commonly accepted categories. If we add to this the instrumentally justified rule, "If it has worked well in the past, try it again," then we get a rather good account, I think, of why there is usually only a small and narrow handful. As to why this strategy works to produce instrumentally successful science, we have already noted that for the most part it does not. Most of what this strategy produces are failures. It is a quirk of scientific memory that this fact gets obscured, much as do the memories of bad times during a holiday vacation when we recount all our "wonderful" vacation adventures to a friend. Those instrumentalists who incline to a general account of knowledge as a social construction can go further at this juncture, and lean on the sociology of science to explain how the scientific community "creates" its knowledge. I am content just to back off here and note that over the problem of the small handful, the instrumentalist scores at least two out of three, whereas the realist, left to his own devices, has struck out.⁹

I think the source of the realist's failure here is endemic to the methodological level, infecting all of his arguments in this domain. It resides, in the first instance, in his repeating the question-begging move from explanatory efficacy to the truth of the explanatory hypothesis. And in the second instance, it resides in his twofold mishandling of the concept of approximate truth: first, in his trying to project from some body of assumed approximate truths to some further and novel such truths, and second, in his needing genuine access to the relation of correspondence.

There are no general connections of this first sort, however, sanctioned by the logic of approximate truth, nor secondly, any such warranted access. However, the realist must pretend that there are, in order to claim explanatory power for his realism. We have seen those two agents infecting the realist way with the problem of the small handful. Let me show them at work in another methodological favorite of the realist, the "problem of conjunctions."

The problem of conjunctions is this. If T and T' are independently well-confirmed, explanatory theories, and if no shared term is ambiguous between the two, then we expect the conjunction of T and T' to be a reliable predictive instrument (provided, of course, that the theories are not mutually inconsistent). Why? challenges the realist, and he answers as follows. If we make the realist assumption that T and T' , being well confirmed, are approximately true of the entities (etc.) to which they refer, and if the unambiguity requirement is taken realistically as requiring a domain of common reference, then the conjunction of the two theories will also be approximately true and, hence, it will produce reliable observational predictions. Q.E.D.

But notice our agents at work. First, the realist makes the question-begging move from explanations to their approximate truth, and then he mistreats approximate truth. For nothing in the logic of approximate truth sanctions the inference from " T is approximately true" and " T' is approximately true" to the conclusion that the conjunction " $T \cdot T'$ " is approximately true. Rather, in general, the tightness of an approximation dissipates as we pile on further approximations. If T is within ϵ , in its estimation of some parameter, and T' is also within ϵ , then the only general thing we can say is that the conjunction will be within 2ϵ of the parameter. Thus, the logic of approximate truth should lead us to the opposite conclusion here; that is, that the conjunction of two theories is, in general, *less* reliable than either (over their common domain). But this is neither what we expect nor what we find. Thus, it seems quite implausible that our actual expectations about the reliability of conjunctions rest on the realist's stock of approximate truths.

Of course, the realist could try to retrench here and pose an additional requirement of some sort of uniformity on the character of the approximations, as between T and T' .¹⁰ It is difficult to see how the realist could do this successfully without making reference to the distance between the approximations and "the truth." For what kind of internalist requirement could possibly insure the narrowing of this distance? But the realist is in no position to impose such requirements, since neither he nor anyone else has the requisite access to "the truth." Thus, whatever uniformity-of-approximation condition the realist might impose, we could still demand

to be shown that this leads closer to the truth, not farther away. The realist will have no demonstration, except to point out to us that it all works (sometimes!). But that was the original puzzle.¹¹ Actually, I think the puzzle is not very difficult. For surely, if we do not entangle ourselves with issues over approximation, there is no deep mystery as to why two compatible and successful theories lead us to expect their conjunction to be successful. For in forming the conjunction, we just add the reliable predictions of one onto the reliable predictions of the other, having antecedently ruled out the possibility of conflict.

There is more to be said about this topic. In particular, we need to address the question as to why we expect the logical gears of the two theories to mesh. However, I think that a discussion of the realist position here would only bring up the same methodological and logical problems that we have already uncovered at the center of the realist argument.

Indeed, this schema of knots in the realist argument applies across the board and vitiates every single argument at the methodological level. Thus my conclusion here is harsh, indeed. The methodological arguments for realism fail, even though, were they successful, they would still not support the case. For the general strategy they are supposed to implement is just not stringent enough to provide rational support for realism. In the next two sections, I will try to show that this situation is just as well, for realism has not always been a progressive factor in the development of science and, anyway, there is a position other than realism that is more attractive.

