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


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.


55. Ibid., 8.


### 3

**The Current Status of Scientific Realism**

Richard N. Boyd

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**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 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-
ccessively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories.

4. The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments.

Critics of realism in the empiricist tradition typically deny theses 1 and 2, and qualify their acceptance of 3 as to avoid commitment to the possibility of theoretical knowledge (however, van Fraassen accepts 1). Antirealists in the constructivist tradition, such as Kuhn, deny 4; however, they may well affirm 1 through 3 on the understanding that the "reality" which scientific theories describe is somehow a social and intellectual construct. As Kuhn and Hanson both argue, a constructivist perspective limits, however, the scope of application of 3, since successive theories can be understood as approximating the truth more closely only when they are part of the same general constructive tradition or paradigm. J. C. C. Smart's version of scientific realism departs from the typical conception in that he rejects 2, holding that distinctively philosophical considerations are required, over and above ordinary standards of scientific evidence, in order to justify our acceptance of the theoretical claims of scientific theories. Since Smart appears to hold that these philosophical considerations are nonvidential, it is perhaps appropriate to treat his position as intermediate between realism and constructivism.

In any event, the principal challenges to scientific realism arise from quite deep epistemological criticisms of 1 through 4. The key antirealist arguments, the standard rebuttals to them in the literature, as well as certain weaknesses in these rebuttals are summarized in the chart in the next section.

**ANTIREALISM IN THE EMPIRICIST TRADITION**

There is a single, simple, and very powerful epistemological argument that represents the basis for the rejection of scientific realism by philosophers in the empiricist tradition. Suppose that \( T \) is a proposed theory of unobservable phenomena, which can be subjected to experimental testing. A theory is said to be empirically equivalent to \( T \) just in case it makes the same predictions about observable phenomena that \( T \) does. Now, it is always possible, given \( T \), to construct arbitrarily many alternative theories that are empirically equivalent to \( T \) but which offer contradictory accounts of the nature of unobservable phenomena. Since scientific evidence for or against a theory consists in the confirmation or...
disconfirmation of one of its observational predictions, \( T \) and each of the
theories empirically equivalent to it will be equally well confirmed or dis-
confirmed by any possible observational evidence. Therefore, no scient-
ific evidence can bear on the question of which of these theories provides
the correct account of unobservable phenomena; at best, it might be pos-
sible to confirm or disconfirm the claim that each of these theories is a
reliable instrument for the prediction of observable phenomena. Since
this construction is possible for any theory \( T \), it follows that scientific
evidence can never decide the question between theories of unobservable
phenomena and, therefore, knowledge of unobservable phenomena is
impossible.

This is the central argument of the verificationist tradition. If sound, it
refutes scientific realism even if it is not associated with a version of the
verifiability theory of meaning. Meaningful or not, theoretical claims are
incapable of confirmation or disconfirmation. We may choose the "sim-
pIest model" for "pragmatic" reasons, but if evidence in science is experi-
mental evidence, then pragmatic standards for theory choice have nothing
to do with truth or knowledge. Scientific realism promises theoretical
knowledge of the world, where, at best, it can deliver only formal
elegance, or computational convenience.

As I have indicated in the chart, the empiricist argument we have been
considering depends on the epistemological principle that empirically
equivalent theories are evidentially indistinguishable. The evidential
indistinguishability thesis (whether explicit or implicit) represents the key
epistemological doctrine of contemporary empiricism and may be thought of
as a precise formulation of the traditional empiricist doctrine
("knowledge empiricism" in the phrase of Bennett)\(^8\) that factual knowl-
dge must always be grounded in experiences, and that there is no a priori
factual knowledge. (As I shall argue later, the evidential indistinguish-
ability thesis is the wrong formulation of the important epistemological
truth in that doctrine; still, it represents the way in which empiricist phil-
osophers of science—and most other empiricists, for that matter—have
understood the fundamental doctrine of empiricist epistemology.)

Let us now turn to the standard rebuttals to the antirealist application of
the indistinguishability thesis. Perhaps the most commonplace rebut-
tal to verificationist or empiricist arguments against realism is that the
distinction between observable and unobservable phenomena is not a
sharp one, and that the fundamental empiricist antirealist argument
therefore rests upon an arbitrary distinction.\(^7\) In assessing this rebuttal, it
is important to distinguish between the question of the truth of the claim
that the distinction between observable and theoretical entities is not
sharp, and the question of the appropriateness of this claim as a rebuttal
to empiricist antirealism. If scientific realism has somehow been estab-
lished, then it may well be evident that the distinction in question is epis-
temologically arbitrary: if we are able to confirm theories of, say, elec-
trons, then we may be able to employ such theories to design electron
detecting instruments whose "readings" may have an epistemological
status essentially like that of ordinary observations. If, however, it is sci-
entific realism that is in dispute, then the considerations just presented
would be inappropriately circular, even if their conclusion is ultimately
sound. Only a non-question-begging demonstration that the distinction
in question is arbitrary would constitute an adequate rebuttal to the
empiricist's strong prima facie case that experimental knowledge cannot
extend to the unobservable realm.

If we understand the rebuttal in question in this light, then several
responses are available to the empiricist which indicate its weakness as a
response to the central epistemological principle of empiricism. In the
first place, it is by no means clear that the empiricist need hold that there
is a sharp distinction between observable and unobservable phenomena
in order to show that the distinction is epistemologically nonarbitrary.
Suppose that there are entities that represent borderline cases of observa-
bility and suppose that there are cases in which it is not clear whether
something is being observed or not. Then there will be some entities
about which our knowledge will be limited by our capacity to observe
them, and there will be cases in which the evidence is equivocal about
whether there are entities of a certain sort at all. But the empiricist need
hardly resist these conclusions: they are independently plausible, and—
provided that there are some clear cases of putative unobservable entities
(atoms, elementary particles, magnetic fields, etc.)—the antirealist claims
of the empiricist are essentially unaffected.

Moreover, there are at least three ways in which the distinction in
question can be made sharper in an epistemologically motivated way. In
the first place, there is nothing obviously wrong with the traditional
empiricist distinction between sense data and putative external objects. It
is often claimed that the failure of logical positivists to construct a sense-
datum language shows that the observation-theory dichotomy cannot be
formulated in such terms, because it would be impossible to say of a
theory that evidence for or against it consists in the confirmation or dis-
confirmation of observational (that is, sense-datum) predictions that are
deduced from the theory. Quite so, but the fact remains that some expe-
riences are of the sort we expect on the basis of the acceptance of a given
theory, and others are of the sort we would not expect. Whatever the
relation of expectation is between theories and sensory experiences, we
may define empirical equivalence with respect to it, and affirm the empir-
The thesis of the evidential indistinguishability of empirically equivalent theories. The result is the classical empiricist formulation of "knowledge empiricism." Insofar as it is plausible, this version of knowledge empiricism provides an argument against scientific realism, even though it also poses the philosophical problem of explicating the relevant expectation relation. In any event, that relation might well be taken to be given by empirical facts about human understanding, rather than by philosophical analysis.