REALISM AND PROGRESS

If we examine the two twentieth-century giants among physical theories, relativity and the quantum theory, we find a living refutation of the realist's claim that only his view of science explains its progress, and we find some curious twists and contrasts over realism as well. The theories of relativity are almost singlehandedly the work of Albert Einstein. Einstein's early positivism and his methodological debt to Mach (and Hume) leap right out of the pages of the 1905 paper on special relativity.¹² The same positivist strain is evident in the 1916 general relativity paper as well, where Einstein (in Section 3 of that paper) tries to justify his requirement of general covariance by means of a suspicious-looking verificationist argument which, he says, "takes away from space and time the last remnants of physical objectivity."¹³ A study of his tortured path to general relativity (see here the brilliant work of John Earman, following on earlier hints by Banesh Hoffmann)¹⁴ shows the repeated use of this

Machist line, always used to deny that some concept has a real referent. Whatever other, competing strains there were in Einstein's philosophical orientation (and there certainly were others), it would be hard to deny the importance of this instrumentalist/positivist attitude in liberating Einstein from various realist commitments. Indeed, on another occasion, I would argue in detail that without the "freedom from reality" provided by his early reverence for Mach, a central tumbler necessary to unlock the secret of special relativity would never have fallen into place.¹⁵ A few years after his work on general relativity, however, roughly around 1920, Einstein underwent a philosophical conversion, turning away from his positivist youth (he was forty-one in 1920) and becoming deeply committed to realism.¹⁶ His subsequent battle with the quantum theory, for example, was fought much more over the issue of realism than it was over the issue of causality or determinism (as it is usually portrayed). In particular, following his conversion, Einstein wanted to claim genuine reality for the central theoretical entities of the general theory, the four-dimensional space-time manifold and associated tensor fields. This is a serious business for if we grant his claim, then not only do space and time cease to be real but so do virtually all of the usual dynamical quantities.¹⁷ Thus motion, as we understand it, itself ceases to be real. The current generation of philosophers of space and time (led by Howard Stein and John Earman) have followed Einstein's lead here. But, interestingly, not only do these ideas boggle the mind of the average man in the street (like you and me), they boggle most contemporary scientific minds as well.¹⁸ That is, I believe the majority opinion among working, knowledgeable scientists is that general relativity provides a magnificent organizing tool for treating certain gravitational problems in astrophysics and cosmology. But few, I believe, give credence to the kind of realist existence and nonexistence claims that I have been mentioning. For relativistic physics, then, it appears that a nonrealist attitude was important in its development, that the founder nevertheless espoused a realist attitude to the finished product, but that most who actually use it think of the theory as a powerful instrument, rather than as expressing a "big truth."

With quantum theory, this sequence gets a twist. Heisenberg's seminal paper of 1925 is prefaced by the following abstract, announcing, in effect, his philosophical stance: "In this paper an attempt will be made to obtain bases for a quantum-theoretical mechanics based exclusively on relations between quantities observable in principle."¹⁹ In the body of the paper, Heisenberg not only rejects any reference to unobservables; he also moves away from the very idea that one should try to form any picture of a reality underlying his mechanics. To be sure, E. Schrödinger, the second father of quantum theory, seems originally to have had a vague pic-

ture of an underlying wavelike reality for his own equation. But he was quick to see the difficulties here and, just as quickly, although reluctantly, abandoned the attempt to interpolate any reference to reality.²⁰ These instrumentalist moves, away from a realist construal of the emerging quantum theory, were given particular force by Bohr's so-called "philosophy of complementarity"; and this nonrealist position was consolidated at the time of the famous Solvay conference, in October of 1927, and is firmly in place today. Such quantum nonrealism is part of what every graduate physicist learns and practices. It is the conceptual back-drop to all the brilliant successes in atomic, nuclear, and particle physics over the past fifty years. Physicists have learned to think about their theory in a highly nonrealist way, and doing just that has brought about the most marvelous predictive success in the history of science.

The war between Einstein, the realist, and Bohr, the nonrealist, over the interpretation of quantum theory was not, I believe, just a sideshow in physics, nor an idle intellectual exercise. It was an important endeavor undertaken by Bohr on behalf of the enterprise of physics as a progressive science. For Bohr believed (and this fear was shared by Heisenberg, A. Sommerfeld, W. Pauli, and M. Born—and all the major players) that Einstein's realism, if taken seriously, would block the consolidation and articulation of the new physics and, thereby, stop the progress of science. They were afraid, in particular, that Einstein's realism would lead the next generation of the brightest and best students into scientific dead ends. Alfred Landé, for example, as a graduate student, was interested in spending some time in Berlin to sound out Einstein's ideas. His supervisor was Sommerfeld, and recalling this period, Landé writes

The more pragmatic Sommerfeld . . . warned his students, one of them this writer, not to spend too much time on the hopeless task of "explaining" the quantum but rather to accept it as fundamental and help work out its consequences.²¹

The task of "explaining" the quantum, of course, is the realist program for identifying a reality underlying the formulas of the theory and thereby explaining the predicative success of the formulas as approximately true descriptions of this reality. It is this program that I have criticized in the first part of this paper, and this same program that the builders of quantum theory saw as a scientific dead end. Einstein knew perfectly well that the issue was joined right here. In the summer of 1935, he wrote to Schrödinger,

The real problem is that physics is a kind of metaphysics; physics describes 'reality'. But we do not know what 'reality' is. We know it only through physical description. . . . But the Talmudic philosopher sniffs at 'reality', as at a frightening creature of the naive mind.²²

By avoiding the bogey of an underlying reality, the "Talmudic" originators of quantum theory seem to have set subsequent generations on precisely the right path. Those inspired by realist ambitions have produced no predictively successful physics. Neither Einstein's conception of a unified field nor the ideas of the de Broglie group about pilot waves, nor the Bohm-inspired interest in hidden variables has made for scientific progress. To be sure, several philosophers of physics, including another Hilary Putnam, and myself, have fought a battle over the last decade to show that the quantum theory is at least consistent with some kind of underlying reality. I believe that Hilary has abandoned the cause, perhaps in part on account of the recent Bell-inequality problem over correlation experiments, a problem that van Fraassen calls "the charybdis of realism."²³ My own recent work in the area suggests that we may still be able to keep realism afloat in this whirlpool.²⁴ But the possibility (as I still see it) for a realist account of the quantum domain should not lead us away from appreciating the historical facts of the matter.