It is true, of course, that the sense-datum formulation of the evidential indistinguishability thesis leads to phenomenalism (at best) about physical objects and other persons. As early logical positivists recognized, this consequence makes it difficult to account for the apparent social and intersubjective character of scientific knowledge. To be sure, this difficulty provides a reason to doubt the truth of the evidential indistinguishability thesis in its sense-datum formulation. But it does not constitute a satisfactory rebuttal to that thesis, nor a satisfactory rebuttal to the anti-realist argument we are considering. The sense-datum version of the indistinguishability thesis is, after all, the obvious precise formulation of the doctrine that factual knowledge is always grounded in experience. The empiricist argument against realism is a straightforward application of that thesis. The fact that the thesis in question has inconvenient consequences shows neither that factual knowledge is not grounded in experience, nor that the (sense-datum version of) the indistinguishability thesis is not the appropriate explication of the doctrine that factual knowledge is grounded in this way. Considerations about the public character of science may provide us with reason to think that there must be something wrong with the phenomenalist's argument against scientific realism, but these considerations do not provide us with any plausible account of what is wrong with it. If I am right, the rebuttal to the sense-datum version of the evidential indistinguishability thesis displays a weakness common to all of the rebuttals to anti-empiricist arguments described in the chart. Each of the principal anti-empiricist arguments raises deep questions in epistemology or semantic theory against scientific realism. The standard rebuttals, insofar as they are effective at all, provide some reason to think that the antirealist arguments in question are unsound, or that realism is true, but they do not succeed in diagnosing the error in these arguments, nor do they point the way to alternative and genuinely realist conceptions of the central issues in epistemology or semantic theory.

It remains to examine the other two ways in which the dichotomy between observable and unobservable phenomena can be sharpened. On the one hand, phenomena might be classed as observable if they are quite plainly observable to persons with normal perceptual abilities. On the other hand, there is the proposal, which seems to be implicit in Maxwell, that entities that may not be directly observable to the unaided senses should count as observable for the purposes of the epistemology of science if they can be detected by the senses when the senses have been aided by devices whose reliability has been previously established by procedures that do not beg the question between empiricists and scientific realists. Roughly at least, the latter proposal can be put this way: Let $O_n$ be the class of entities that are observable to the typical unaided senses; for any $n$, let $O_{n+1}$ be the class of entities that are detectable by procedures whose legitimacy can be established on the basis of theories that can be established (and can be applied to justify those procedures) without presupposing the existence of entities not in $O_n$; the union of the sets $O_n$ is the class of observables in the sense relevant to the epistemology of science.

Neither of these proposals is without difficulties. Either can be challenged from the perspective of traditional empiricism by a simple application of the sense-datum version of the evidential indistinguishability thesis. The proposal that observability should be defined in terms of what is plainly observable to the unaided senses may be challenged for failing to account, for example, for observations made through a simple light microscope or telescope. The more generous conception is open to the challenge that it fails to see the force of the evidential indistinguishability thesis with respect to its own conception of observability—that it fails, for example, to recognize that there are infinitely many different and evidentially indistinguishable hypotheses that could explain the intersubjectively observable images which are the objective data of light microscopy.

In any event, each of these proposals reflects an important aspect of the intuitive conception that experimental knowledge is grounded in observation. What is important for our purposes is that either account of observability is sufficient to sustain a significant antirealist application of the evidential indistinguishability thesis. That this is true for the less generous conception of observability is obvious. In regard to the more generous conception, it is important to recognize that what is proposed is not that one may treat as observable whatever phenomena can be identified by "inductive inference to the best explanation" as causes of the results of laboratory "measurement" or "detection." A general appeal to a principle of inductive inference to theoretical explanations would beg the question against the empiricist in this context. Instead, the proposed account of observability depends crucially on the conception that theories whose confirmation by observations is unproblematic from an empiricist point of view can be employed to legitimize an additional
level of observables and that this process can then be iterated. The example of light microscopy is illustrative here: the idea is that the lens-makers' equations can be confirmed in a fashion entirely acceptable to empiricism, and that these equations can then be used to legitimize interpreting the images observed through a microscope as images of otherwise unobservable entities.

It is not clear that this approach even gets off the ground as a non-question-begging account of observability. Arguably, the empiricist will hold that the lens-makers' equations, for example, are confirmable only insofar as they are understood to apply to unproblematically observable entities. The application of these equations that underlies the broader conception of observability requires that they be confirmed even when they are understood to apply to the very entities whose observability they are supposed to legitimize. It is by no means clear that objections such as this do not yield the conclusion that $0_n = 0_{n+1}$ for all $n$.

Even if this problem is somehow circumvented, it is still true that the generous definition of observability is unlikely to legitimize knowledge of the standard "unobservables" which worry the philosopher of science. The reason is this: the account of observability we are considering cannot work to legitimize as observable putative entities that are such that the available procedures for (as a realist would say) measuring and detecting them depend upon explicit theories of those entities themselves, or (worse yet) upon theories of other (putative) entities as well, which are equally unobservable in the traditional sense. In such cases only a question-begging inductive inference to a theoretical explanation of the results of the relevant "measurements" or "detections" would suffice to legitimize the entities in question. But it is almost certain that the basic unobservable putative features of matter (atoms, their constituent particles, electric and magnetic fields, etc.) fall into the category of entities for which legitimization would be question-begging. Therefore the central claims of antirealist empiricism in the philosophy of science will be sustained even if the evidential indistinguishability thesis is so understood as not to rule out the use of, for example, light microscopes in scientific observations.

We may apparently conclude the following about the rebuttal to empiricist antirealist arguments which turns on the claim that the distinction between observable entities and unobservable ones is not sharp and that the empiricist argument therefore rests upon an epistemologically arbitrary distinction: The distinction in question need not be sharp in order to be nonarbitrary. Moreover, there are at least three epistemologically motivated ways of making it sharper. An examination of each of these refinements of the distinction indicates features that might make it reasonable to suppose that there is something problematic about the basic empiricist argument against realism, but none of these considerations provides any diagnosis of the error, nor do any of them allow us to foresee any alternative to the doctrine of evidential indistinguishability of empirically equivalent theories upon which the empiricist argument depends. The standard rebuttals are inadequate in the face of the serious epistemological issues raised by the empiricist position.

I said that we may apparently reach these conclusions because it may seem that I have overlooked the real force of the rebuttal under consideration. The real force, it might seem, lies in the following consideration: it has often happened that scientists have postulated unobservable entities and have developed and confirmed, to their satisfaction, theories about them, and that they have much later been able, on the basis of those very theories, to measure or detect those very entities whose existence they earlier had postulated. Examples may include germs, viruses, atoms, and neutrinos. Surely, this shows that the sorts of inductive inference to theoretical explanations in which scientists engage are reliable, whatever empiricists may say.