One can hardly doubt the importance of a nonrealist attitude for the development and practically infinite success of the quantum theory. Historical counterfactuals are always tricky, but the sterility of actual realist programs in this area at least suggests that Bohr and company were right in believing that the road to scientific progress here would have been blocked by realism. The founders of quantum theory never turned on the nonrealist attitude that served them so well. Perhaps that is because the central underlying theoretical device of quantum theory, the densities of a complex-valued and infinite-dimensional wave function, are even harder to take seriously than is the four-dimensional manifold of relativity. But now, there comes a most curious twist. For just as the practitioners of relativity, I have suggested, ignore the *realist* interpretation in favor of a more pragmatic attitude toward the space-time structure, the quantum physicists would appear to make a similar reversal and to forget their nonrealist history and allegiance when it comes time to talk about new discoveries.

Thus, anyone in the business will tell you about the exciting period, in the fall of 1974, when the particle group at Brookhaven, led by Samuel Ting, discovered the J particle, just as a Stanford team at the Stanford Linear Accelerator Center (SLAC), under Burton Richter, independently found a new particle they called " ψ ". These turned out to be one and the same, the so-called ψ/J particle (Mass 3,098 MeV, Spin 1, Resonance 67 KeV, Strangeness 0). To explain this new entity, the theoreticians were led to introduce a new kind of quark, the so-called charmed quark. The ψ/J particle is then thought to be made up out of a charmed quark and an anticharmed quark, with their respective spins aligned. But if this is

correct, then there ought to be other such pairs anti-aligned, or with variable spin alignments, and these ought to make up quite new observable particles. Such predictions from the charmed-quark model have turned out to be confirmed in various experiments.

In this example, I have been intentionally a bit more descriptive in order to convey the realist feel to the way scientists speak in this area. For I want to ask whether this is a return to realism or whether, instead, it can somehow be reconciled with a fundamentally nonrealist attitude.²⁵ I believe that the nonrealist option is correct, but I will not defend that answer here, however, because its defense involves the articulation of a compelling and viable form of nonrealism; and that is the task of the third (and final) section of this paper.

NONREALISM

Even if the realist happens to be a talented philosopher, I do not believe that, in his heart, he relies for his realism on the rather sophisticated form of abductive argument that I have examined and rejected in the first section of this paper, and which the history of twentieth-century physics shows to be fallacious. Rather, if his heart is like mine (and I *do* believe in a common nature), then I suggest that a more simple and homely sort of argument is what grips him. It is this, and I will put it in the first person. I certainly trust the evidence of my senses, on the whole, with regard to the existence and features of everyday objects. And I have similar confidence in the system of "check, double-check, triple-check" of scientific investigation, as well as the other safeguards built into the institutions of science. So, if the scientists tell me that there really are molecules, and atoms, and ψ/J particles and, who knows, maybe even quarks, then so be it. I trust them and, thus, must accept that there really are such things, with their attendant properties and relations. Moreover, if the instrumentalist (or some other member of the species "non-realistica") comes along to say that these entities, and their attendants, are just fictions (or the like), then I see no more reason to believe him than to believe that *he* is a fiction, made up (somehow) to do a job on me; which I do not believe. It seems, then, that I had better be a realist. One can summarize this homely and compelling line as follows: it is possible to accept the evidence of one's senses and to accept, *in the same way*, the confirmed results of science only for a realist; hence, I should be one (and so should you!).

What is it to accept the evidence of one's senses and, *in the same way*, to accept confirmed scientific theories? It is to take them into one's life as

true, with all that implies concerning adjusting one's behavior, practical and theoretical, to accommodate these truths. Now, of course, there are truths, and truths. Some are more central to us and our lives, some less so. I might be mistaken about anything, but were I mistaken about where I am right now, that might affect me more than would my perhaps mistaken belief in charmed quarks. Thus, it is compatible with the homely line of argument that some of the scientific beliefs that I hold are less central than some, for example, perceptual beliefs. Of course, were I deeply in the charmed-quark business, giving up that belief might be more difficult than giving up some at the perceptual level. (Thus we get the phenomenon of "seeing what you believe," as is well known to all thoughtful people.) When the homely line asks us, then, to accept the scientific results "in the same way" in which we accept the evidence of our senses, I take it that we are to accept them both as true. I take it that we are being asked not to distinguish between kinds of truth or modes of existence or the like, but only among truths themselves, in terms of centrality, degrees of belief, or such.