Taken at face value, this argument is question-begging: it assumes at the outset that what scientific realists describe as "measurement" and "detection" of the entities in question are really measurement and detection. But there is an argument for realism lurking here. It does not turn on the claim that the empiricist has drawn the observable-unobservable dichotomy arbitrarily; such a reading makes the argument question-begging. Instead, what we have is an example of the third anti-empiricist rebuttal (indicated on the chart). In general, that rebuttal points to the astonishing predictive reliability of well-confirmed scientific theories as evidence that they must be approximately true as descriptions of unobservable entities. The cases of predictive reliability that makes this argument plausible are typically those in which predictions quite different from the ones that were involved in the initial confirmation of a theory—and especially predictions arrived at by calculations that take the theoretical machinery of the theory quite seriously—turn out to be surprisingly accurate. In such cases, it seems that miracles are the only alternative to a realist explanation of the success of scientific practice. Cases in which what is predicted are results of (what a realist would call) "measurement" or "detection" of the postulated unobservable entities are especially clear examples of the cases to which this argument applies.

This rebuttal to empiricist antirealism has considerable force (indeed, it is probably the argument that reconstructs the reason why most scientific realists are realists). But it suffers from the same defect that we observed earlier in the case of the first rebuttal: while it provides good
reason to think that there must be something wrong with the empiricists’ argument, it affords us no diagnosis of what is wrong with it. No rebuttal to the basic epistemological principle of the empiricist argument (the evidential indistinguishability thesis) follows from this rebuttal; nor is there any rebuttal to the application of that basic principle to the issue of scientific realism. We are provided with a reason to suppose that realism is true, but we are not provided with any epistemology to go with that conclusion.

There remains one rebuttal among the standard responses to empiricist antirealism, and it does seem to challenge directly the evidential indistinguishability thesis. The evidential indistinguishability thesis asserts that empirically equivalent theories are evidentially indistinguishable. But it has been widely recognized by philosophers of science that this is wrong. It might be right, they would argue, if the only predictions from a theory that are appropriate to test are those that can be deduced from the theory in isolation. But it is universally acknowledged that in theory testing we are permitted to use various well-confirmed theories as “auxiliary hypotheses” in the derivation of testable predictions. Thus, two different theories might be empirically equivalent—they might have the same consequences about observable phenomena—but it might be easy to design a crucial experiment for deciding between the theories if one could find a suitable set of auxiliary hypotheses such that when they were brought into play as additional premises, the theories (so expanded) were no longer empirically equivalent.

There is almost no doubt that considerations of this sort rebut any verificationalist attempt to classify individual statements or theories as literally meaningful or literally meaningless by the criterion of verifiability in principle. But there is no reason to suppose that the rebuttal based on the role of auxiliary hypotheses is fatal to the basic claim of the evidential indistinguishability thesis, or to its antirealistic application. The reason is this: we may reformulate the evidential indistinguishability thesis so that it applies not to individual theories, but to “total sciences.” The thesis, so understood, then asserts that empirically equivalent total sciences are evidentially indistinguishable. Since total sciences are self-contained with respect to auxiliary hypotheses, the rebuttal we have been considering does not apply, and the revised version of the evidential indistinguishability thesis entails that at no point in the history of science could we have knowledge that the theoretical claims of the existing total science are true or approximately true.10

One objection that has sometimes been offered against the employment of the notion of a total science is the observation that if, by a “total science,” one means the set of well-established theories at a particular time in the history of science, then total sciences are almost certainly always logically inconsistent and, therefore, they have all possible observational consequences and cannot be experimentally confirmed. In this case, as in the case of the objection discussed earlier to the sense-datum version of the evidential indistinguishability thesis, there is an obvious reply. Somehow, scientists manage to cope with inconsistent total sciences; they have a good idea of which tentatively accepted or merely approximate (as they might say) theories should not be employed together in making predictions. They have a pretty good idea which predictions not to trust. All we need to do is to define empirical equivalence with respect to the practice of scientists. The evidential indistinguishability thesis formulated with respect to total sciences in this way yields the antirealist conclusion of empiricists, and it certainly seems reasonable to hold that such version of the evidential indistinguishability thesis represents the obvious interpretation of “knowledge empiricism” once the role of auxiliary hypotheses is acknowledged. Thus, the fact that auxiliary hypotheses play a crucial role in theory confirmation does not constitute a significant rebuttal to a sophisticated version of the standard empiricist argument against scientific realism.

There is a point regarding the use of auxiliary hypotheses that can be made the basis for a very strong defense of scientific realism. The use of auxiliary hypotheses, like other applications of what positivists called the “unity of science” principle, depends upon judgments of univocality regarding different occurrences of the same theoretical terms. It is possible to argue that only a realist conception of the semantics and epistemology of science can account for the role of such univocality judgments in contributing to the reliability of scientific methodology.11 But this argument is not anticipated in the standard rebuttals to empiricist antirealism.

One must conclude that the standard rebuttals to the central empiricist argument against scientific realism are significantly flawed. Where they do provide reason to suspect that the empiricist argument is unsound (or, more directly, that realism is true), they do not provide any effective rebuttal to the main epistemological principle (the evidential indistinguishability thesis) upon which the empiricist argument depends, nor do they indicate respects in which the application of that principle to the question of realism is unwarranted.

CONSTRUCTIVIST ANTIREALISM

There is a single basic empiricist argument against realism, and it is an argument of striking simplicity and power. In the case of constructivist antirealism, the situation is much more complex. In part, at least, this is
so because constructivist philosophers of science have typically been led to antirealist conclusions by reflections upon the results of detailed examinations of the history and actual methodological practices of science as well as by reflections on the psychology of scientific understanding. Different philosophers have focused on different aspects of the complex procedures of actual science as a basis for antirealist conclusions. Nevertheless, it is possible, I believe, to identify the common thread in all of these diverse arguments. Roughly, the constructivist antirealist reasons as follows: The actual methodology of science is profoundly theory-dependent. What scientists count as an acceptable theory, what they count as an observation, which experiments they take to be well designed, which measurement procedures they consider legitimate, what problems they seek to solve, and what sorts of evidence they require before accepting a theory—which are all features of scientific methodology—are in practice determined by the theoretical tradition within which scientists work. What sort of world must there be, the constructivist asks, for this sort of theory-dependent methodology to constitute a vehicle for gaining knowledge? The answer, according to the constructivist, is that the world that scientists study, in some robust sense must be defined or constituted by or "constructed" from the theoretical tradition in which the scientific community in question works. If the world that scientists study were not partly constituted by their theoretical tradition, then, so the argument goes, there would be no way of explaining why the theory-dependent methods that scientists use are a way of finding out what is true.