Let us suppose this understood. Now, do you think that Bohr, the archenemy of realism, could toe the homely line? Could Bohr, fighting for the sake of science (against Einstein's realism) have felt compelled either to give up the results of science, or else to assign to its "truths" some category different from the truths of everyday life? It seems unlikely. And thus, unless we uncharitably think Bohr inconsistent on this basic issue, we might well come to question whether there is any necessary connection moving us from accepting the results of science as true to being a realist.²⁶

Let me use the term 'antirealist' to refer to any of the many different specific enemies of realism: the idealist, the instrumentalist, the phenomenalist, the empiricist (constructive or not), the conventionalist, the constructivist, the pragmatist, and so forth. Then, it seems to me that both the realist and the antirealist must toe what I have been calling "the homely line." That is, they must both accept the certified results of science as on par with more homely and familiarly supported claims. That is not to say that one party (or the other) cannot distinguish more from less well-confirmed claims at home or in science; nor that one cannot single out some particular mode of inference (such as inference to the best explanation) and worry over its reliability, both at home and away. It is just that one must maintain parity. Let us say, then, that both realist and antirealist accept the results of scientific investigations as 'true', on par with more homely truths. (I realize that some antirealists would rather use a different word, but no matter.) And call this acceptance of scientific truths the "core position."²⁷ What distinguishes realists from antirealists, then, is what they add onto this core position.

The antirealist may add onto the core position a particular analysis of the concept of truth, as in the pragmatic and instrumentalist and conventionalist conceptions of truth. Or the antirealist may add on a special analysis of concepts, as in idealism, constructivism, phenomenism, and in some varieties of empiricism. These addenda will then issue in a special meaning, say, for existence statements. Or the antirealist may add on certain methodological strictures, pointing a wary finger at some particular inferential tool, or constructing his own account for some particular aspects of science (e.g., explanations or laws). Typically, the antirealist will make several such additions to the core.

What then of the realist, what does he add to his core acceptance of the results of science as really true? My colleague, Charles Chastain, suggested what I think is the most graphic way of stating the answer—namely, that what the realist adds on is a desk-thumping, foot-stamping shout of "Really!" So, when the realist and antirealist agree, say, that there really are electrons and that they really carry a unit negative charge and really do have a small mass (of about 9.1×10^{-28} grams), what the realist wants to add is the emphasis that all this is really so. "There really are electrons, really!" This typical realist emphasis serves both a negative and a positive function. Negatively, it is meant to deny the additions that the antirealist would make to that core acceptance which both parties share. The realist wants to deny, for example, the phenomenistic reduction of concepts or the pragmatic conception of truth. The realist thinks that these addenda take away from the substantiality of the accepted claims to truth or existence. "No," says he, "they *really* exist, and not in just your diminished antirealist sense." Positively, the realist wants to explain the robust sense in which *he* takes these claims to truth or existence, namely, as claims about reality—what is really, really the case. The full-blown version of this involves the conception of truth as correspondence with the world, and the surrogate use of approximate truth as near-correspondence. We have already seen how these ideas of correspondence and approximate truth are supposed to explain what *makes* the truth *true* whereas, in fact, they function as mere trappings, that is, as superficial decorations that may well attract our attention but do not compel rational belief. Like the extra "really," they are an arresting foot-thump and, logically speaking, of no more force.

It seems to me that when we contrast the realist and the antirealist in terms of what they each want to add to the core position, a third alternative emerges—and an attractive one at that. It is the core position itself, *and all by itself*. If I am correct in thinking that, at heart, the grip of realism only extends to the homely connection of everyday truths with scientific truths, and that good sense dictates our acceptance of the one on the same basis as our acceptance of the other, then the homely line makes the

core position, all by itself, a compelling one, one that we ought to take to heart. Let us try to do so, and to see whether it constitutes a philosophy, and an attitude toward science, that we can live by.

The core position is neither realist nor antirealist; it mediates between the two. It would be nice to have a name for this position, but it would be a shame to appropriate another "ism" on its behalf, for then it would appear to be just one of the many contenders for ontological allegiance. I think it is not just one of that crowd but rather, as the homely line behind it suggests, it is for commonsense epistemology—the natural ontological attitude. Thus, let me introduce the acronym NOA (pronounced as in "Noah"), for *natural ontological attitude*, and, henceforth, refer to the core position under that designation.

To begin showing how NOA makes for an adequate philosophical stance toward science, let us see what it has to say about ontology. When NOA counsels us to accept the results of science as true, I take it that we are to treat truth in the usual referential way, so that a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations. Thus, NOA sanctions ordinary referential semantics and commits us, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true. Our belief in their existence will be just as strong (or weak) as our belief in the truth of the bit of science involved, and degrees of belief here, presumably, will be tutored by ordinary relations of confirmation and evidential support, subject to the usual scientific canons. In taking this referential stance, NOA is not committed to the progressivism that seems inherent in realism. For the realist, as an article of faith, sees scientific success, over the long run, as bringing us closer to the truth. His whole explanatory enterprise, using approximate truth, forces his hand in this way. But, a "noaer" (pronounced as "knower") is not so committed. As a scientist, say, within the context of the tradition in which he works, the noaer, of course, will believe in the existence of those entities to which his theories refer. But should the tradition change, say in the manner of the conceptual revolutions that Kuhn dubs "paradigm shifts," then nothing in NOA dictates that the change be assimilated as being progressive, that is, as a change where we learn more accurately about *the same things*. NOA is perfectly consistent with the Kuhnian alternative, which construes such changes as wholesale changes of reference. Unlike the realist, adherents to NOA are free to examine the facts in cases of paradigm shift, and to see whether or not a convincing case for stability of reference across paradigms can be made without superimposing on these facts a realist-progressivist superstructure. I have argued elsewhere that if one makes oneself free, as NOA enables one to do, then

the facts of the matter will not usually settle the case;²⁸ and that this is a good reason for thinking that cases of so-called "incommensurability" are, in fact, genuine cases where the question of stability of reference is indeterminate. NOA, I think, is the right philosophical position for such conclusions. It sanctions reference and existence claims, but it does not force the history of science into prefit molds.