To this argument there is typically added another which addresses an apparent problem with constructivism. The problem is that scientists seem sometimes to be forced by new data to abandon important features of their current theories and to adopt radically new theories in their place. This phenomenon, it would seem, must be an example of scientific theories being brought into conformity with a theory-independent world, rather than an example of the construction of reality within a theoretical tradition. In response to this problem, constructivism often asserts that successive theories in science that represent the sort of radical "breaks" in tradition at issue are "incommensurable." The idea here is that the standards of evidence, interpretation, and understanding dictated by the old theory, on the one hand, and by the new theory, on the other hand, are so different that the transition between them cannot be interpreted as having been dictated by any common standards of rationality. Since there are no significant theory-independent standards of rationality, it follows that the transition in question is not a matter of rationally adopting a new conception of (theory-independent) reality in the light of new evidence; instead, what is involved is the adoption of a wholly new conception of the world, complete with its own distinctive standards of rationality. In its most influential version, this argument incorporates the claim that the semantics of the two consecutive theories change to such an extent that those terms that they have in common should not be thought of as having the same referents in the two theories. Thus, transitions of the sort we are discussing ("scientific revolutions" in Kuhn's terminology) involve a total change of theoretical subject matter.

There are two closely related standard rebuttals to these antirealist arguments. In the first place, against the claim that realism must be abandoned because scientific methodology is too theory-dependent to constitute a discovery (as opposed to a construction) procedure, it is often replied that for any two rival scientific theories it is always possible to find a methodology for testing them that is neutral with respect to the theories in question. Thus the choice between rival scientific theories on the basis of experimental evidence can be rational even though experimental methodology is theory-dependent. The outcome of a "crucial experiment" that pits one rival theory against another need not be biased, since such an experiment can be conducted on the basis of a methodology that—however theory-dependent—is not committed to either of the two contesting theories.

Against the incommensurability claim, it is often argued that an account of reference for theoretical expressions can be provided that makes it possible to describe scientific revolutions as involving continuity in reference for the theoretical terms common to the laws of the earlier and later theoretical traditions or "paradigms." With such referential continuity comes a kind of continuity of methodology as well, because (assuming continuity of reference) the actual cases of scientific revolutions typically result in the preservation of some of the theoretical machinery of the earlier paradigm in the structure of the new one, and this, in turn, guarantees a continuity of methodology.

Neither of these rebuttals is fully adequate as a response to constructivist antirealism. Consider first the claim that for any two rival theories there is a methodology for testing them that is neutral with respect to the issues on which they differ ("pair-wise theory-neutrality of method" in the chart). It is generally true that for theoretical rivalries that arise in actual science, a relevantly neutral testing methodology will exist. Indeed, the use of such "neutral" testing methodologies is a routine part of what Kuhn calls "normal science." And indeed, the existence of such methodologies helps to explain how scientists can appeal to common standards of rationality even when they have theoretical differences of the sort that influence methodological judgments. Nevertheless, pair-wise theory-neutrality of method does not provide a reason to reject the antirealist conclusions of the constructivist.

Remember that what the constructivist argues is that a general method-
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ology that is predicated upon a particular theoretical tradition and is theory-determined to its core cannot be understood as a methodology for discovering features of a world that is not in some significant way defined by that tradition. All that the doctrine of pair-wise theory-neutral methods asserts is that within the theoretical and methodological tradition in question, there are available experimental procedures that are neutral with respect to quite particular disputes between alternative ways of modifying or extending that very tradition. There is no suggestion of a procedure by which scientific methodology can escape from the presuppositions of the tradition and examine objectively the structure of a theory-independent world. Insofar as the profound theory-dependence of method raises an epistemological problem for realism, the pair-wise theory-neutrality of methods does not provide an answer to it.

Perhaps surprisingly, it does not help either to demonstrate that successive paradigms are commensurable. Suppose that a satisfactory account of referential continuity for theoretical terms during scientific revolutions is available. Suppose further (what is not implied by the former claim) that the theoretical continuity thus established during revolutionary periods is such that the transition between the prerevolutionary theory and the postrevolutionary one is governed by a continuously evolving standard of scientific rationality. If these suppositions are true, then much of what Kuhn, for example, has claimed about the history of science will be mistaken: postrevolutionary scientists will (contrary to Kuhn) be building on the theoretical achievements of their prerevolutionary predecessors; the adoption of new "paradigms" will be scientifically rational; and it will not involve a "Gestalt shift" in the scientific community's understanding of the world, whatever may be the case for some individual scientists. But, the basic constructivist epistemological objection to scientific realism will still be unrebuted. If the theory-dependence of methodology provides reason to doubt that scientific inquiry possesses the right sort of objectivity for the study of a theory-independent world, then the sort of historical continuity through scientific revolutions we are considering will not address that doubt. Only if the transitional methodology during revolutions were largely theory-neutral would the fact of methodological and semantic continuity between revolutions provide, by itself, a rebuttal to the constructivist antirealist; but there is no chance that such theory-independence could be demonstrated by the sort of rebuttal to incommensurability we are considering. Indeed, there is no reason of any sort to suppose that such a theory-neutral method ever prevails.

In the present case, as in the case of the standard rebuttals to empiricist antirealism, it is by no means true that the standard rebuttals to the con-

structivist arguments are irrelevant to the issue of scientific realism. If there were no such phenomenon as pair-wise theory-neutrality of method, then it would be hard to see how there could be any sort of scientific objectivity, realist or constructivist. If there is no way of defending the continuity of subject matter and methodology during most of the episodes which Kuhn calls scientific revolutions, then the realist conception of science is rendered most implausible. The point is that, even though these prorealist rebuttals to constructivist antirealism do provide some support for aspects of the realist position, they fail to offer any reason to reject the basic epistemological argument against realism which the constructivist offers.

EMPIRICISM AND CONSTRUCTIVISM

Kuhn presents his constructivist account of science as an alternative to the tradition of logical empiricism and, indeed, there is much he says with which traditional positivists would disagree. There are, nevertheless, important similarities between the constructivist and the empiricist approach to the philosophy of science. Kuhn, for example, relies on the late positivist 'law-cluster' account of the meaning of theoretical terms in his famous argument against the semantic commensurability of successive paradigms. Similarly, R. Carnap's mature positivism of the early 1950s has much in common with Kuhn's views. In particular, "Empiricism, Semantics, and Ontology" offers an account of the criteria for the rational acceptance of a linguistic framework which is surprisingly like a formalized version of Kuhn's view. We may say with some precision what the points of similarity between Kuhn and Carnap are. In the first place, they are agreed that the day-to-day business of the development and testing of scientific theories is governed by broader and more basic theoretical principles, including the most basic laws and definitions of the relevant sciences.

There is a far deeper point of agreement. Kuhn, and constructivists generally, cannot consistently accept the principle of the evidential indistinguishability of empirically equivalent total sciences; they hold, after all, that 'facts'—insofar as they are the subject matter of the sciences—are partly constituted or defined by the adoption of "paradigms" or theoretical traditions, so that there is a sort of a priori character to the scientist's knowledge of the fundamental laws in the relevant tradition or paradigm. But they agree with logical empiricists in holding that any rational constraint on theory acceptance that is not purely pragmatic and that does not accord with the evidential indistinguishability thesis must be essen-
tially conventional. For Carnap and other positivists, the conventions are essentially linguistic: they amount to the conventional adoption of one set of "L-truths" rather than another. For Kuhn and other constructivists, the conventions go far deeper: they amount to the social construction of reality and of experimental "facts." What neither empiricists nor constructivists accept is the idea that the regulation of theory acceptance by features (linguistic or otherwise) of the existing theoretical tradition can be a reliable guide to the discovery of theory-independent matters of fact.