So far I have managed to avoid what, for the realist, is the essential point, for what of the "external world"? How can I talk of reference and of existence claims unless I am talking about referring to things right out there in the world? And here, of course, the realist, again, wants to stamp his feet.²⁹ I think the problem that makes the realist want to stamp his feet, shouting "Really!" (and invoking the external world) has to do with the stance the realist tries to take vis-à-vis the game of science. The realist, as it were, tries to stand outside the arena watching the ongoing game and then tries to judge (from this external point of view) what the point is. It is, he says, *about* some area external to the game. The realist, I think, is fooling himself. For he cannot (really!) stand outside the arena, nor can he survey some area off the playing field and mark it out as what the game is about.

Let me try to address these two points. How are we to arrive at the judgment that, in addition to, say, having a rather small mass, electrons are objects "out there in the external world"? Certainly, we can stand off from the electron game and survey its claims, methods, predictive success, and so forth. But what stance could we take that would enable us to judge what the theory of electrons is *about*, other than agreeing that it is about electrons? It is not like matching a blueprint to a house being built, or a map route to a country road. For we are *in* the world, both physically and conceptually.³⁰ That is, *we* are among the objects of science, and the concepts and procedures that we use to make judgments of subject matter and correct application are themselves part of that same scientific world. Epistemologically, the situation is very much like the situation with regard to the justification of induction. For the problem of the external world (so-called) is how to satisfy the realist's demand that we justify the existence claims sanctioned by science (and, therefore, by NOA) as claims to the existence of entities "out there." In the case of induction, it is clear that only an inductive justification will do, and it is equally clear that no inductive justification will do at all. So too with the external world, for only ordinary scientific inferences to existence will do, and yet none of them satisfies the demand for showing that the existent is really "out there." I think we ought to follow Hume's prescription on induction, with regard to the external world. There is no possibility for justifying the kind of externality that realism requires, yet it may well

be that, in fact, we cannot help yearning for just such a comforting grip on reality. I shall return to this theme at the close of the paper.

If I am right, then the realist is chasing a phantom, and we cannot actually do more, with regard to existence claims, than follow scientific practice, just as NOA suggests. What then of the other challenges raised by realism? Can we find in NOA the resources for understanding scientific practice? In particular (since it was the topic of the first part of this paper), does NOA help us to understand the scientific method, such as the problems of the small handful or of conjunctions? The sticking point with the small handful was to account for why the few and narrow alternatives that we can come up with, result in successful novel predictions, and the like. The background was to keep in mind that most such narrow alternatives are not successful. I think that NOA has only this to say. If you believe that guessing based on some truths is more likely to succeed than guessing pure and simple, then if our earlier theories were in large part true and if our refinements of them conserve the true parts, then guessing on this basis has some relative likelihood of success. I think this is a weak account, but then I think the phenomenon here does not allow for anything much stronger since, for the most part, such guesswork fails. In the same way, NOA can help with the problem of conjunctions (and, more generally, with problems of logical combinations). For if two consistent theories in fact have overlapping domains (a fact, as I have just suggested, that is not so often decidable), and if the theories also have true things to say about members in the overlap, then conjoining the theories just adds to the truths of each and, thus, *may*, in conjunction, yield new truths. Where one finds other successful methodological rules, I think we will find NOA's grip on the truth sufficient to account for the utility of the rules.

Unlike the realist, however, I would not tout NOA's success at making science fairly intelligible as an argument in its favor, vis-à-vis realism or various antirealisms. For NOA's accounts are available to these fellows, too, provided what they add to NOA does not negate its appeal to the truth, as does a verificationist account of truth or the realists' longing for approximate truth. Moreover, as I made plain enough in the first section of this paper, I am sensitive to the possibility that explanatory efficacy can be achieved without the explanatory hypothesis being true. NOA may well make science seem fairly intelligible and even rational, but NOA could be quite the wrong view of science for all that. If we posit as a constraint on philosophizing about science that the scientific enterprise should come out in our philosophy as not too unintelligible or irrational, then, perhaps, we can say that NOA passes a minimal standard for a philosophy of science.

Indeed, perhaps the greatest virtue of NOA is to call attention to just how minimal an adequate philosophy of science can be. (In this respect, NOA might be compared to the minimalist movement in art.) For example, NOA helps us to see that realism differs from various antirealisms in this way: realism adds an outer direction to NOA, that is, the external world and the correspondence relation of approximate truth; antirealisms (typically) add an inner direction, that is, human-oriented reductions of truth, or concepts, or explanations (as in my opening citation from Hume). NOA suggests that the legitimate features of these additions are already contained in the presumed equal status of everyday truths with scientific ones, and in our accepting them both as *truths*. No other additions are legitimate, and none are required.