Of course, empiricists and constructivists differ, especially regarding the extent to which experimental observations can be divorced from theoretical considerations, and (if constructivists are "relativists" in the Kuhnian tradition) about the methodological commensurability of successive theoretical traditions or paradigms. It is interesting to note that Kuhn and the Carnap of the early 1950s do not disagree about the semantical incommensurability of the theoretical portions of alternative linguistic frameworks for science—neither accepts any doctrine of continuity of reference for theoretical terms in the transition to alternative frameworks. Indeed, for Carnap, questions of reference and ontology are meaningless when raised outside the scope of some particular linguistic framework. That Kuhn and Carnap should agree to this extent about the semantics of theoretical terms is less surprising when one realizes that Kuhn's account of the meaning of such terms is simply a more subtle and historically more accurate version of Carnap's.²

One further point of agreement between empiricists and constructivists is significant for our purposes. Empiricist philosophers of science deny that knowledge of theoretical entities is possible. But it is no part of contemporary empiricism to deny that the scientific method yields objective instrumental knowledge: knowledge of regularities in the behavior of observable phenomena. It is important to see that this point is not seriously contested by constructivist philosophers of science. It is true that constructivists insist that observation in science is significantly theory-determined, and that Kuhn, for example, emphasizes that experimental results that are anomalous in the light of the prevailing theoretical conceptions are typically ignored if they cannot readily be assimilated into the received theoretical framework. But no serious constructivist maintains that the predictive reliability of theories in mature science or the reliability of scientific methodology in identifying predictively reliable theories is largely an artifact of the tendency to ignore anomalous results. Such a view would be nonsensical in the light of the contributions of pure science to technological advance.

There is one point that, whether ultimately compatible with empiri-

cism or not, is certainly emphasized by constructivists much more than by empiricists, and is especially relevant when one considers the role of scientific methodology in producing instrumental knowledge. It was recognized early on by logical empiricists that any account of the methodology of science requires some account of the way in which the "degree of confirmation" of a theory, given a body of observational evidence, is to be determined. More recently, N. Goodman has, following Locke, raised a question that is really a special case of the problem of determining degree of confirmation. Any account of the methodology of science must account for judgments of 'projectability' of predicates or, to put the issue more broadly, it must provide an account of the standards by which scientists determine which general conclusions are even real candidates for acceptance given an (always finite) body of available data. This question is interesting precisely because, given any finite body of data, there are infinitely many different general theories that are logically consistent with those data (indeed, there will be infinitely many such theories that are pair-wise empirically inequivalent, given the existing total science as a source of auxiliary hypotheses).

What Kuhn and other constructivists insist (correctly, I believe) is that judgments of projectability and of degrees of confirmation are quite profoundly dependent upon the theories that make up the existing theoretical tradition or paradigm. The theoretical tradition dictates the terms in which questions are posed and the terms in which possible answers are articulated. In a similar way, theoretical considerations dictate the standards for experimental design and for the assessment of the experimental evidence. Assuming this to be true, and assuming, as reasonable constructivists must, that the reliability of scientific methodology in producing instrumental knowledge is not to be explained largely by the tendency to ignore anomalous data, we can see that an important epistemological issue emerges regarding judgments of projectability and of degree of confirmation: why should so theory-dependent a methodology be reliable at producing knowledge about (largely theory-independent) observable phenomena?

A related question about what we might call the "instrumental reliability" of scientific method should prove challenging both to Kuhn, and to empiricists who share with Kuhn the law-cluster theory of the meaning of theoretical terms. Judgment of univocality for particular occurrences of the (lexicographically) same theoretical term play an important epistemological role in scientific methodology. This is evident since commonplaces such as the use of auxiliary hypotheses in theory testing, or applications of the principle of "unity of science" in the derivation of observational predictions from theories that have already been accepted, depend
upon prior assessments of univocality. This means that scientific standards for the assessment of univocality for token occurrences of theoretical terms must play a crucial epistemological role, and it must be the business of an adequate account of the language of science to say what those standards are and why they are such as to render instrumentally reliable the methodological principles in actual science which depend upon univocality judgments.21

Unlike earlier positivist theories of meaning for theoretical terms (like operationalism, for example) the law-cluster theory does not say what it is for two tokens of orthographically the same theoretical term to occur with the same meaning or reference. The meaning of a theoretical term is given by the most basic laws in which it occurs; this may possibly tell us something about diachronic questions about univocality of theoretical terms. But suppose that t and t' are two tokens of orthographically the same theoretical term, used at the same time, and that neither t nor t' occurs in a law that is fundamental in the sense relevant to the law-cluster theory. This latter condition describes the circumstances of almost all tokens of theoretical terms in actual scientific usage. Under the circumstances in question, the law-cluster theory says nothing about the question of whether t and t' have the same meaning or reference. Only when the synchronic problem of univocality in such cases is presumed to have already been solved does the law-cluster theory have anything to say about univocality for theoretical terms. The law-cluster theory is thus entirely without the resources to address the important question of the contribution that judgments of univocality for theoretical terms make to the instrumental reliability of scientific methodology.

Thus we have identified two questions that pose especially sharp challenges to both empiricist and constructivist conceptions of science: why are theory-dependent standards for assessing projectability and degrees of confirmation instrumentally reliable? and, how do judgments of univocality for theoretical terms contribute to the instrumental reliability of scientific methodology? I shall argue in the next section that answers to these challenges provide the basis for a new and more effective defense of scientific realism.

DEFENDING SCIENTIFIC REALISM

Elsewhere, I have offered a defense of scientific realism against empiricist antirealism which proceeds by proposing that a realistic account of scientific theories is a component in the only scientifically plausible explanation for the instrumental reliability of scientific methodology.22 What I propose to do here is to summarize this defense very briefly and to indicate how it also constitutes a defense of scientific realism against constructivist criticisms, and how it avoids the weaknesses in the traditional rebuttals to antirealist arguments.

The proposal that scientific realism might be required in order to explain adequately the instrumental reliability of scientific methodology can be motivated by reexamining the principal constructivist argument against scientific realism (see 2. a in the chart). The constructivist asks, What must the world be like in order that a methodology so theory-dependent as ours could constitute a way of finding out what is true? She answers: The world would have to be largely defined or constituted by the theoretical tradition that defines that methodology. It is clear that another answer is at least possible: the world might be one in which the laws and theories embodied in our actual theoretical tradition are approximately true. In that case, the methodology of science might progress dialectically. Our methodology, based on approximately true theories, would be a reliable guide to the discovery of new results and the improvement of older theories. The resulting improvement in our knowledge of the world would result in a still more reliable methodology leading to still more accurate theories, and so on.23

What I have argued in the works cited above is that this conception of the enterprise of science provides the only scientifically plausible explanation for the instrumental reliability of the scientific method. In particular, I argue that the reliability of theory-dependent judgments of projectability and degrees of confirmation can only be satisfactorily explained on the assumption that the theoretical claims embodied in the background theories which determine those judgments are relevantly approximately true, and that scientific methodology acts dialectically so as to produce in the long run an increasingly accurate theoretical picture of the world. Since logical empiricists accept the instrumental reliability of actual scientific methodology, this defense of realism represents a cogent challenge to logical empiricist antirealism. It remains to be seen whether it has the weaknesses of more traditional responses to empiricist antirealism, but, first, let us examine its relevance to constructivism.

First, it should be observed that the argument for realism that I have indicated is a direct response to the central constructivist argument against realism. If the argument for realism is correct, then we can see what is wrong with the central constructivist argument; the constructivist’s epistemological challenge to scientific realism rests upon the wrong explanation for the reliability of the scientific method as a guide to truth.

It is equally important to see that there is no answer within a purely constructivist framework to the question of why the methods of science
are instrumentally reliable. The instrumental reliability of particular scientific theories cannot be an artifact of the social construction of reality. Even within "pure" science this is acknowledged, for example, by Kuhn. The anomalous observations that (sometimes) give rise to "scientific revolutions" cannot be reflections of a fully paradigm-dependent world: anomalies are defined as observations that are inexplicable within the relevant paradigm. It is even more evident that theory-dependent technological progress (the most striking example of the instrumental reliability of scientific methods as well as theories) cannot be explained by an appeal to social construction of reality. It cannot be that the explanation for the fact that airplanes, whose design rests upon enormously sophisticated theory, do not often crash is that the paradigm defines the concept of an airplane in terms of crash resistance. If the empiricist cannot offer a satisfactory account of the instrumental reliability of scientific method (as I have argued in the works cited), then the constructivist—who even more than the empiricist emphasizes the theory dependence of that method—cannot do so either. Thus, the epistemological thrust of constructivism is directly challenged by the argument for scientific realism under consideration.

It is clear, moreover, that if scientific realism is defended in this way, then the more traditional rebuttals to constructivist antirealism are rendered fully effective. If the fundamental epistemological thrust of constructivism is mistaken, then (as I indicated earlier) the pair-wise theory-neutrality of scientific methodology, and the continuity of reference of theoretical terms and methods across "revolutions" are crucial components in the defense of scientific realism.

Let us turn now to the question of whether the defense of realism we are considering has the weakness of the more traditional rebuttals to empiricist antirealism. Those rebuttals had the defect that, while they provided some reason to believe that scientific realism is true, they offered no insight into the question of what is wrong with the crucial empiricist argument against realism. Here, the argument under consideration succeeds where the more traditional arguments fail. What is wrong with the fundamental empiricist argument is that the principle that empirically equivalent total sciences are evidentially indistinguishable is false, and it represents the wrong reconstruction of the perfectly true doctrine that factual knowledge is grounded in observation.

The point here is that if the realist and dialectical conception of scientific methodology is right, then considerations of the theoretical plausibility of a proposed theory in the light of the actual (and approximately true) theoretical tradition are evidential considerations: results of such assessments of plausibility constitute evidence for or against proposed theories. Indeed, such considerations are a matter of theory-mediated empirical evidence, since the background theories, with respect to which assessments of plausibility are made, are themselves empirically tested (again, in a theory-mediated way). Theory-mediated evidence of this sort is no less empirical than more direct experimental evidence—largely because the evidential standards that apply to so-called direct experimental tests of theories are theory-determined in just the same way that judgments of plausibility are. In consequence, the actual theoretical tradition has an epistemically privileged position in the assessment of empirical evidence. Thus, a total science whose theoretical conception is significantly in conflict with the received theoretical tradition is, for that reason, subject to "indirect" but perfectly real prima facie disconfirmation relative to an empirically equivalent total science that reflects the existing tradition. The evidential indistinguishability thesis is therefore false, and the basic empiricist antirealist argument is fully rebutted.

It might seem that the realist conception that theoretical considerations in science are evidential would reflect a weakening of ordinary standards of evidential rigor in science. After all, on the realist conception, a theory can get evidential support both from (direct) experimental evidence and from (indirect) theoretical considerations. Moreover, the realist proposal might seem to make it impossible to disconfirm traditional theories, treating them as a priori truths in much the same way that the constructivist conception does. Neither of these claims proves to be sound. In the first place, rigorous assessment of experimental evidence in science depends fundamentally upon just the principle that theoretical considerations are evidential: that is why a realist conception of theories is necessary to account for the instrumental reliability of our standards for assessing experimental evidence. Second, the realist conception of theory-mediated experimental evidence does not have the consequence that any traditional laws are immune from refutation. Instead, it provides the explanation of how rigorous testing of these and other laws is possible. The dialectical process of improvement in the theoretical tradition does not preclude, but instead requires, that particular laws or principles in the tradition may have to be abandoned in the light of new evidence.

Let us turn now to the second puzzle about the instrumental reliability of scientific method which was raised at the end of the preceding section: how to account for the epistemic reliability of judgments of univocality for theoretical terms. The realistic account of the instrumental reliability of judgments of projectability requires that the kinds or categories into which features of the world are sorted for the purpose of inductive inference be determined by theoretical considerations rather than being fixed by conventional definitions, however abstract. In particular, the law-
cluster theory of meaning, understood conventionally, is inadequate as an account of the "definitions" of theoretical terms in science. It has been widely recognized that if theoretical terms in science are to refer to entities or kinds whose "essences" are determined by empirical investigation rather than by stipulation, then the traditional conception of reference fixing by stipulatory conventions must be abandoned for such terms in favor of some "causal" or "naturalistic" theory of reference.26

Given the distinctly realistic conception of scientific knowledge described previously, it is possible to offer a naturalistic theory of reference which is especially appropriate to an understanding of the role of theoretical considerations in scientific reasoning. Such a theory defines reference in terms of relations of "epistemic access."27 Roughly, a (type) term t refers to some entity e just in case complex causal interactions between features of the world and human social practices bring it about that what is said of t is, generally speaking and over time, reliably regulated by the real properties of e. Because such regulation of what we say by the real features of the world depends upon the approximate truth of background theories, the approximate reliability of measurement and detection procedures, and the like, the epistemic access account of reference can explain the grains of truth in such previous accounts of reference as the law-cluster theory, or operationalism.28

Consider now the question of univocality for two token occurrences of orthographically the same theoretical term. Such a pair of terms will be co-referential just in case the social history of each of their occurrences links them, by the relevant sort of causal relations, to a situation of reliable belief regulation by the actual properties of the same feature of the world. The relevant sorts of causal relations are to be determined by epistemology, construed as an empirical investigation into the mechanisms of reliable belief regulation. Thus it is an empirical question, not a conceptual one, whether two such tokens are univocal.

Because the epistemic access account of reference can explain the grains of truth in the other theories of reference for theoretical terms which have been advanced to explain the actual judgments of scientists and historians about issues of univocality, there is every reason to believe that the epistemic access account can explain why the ordinary standards for judging univocality that prevail in science are reliable indicators of actual coreferentiality. Together with the realist's conception that scientific methodology produces (typically and over time) approximately true beliefs about theoretical entities, the epistemic access account of reference provides an explanation of how univocality judgments contribute to the reliability of scientific methodology, an explanation that is fully in accord with the general realist conception of scientific methodology described here.