It will be apparent by now that a distinctive feature of NOA, one that separates it from similar views currently in the air, is NOA's stubborn refusal to amplify the concept of truth, by providing a theory or analysis (or even a metaphorical picture). Rather, NOA recognizes in "truth" a concept already in use and agrees to abide by the standard rules of usage. These rules involve a Davidsonian-Tarskian, referential semantics, and they support a thoroughly classical logic of inference. Thus NOA respects the customary "grammar" of 'truth' (and its cognates). Likewise, NOA respects the customary epistemology, which grounds judgments of truth in perceptual judgments and various confirmation relations. As with the use of other concepts, disagreements are bound to arise over what is true (for instance, as to whether inference to the best explanation is always truth-conferring). NOA pretends to no resources for settling these disputes, for NOA takes to heart the great lesson of twentieth-century analytic and Continental philosophy, namely, that there *are* no general methodological or philosophical resources for deciding such things. The mistake common to realism and all the antirealisms alike is their commitment to the existence of such nonexistent resources. If pressed to answer the question of what, then, does it *mean* to say that something is true (or to what does the truth of so-and-so commit one), NOA will reply by pointing out the logical relations engendered by the specific claim and by focusing, then, on the concrete historical circumstances that ground that particular judgment of truth. For, after all, there is nothing more to say.³¹

Because of its parsimony, I think the minimalist stance represented by NOA marks a revolutionary approach to understanding science. It is, I would suggest, as profound in its own way as was the revolution in our conception of morality, when we came to see that founding morality on God and His Order was *also* neither legitimate nor necessary. Just as the typical theological moralist of the eighteenth century would feel bereft to

read, say, the pages of *Ethics*, so I think the realist must feel similarly when NOA removes that "correspondence to the external world" for which he so longs. I too have regret for that lost paradise, and too often slip into the realist fantasy. I use my understanding of twentieth-century physics to help me firm up my convictions about NOA, and I recall some words of Mach, which I offer as a comfort and as a closing. With reference to realism, Mach writes

It has arisen in the process of immeasurable time without the intentional assistance of man. It is a product of nature, and preserved by nature. Everything that philosophy has accomplished . . . is, as compared with it, but an insignificant and ephemeral product of art. The fact is, every thinker, every philosopher, the moment he is forced to abandon his one-sided intellectual occupation . . . , immediately returns [to realism].

Nor is it the purpose of these "introductory remarks" to discredit the standpoint [of realism]. The task which we have set ourselves is simply to show why and for what purpose we hold that standpoint during most of our lives, and why and for what purpose we are . . . obliged to abandon it.

These lines are taken from Mach's *The Analysis of Sensations* (Sec. 14). I recommend that book as effective realism-therapy, a therapy that works best (as Mach suggests) when accompanied by historicophysical investigations (real versions of the breakneck history of my second section, "Realism and Progress"). For a better philosophy, however, I recommend NOA.

NOTES

My thanks to Charles Chastain, Gerald Dworkin, and Paul Teller for useful preliminary conversations about realism and its rivals, but especially to Charles—for only he, then, (mostly) agreed with me, and surely that deserves special mention. This paper was written by me, but cothought by Micky Forbes. I don't know any longer whose ideas are whose. That means that the responsibility for errors and confusions is at least half Micky's (and she is two-thirds responsible for "NOA"). Finally, I am grateful to the many people who offered comments and criticisms at the conference, and subsequently. I am also grateful to the National Science Foundation for a grant in support of this research.

1. In the final section, I call this postrealism "NOA." Among recent views that relate to NOA, I would include Hilary Putnam's "internal realism," Richard Rorty's "epistemological behaviorism," the "semantic realism" espoused by Paul Horwich, parts of the "Mother Nature" story told by William Lycan, and the defense of common sense worked out by Joseph Pitt (as a way of reconciling W. Sellars's manifest and scientific images). For references, see Hilary Putnam,

Meaning and the Moral Sciences (London: Routledge and Kegan Paul, 1978); Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton: Princeton University Press, 1979); Paul Horwich, "Three Forms of Realism," *Synthese* 51 (1982): 181-201; William G. Lycan, "Epistemic Value" (preprint, 1982); and Joseph C. Pitt, *Pictures, Images and Conceptual Change* (Dordrecht: D. Reidel, 1981). The reader will note that some of the above consider their views a species of realism, whereas others consider their views antirealist. As explained below, NOA marks the divide; hence its "postrealism."

2. Larry Laudan, "A Confutation of Convergent Realism," this volume.

3. Richard N. Boyd, "Scientific Realism and Naturalistic Epistemology," in *PSA* (1980), vol. 2, ed. P. D. Asquith and R. N. Giere (E. Lansing: Philosophy of Science Association, 1981), 613-662. See also, Boyd's article in this book, and further references there.

4. Hilary Putnam, "The Meaning of 'Meaning,'" in *Language, Mind and Knowledge*, ed. K. Gunderson (Minneapolis: University of Minnesota Press, 1975), 131-193. See also his article in this volume.

5. Bas C. van Fraassen, *The Scientific Image* (Oxford: The Clarendon Press, 1980). See especially pp. 97-101 for a discussion of the truth of explanatory theories. To see that the recent discussion of realism is joined right here, one should contrast van Fraassen with W. H. Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981), esp. chap. 8.

6. Nancy Cartwright's *How The Laws of Physics Lie* (Oxford: Oxford University Press, 1983) includes some marvelous essays on these issues.

7. Some realists may look for genuine support, and not just solace, in such a coherentist line. They may see in their realism a basis for general epistemology, philosophy of language, and so forth (as does Boyd, "Scientific Realism and Naturalistic Epistemology"). If they find in all this a coherent and comprehensive world view, then they might want to argue for their philosophy as Wilhelm Wien argued (in 1909) for special relativity, "What speaks for it most of all is the inner consistency which makes it possible to lay a foundation having no self-contradictions, one that applies to the totality of physical appearances." Quoted by Gerald Holton, "Einstein's Scientific Program: Formative Years" in *Some Strangeness in the Proportion*, ed. H. Woolf (Reading: Addison-Wesley, 1980), 58. Insofar as the realist moves away from the abductive defense of realism to seek support, instead, from the merits of a comprehensive philosophical system with a realist core, he marks as a failure the bulk of recent defenses of realism. Even so, he will not avoid the critique pursued in the text. For although my argument above has been directed, in particular, against the abductive strategy, it is itself based on a more general maxim, namely, that the form of argument used to support realism must be more stringent than the form of argument embedded in the very scientific practice that realism itself is supposed to ground—on pain of begging the question. Just as the abductive strategy fails because it violates this maxim, so too would the coherentist strategy, should the realist turn from one to the other. For, as we see from the words of Wien, the same coherentist line that the realist would appropriate for his own support, is part of ordinary scientific practice in framing judgments about competing theories. It is, therefore, not a line of defense avail-

able to the realist. Moreover, just as the truth-bearing status of abduction is an issue dividing realists from various nonrealists, so too is the status of coherence-based inference. Turning from abduction to coherence, therefore, still leaves the realist begging the question. Thus, when we bring out into the open the character of arguments for realism, we see quite plainly that they do not work.

In support of realism there seem to be only those "reasons of the heart" which, as Pascal says, reason does not know. Indeed, I have long felt that belief in realism involves a profound leap of faith, not at all dissimilar from the faith that animates deep religious convictions. I would welcome engagement with realists on this understanding, just as I enjoy conversation on a similar basis with my religious friends. The dialogue will proceed more fruitfully, I think, when the realists finally stop pretending to a rational support for their faith, which they do not have. Then we can all enjoy their intricate and sometimes beautiful philosophical constructions (of, e.g., knowledge, or reference, etc), even though, as nonbelievers, they may seem to us only wonderful castles in the air.

8. I hope all readers of this essay will take this idea to heart. For in formulating the question as how to explain why the methods of science lead to instrumental success, the realist has seriously misstated the explanandum. Overwhelmingly, the results of the conscientious pursuit of scientific inquiry are failures: failed theories, failed hypotheses, failed conjectures, inaccurate measurements, incorrect estimations of parameters, fallacious causal inferences, and so forth. If explanations are appropriate here, then what requires explaining is why the very same methods produce an overwhelming background of failures and, occasionally, also a pattern of successes. The realist literature has not yet begun to address this question, much less to offer even a hint of how to answer it.

9. Of course, the realist can appropriate the devices and answers of the instrumentalist, but that would be cheating, and it would, anyway, not provide the desired support of realism per se.

10. Paul Teller has made this suggestion to me in conversation.

11. Ilkka Niiniluoto's "What Shall We Do with Verisimilitude?" *Philosophy of Science* 49 (1982): 181-197, contains interesting formal constructions for "degree of truthlikeness," and related versimilia. As conjectured above, they rely on an unspecified correspondence relation to the truth and on measures of the "distance" from the truth. Moreover, they fail to sanction that projection from some approximate truths to other, novel truths, which lies at the core of realist rationalizations.

12. See Gerald Holton, "Mach, Einstein, and the Search for Reality," in his *Thematic Origins of Scientific Thought* (Cambridge: Harvard University Press, 1973), 219-259. I have tried to work out the precise role of this positivist methodology in my "The Young Einstein and the Old Einstein," in *Essays in Memory of Imre Lakatos*, ed. R. S. Cohen et al. (Dordrecht: D. Reidel, 1976), 145-159.

13. A. Einstein et al., *The Principle of Relativity*, trans. W. Perrett and G. B. Jeffrey (New York: Dover, 1952), 117.

14. John Earman et al., "Lost in the Tensors," *Studies in History and Philosophy of Science* 9 (1978): 251-278. The tortuous path detailed by Earman is sketched by B. Hoffmann, *Albert Einstein, Creator and Rebel* (New York: New

American Library, 1972), 116-128. A nontechnical and illuminating account is given by John Stachel, "The Genesis of General Relativity," in *Einstein Symposium Berlin*, ed. H. Nelkowski et al. (Berlin: Springer-Verlag, 1980).

15. I have in mind the role played by the analysis of simultaneity in Einstein's path to special relativity. Despite the important study by Arthur Miller, *Albert Einstein's Special Theory of Relativity* (Reading: Addison-Wesley, 1981), and an imaginative pioneering work by John Earman (and collaborators), the details of which I have been forbidden to disclose, I think the role of positivist analysis in the 1905 paper has yet to be properly understood. My ideas here were sparked by Earman's playful reconstructions. So I cannot expose my ideas until John is ready to expose his.

16. Peter Barker, "Einstein's Later Philosophy of Science," in *After Einstein*, ed. P. Barker and C. G. Shugart (Memphis: Memphis State University Press, 1981), 133-146, is a nice telling of this story.