Finally, the epistemic access account provides a precise formulation of the crucial realist claim that typically (perhaps despite changes in law-clusters) there is continuity of reference across "scientific revolutions."29 Indeed, it permits us to integrate cases of what H. Field calls "partial denotation"30 into a general theory of reference and thus to treat cases of "denotational refinement" as establishing referential continuity in the relevant sense.

If the dialectical and realistic conception of scientific methodology described here and the related epistemic access conception of reference are approximately correct, then together they constitute a rebuttal to both empiricist and constructivist antirealism which suffers none of the shortcomings of the more traditional rebuttals, while at the same time accommodating the insights that the more traditional rebuttals provide.

SCIENTIFIC REALISM AND METAPHILOSOPHY

I have examined traditional rebuttals to antirealist arguments in the empiricist and constructivist traditions and have suggested that these rebuttals have the weakness that they do not provide a diagnosis of the epistemological errors that must—if realism is true—lie behind the standard argument against realism. I indicated how a distinctly realistic and dialectical conception of scientific methodology, together with a closely related naturalistic conception of reference, could provide the basis for a defense of realism that does diagnose the epistemological errors in antirealist arguments. If the conception of scientific knowledge and language described here is correct, then it has implications for philosophical methodology which are sufficiently startling that they may help to explain why the dialectical and realist account of the reliability of scientific methodology was not put forward earlier as the epistemological foundation for scientific realism.

I believe it is fair to say that scientific realists have had a conception of their dispute with empiricist and (more recently) with constructivist antirealists according to which they shared with their opponents a general conception of the logic and methods of science, and according to which the dispute between realists and antirealists was over whether that logic and those methods were adequate to secure theoretical knowledge of a theory-independent reality. It was not anticipated that a new and distinctly realist general account of the methods of science would be necessary in order to defend scientific realism. This conception of a shared account of the logic and methods of science was advanced explicitly by E. Nagel, in discussing the realism-empiricist dispute:
It is difficult to escape the conclusion that when the two opposing views on the
cognitive status of theories are stated with some circumspection, each can assimil-
ate into its formulation not only the facts concerning the primary subject matter
explored by experimental inquiry but also the relevant facts concerning the logic
and procedures of science. In brief, the opposition between these views is a con-
ict over preferred mode of speech. 11

It is evident that the argument for scientiﬁc realism described in the
preceding section departs from this understanding. According to that
argument, no empiricist or constructivist account of the methods of sci-
ence can explain the phenomenon of instrumental knowledge in science,
the very kind of scientiﬁc knowledge about which realists, empiricists,
and constructivists largely agree. Only on a distinctly realist conception
of the logic and methods of science—a conception that empiricists and
constructivists cannot share—can instrumental knowledge be explained.

The distinctly realist conception of the methodology of science departs
even farther from the normal conception of the epistemology of science.
At least since Descartes, the characteristic conception of epistemology in
general has been that the most basic epistemological principles—the basic
canons of reasoning or justiﬁcation—should be defeasible a priori. Thus,
for example, almost all empiricists have thought that “knowledge empirici-
cism” represented an a priori truth about knowledge, and that the most
basic principles of inductive reasoning, whatever they are, can be de-
ﬁned a priori. Similar conceptions are even more clearly seen in the
rationalist and Kantian traditions. What is striking is that, if the distinctly
realist account of scientiﬁc knowledge is sound, then the most basic
principles of inductive inference lack any a priori justiﬁcation. That this is so
can be seen by reﬂecting on what the scientiﬁc realist must say about the
history of the scientiﬁc method.

According to the distinctly realist account of scientiﬁc knowledge, the
reliability of the scientiﬁc method as a guide to (approximate) truth is to
be explained only on the assumption that the theoretical tradition that
defines our actual methodological principles reﬂects an approximately
true account of the natural world. On that assumption, scientiﬁc meth-
ods will lead to successively more accurate theories and to successively
more reliable methodological practices. 12 If we now inquire how the
theoretical tradition came to embody sufﬁciently accurate theories in the
ﬁrst place, the scientiﬁc realist cannot appeal to the scientiﬁc method as
an explanation, because that method is epistemically reliable only on the
assumption that the relevant theoretical tradition already embodies a suf-
ficiently good approximation to the truth. The realist, as I have por-
trayed here, must hold that the reliability of the scientiﬁc method rests
upon the logically, epistemically, and historically contingent emergence

of suitably approximately true theories. Like the causal theorist of per-
ception or other “naturalistic” epistemologists, the scientiﬁc realist must
deny that the most basic principles of inductive inference or justiﬁcation
are defeasible a priori. In a word, the scientiﬁc realist must see episte-

mology as an empirical science. 13

Closely analogous consequences follow from the epistemic access
account of reference when it is applied in the light of scientiﬁc realism.
The question of whether two tokens of a theoretical term are coreferen-
tial is, for example, a purely empirical question that cannot be resolved
by conceptual analysis. If we think of the “meaning” of a theoretical term
as comprising those features of its use in virtue of which it has whatever
referent it in fact has, then meanings of theoretical terms are not given by
a priori stipulations or social conventions. It is logically, historically, and
epistemically contingent matter which features of the use of a given term
constitute its meaning, in the sense of meaning relevant to referential
semantics. There simply are not going to be any important analytic or
conceptual truths about any scientiﬁcally interesting subject matter.

If these controversial consequences of a thoroughgoing realist conception
of scientiﬁc knowledge are sound, then it would be hard to escape a
still more controversial conclusion: philosophy is itself a sort of empirical
science. It may well be a normative science—epistemology, for example,
may aim at understanding which belief-regulating mechanisms are rela-
tible guides to the truth—but it will be no less an empirical science for
being normative in this way.

ISSUES OF PHILOSOPHICAL METHOD

In this section, I shall discuss two issues of philosophical methodology
raised by the arguments for scientiﬁc realism described in the section
“Defending Scientiﬁc Realism.” First, I shall discuss, at some length, an
important challenge raised by Arthur Fine against the basic strategy of
those arguments. Then, I shall discuss, somewhat more brieﬂy, certain
questions about the ways in which evidence from the history of science
bears upon the arguments in question.

THE CHALLENGE TO ABDUCTION

Fine raises a number of interesting objections to the arguments for scientiﬁc
realism outlined earlier. 14 Of these objections, one is particularly
striking because it challenges not the details of the argument for realism,
but its basic philosophical strategy.
Fine's objection is extremely simple and elegant. The proposed defense of realism proceeds by an abductive argument: we are encouraged to accept realism because, realists maintain, realism provides the best explanation of the instrumental reliability of scientific methodology. Suppose for the sake of argument that this is true. We are still not justified in believing that realism is true. This is so because the issue between realists and empiricists is precisely over the question of whether or not abduction is an epistemologically justifiable inferential principle, especially when, as in the present case, the explanation postulated involves the operation of unobservable mechanisms. After all, if abductive inference is justifiable, then there is no epistemological problem about the theoretical postulation of unobservables in the first place. It is precisely abductive inference to unobservables that the standard empiricist arguments call into question. Thus, the abductive defense of realism we are considering is viciously circular.