17. Roger Jones in "Realism About What?" (in draft) explains very nicely some of the difficulties here.

18. I think the ordinary, deflationist attitude of working scientists is much like that of Steven Weinberg, *Gravitation and Cosmology: Principles and Applications of the General Theory of Relativity* (New York: Wiley, 1972).

19. See B. L. van der Waerden, *Sources of Quantum Mechanics* (New York: Dover, 1967), 261.

20. See Linda Wessels, "Schrödinger's Route to Wave Mechanics," *Studies in History and Philosophy of Science* 10 (1979): 311-340.

21. A. Landé, "Albert Einstein and the Quantum Riddle," *American Journal of Physics* 42 (1974): 460.

22. Letter to Schrödinger, June 19, 1935. See my "Einstein's Critique of Quantum Theory: The Roots and Significance of EPR," in *After Einstein* (see n. 16), 147-158, for a fuller discussion of the contents of this letter.

23. Bas van Fraassen, "The Charybdis of Realism: Epistemological Implications of Bell's Inequality," *Synthese* 52 (1982): 25-38.

24. See my "Antinomies of Entanglement: The Puzzling Case of The Tangled Statistics," *Journal of Philosophy* 79, 12 (1982), for part of the discussion and for reference to other recent work.

25. The nonrealism that I attribute to students and practitioners of the quantum theory requires more discussion and distinguishing of cases and kinds than I have room for here. It is certainly not the all-or-nothing affair I make it appear in the text. I hope to carry out some of the required discussion in a talk for the American Philosophical Association meetings, Pacific Division, March 1983, entitled "Is Scientific Realism Compatible with Quantum Physics?" My thanks to Paul Teller and James Cushing, each of whom saw the need for more discussion here.

26. I should be a little more careful about the historical Bohr than I am in the text. For Bohr himself would seem to have wanted to truncate the homely line somewhere between the domain of chairs and tables and atoms, whose existence he plainly accepted, and that of electrons, where he seems to have thought the question of existence (and of realism, more generally) was no longer well defined.

An illuminating and provocative discussion of Bohr's attitude toward realism is given by Paul Teller, "The Projection Postulate and Bohr's Interpretation of Quantum Mechanics," pp. 201-223 n. 3. Thanks, again, to Paul for helping to keep me honest.

22. In this context, for example, van Fraassen's "constructive empiricism" would prefer the concept of empirical adequacy, reserving "truth" for an (unspecified) literal interpretation and believing in that truth only among observables. It is clear, nevertheless, that constructive empiricism follows the homely line and accepts the core position. Indeed, this seems to be its primary motivating rationale. If we reread constructive empiricism in our terminology, then, we would say that it accepts the core position but adds to it a construal of truth as empirical adequacy. Thus, it is antirealist, just as suggested in the next paragraph below. I might mention here that in this classification Putnam's internal realism also comes out as antirealist. For Putnam also accepts the core position, but he would add to it a Peircean construal of truth as ideal rational acceptance. This is a mistake, which I expect that Putnam will realize and correct in future writings. He is criticized for it, soundly I think, by Paul Horwich ("Three Forms of Realism") whose own "semantic realism" turns out, in my classification, to be neither realist nor antirealist. Indeed, Horwich's views are quite similar to what is called "NOA" below, and could easily be read as sketching the philosophy of language most compatible with NOA. Finally, the "epistemological behaviorism" espoused by Rorty is a form of antirealism that seems to me very similar to Putnam's position, but achieving the core parity between science and common sense by means of an acceptance that is neither ideal nor especially rational, at least in the normative sense. (I beg the reader's indulgence over this summary treatment of complex and important positions. I have been responding to Nancy Cartwright's request to differentiate these recent views from NOA. So if the treatment above strikes you as insensitive, or boring, please blame Nancy.)

28. "How To Compare Theories: Reference and Change," *Nous* 9 (1975): 17-32.

29. In his remarks at the Greensboro conference, my commentator, John King, suggested a compelling reason to prefer NOA over realism; namely, because NOA is less percussive! My thanks to John for this nifty idea, as well as for other comments.

30. "There is, I think, no theory-independent way to reconstruct phrases like 'really true'; the notion of match between the ontology of a theory and its 'real' counterpart in nature now seems to me illusive in principle." T. S. Kuhn, "Postscript," in *The Structure of Scientific Revolutions*, 2d ed. (Chicago: University of Chicago Press, 1970), 206. The same passage is cited for rebuttal by W. H. Newton-Smith, in *The Rationality of Science*. But the "rebuttal" sketched there in chapter 8, sections 4 and 5, not only runs afoul of the objections stated here in my first section, it also fails to provide for the required theory-independence. For Newton-Smith's explication of verisimilitude (p. 204) makes explicit reference to some unspecified background theory. (He offers either current science or the Peircean limit as candidates.) But this is not to rebut Kuhn's challenge (and mine); it is to concede its force.

31. No doubt I am optimistic, for one can always think of more to say. In particular, one could try to fashion a general, descriptive framework for codifying and classifying such answers. Perhaps there would be something to be learned from such a descriptive, semantical framework. But what I am afraid of is that this enterprise, once launched, would lead to a proliferation of frameworks not so carefully descriptive. These would take on a life of their own, each pretending to ways (better than its rivals) to settle disputes over truth claims, or their import. What we need, however, is less bad philosophy, not more. So here, I believe, silence is indeed golden.