It is reasonable to think of Fine's objection in the light of the previous discussion of the "no miracles" argument for realism (discussed in the section "Antirealism in the Empiricist Tradition"). Against the "no miracles" argument I argued that, even if realism provides the best explanation for the predictive reliability of scientific theories, there remains for the realist the problem that this fact does not constitute a rebuttal to the very powerful epistemological considerations that form the basis for empiricist antirealism. Fine, in effect, presents a generalized version of this response to the "no miracles" argument. In the first place, Fine's version of the response in question applies not only to the "no miracles" argument but to any argument for realism that addsuce realism as (a component of) the best explanation for some natural phenomenon. In particular, Fine's objection applies to the argument for realism offered in the section on "Defending Scientific Realism." Suppose now that scientific realism provides the best explanation for the reliability (not just of individual theories but) of the methodology of science as a whole. This fact by itself does not constitute a rebuttal to the epistemological principles upon which the empiricist criticism of realism rests.

Moreover, Fine's objection diagnoses not only a weakness in such arguments for realism but a circularity as well. The issue of scientific realism is—at least insofar as the dispute between realists and empiricists is concerned—a debate over the legitimacy of inductive inference to the best explanation, at least in those cases in which the explanation in question postulates unobservable entities. Arguments for realism of the sort which Fine criticizes employ just this sort of inference, and, thus, simply beg the question between realists and empiricist antirealists.

Several things must be said in reply to Fine's subtle and elegant objection. In the first place, Fine's entirely correct insistence that the issue between empiricists and realists is over the legitimacy of abductive inference is a double-edged sword. While it facilitates the identification of a sort of circularity in arguments for realism, it also highlights the epistemological oddity of consistent empiricism. The rejection of abduction or inference to the best explanation would place quite remarkable strictures on intellectual inquiry. In particular, it is by no means clear that students of the sciences, whether philosophers or historians, would have any methodology left if abduction were abandoned. If the fact that a theory provides the best available explanation for some important phenomenon is not a justification for believing that the theory is at least approximately true, then it is hard to see how intellectual inquiry could proceed. Of course, the antirealist might accept abductive inferences whenever their conclusions do not postulate unobservables, while rejecting such inferences to "theoretical" conclusions. In this case, however, the burden of proof would no longer lie exclusively on the realist's side: the antirealist must justify the proposed limitation on an otherwise legitimate principle of inductive inference.

This difficulty for the antirealist is exacerbated when one considers the issue of inductive inference in science itself. It must be remembered that empiricist philosophers of science do not intend to be fully skeptical: it is no part of standard empiricist philosophy of science to reject all non-deductive inferences. Instead, a selective skepticism is intended: (some) inductive generalizations about observables are to be empiristemologically legitimate, while inferences to conclusions about unobservables are to be rejected. As Hanson, Kuhn, and others have shown, the actual methods of science are profoundly theory-dependent. I have emphasized in previously cited publications that this theory-dependence extends to the methods scientists employ in making inductive generalizations about observable phenomena. Both the choice of generalizations that are seriously advanced and the assessment of the evidence for or against them rest upon theoretical inferences that manifest, or depend upon, the sort of abductive inferences to which the empiricist objects. In the terminology of recent empiricism, both the assessment of "projectability" of predicates, and the assessment of "degree of confirmation" of generalizations about observables depend in practice, upon inferences about "theoretical entities." Of course, acknowledging these facts about scientific practice would not commit the empiricist to agreeing that realism provides the best explanation for the instrumental reliability of scientific methodology nor, as Fine insists, would agreeing to that proposition commit the empiricist to holding that there is any reason to believe that realism is true. Nevertheless it certainly seems that, unless—as is very unlikely—the...
apparent theory-dependence of inductive inference about observables is really only apparent, the empiricist who rejects abductive inferences regarding unobservables must hold that even the inductive inferences scientists make about observables are unjustified.

It might seem that there is an easy way out of this last difficulty for the empiricist. Suppose that inductive inferences about observables in science are genuinely theory-dependent and that, therefore, the (necessarily theoretical) justifications, which scientists would ordinarily offer in defense of their inductive inferences about observables, themselves rest on theoretical claims that are without justification. Still, a philosopher might propose a sort of inductive justification of theory-dependent scientific inductions. Let the inductive procedures of science be as theory-dependent as you like, and let the justifications offered for individual inferences by scientists be as faulty as the empiricist claims. The fact remains that the (theory-dependent) methodology of science gives evidence of being instrumentally reliable. Let that constitute the justification for the inferences which scientists make. The thesis that the methodology of science is instrumentally reliable is, after all, a thesis about observable phenomena. It is, moreover, well confirmed by the observational evidence presented by the recent history of science and technology. Since no abductive inference objectionable from an empiricist perspective is required to establish the generalization that scientific methodology is instrumentally reliable, we may accept this generalization and then apply it to justify the acceptance of the inductive generalizations scientists arrive at by employing the scientific method. Even though the theoretical reasoning that underlies inductive inferences about observables may not be justificatory, a second-order induction about the instrumental reliability of such reasoning might still afford a justification for that part of scientific practice that is supposed to be immune from the empiricist's selective skepticism.

It is very doubtful that this application of the inductive justification of induction can help the empiricist we are considering to avoid the conclusion that inductive generalizations in science about observables are unjustified. The hypothesis that scientific methodology is instrumentally reliable (henceforth, the "reliability hypothesis") is itself an inductive generalization about observable phenomena. If, as I have suggested earlier, the confirmation or disconfirmation of such generalizations typically presupposes theoretical considerations of the sort our empiricist cannot accept, then we should expect that this might be true of the confirmation of the reliability hypothesis itself. If this is so, then the effort to circumvent the empiricist's conclusion that inductive generalizations in science are unjustified because they are theory-dependent, by appealing to the confirmation of the reliability hypothesis, will have failed. The reliability hypothesis will itself be unjustified by the standards of the empiricist we are considering.

I earlier suggested that theory-dependent considerations enter into the confirmation or disconfirmation of inductive generalizations in science in two related ways. In the first, theoretical considerations are decisive in solving what Goodman calls the problem of "projectability." Given any finite body of observational data, there are infinitely many different generalizations about observables that are logically compatible with them. Theoretical considerations dictate the choice of a relatively small, finite number of these generalizations as "projectable," that is, as worthy of serious scientific and experimental consideration. Moreover, when the experimental evidence for or against such projectively appropriate generalizations is assessed, theoretical considerations are crucial in determining the degree of confirmation or disconfirmation which those generalizations receive, given any particular body of observational evidence. If this is so, then we might expect to be able to discern the effects of both sorts of theory-dependent judgments in the special case of the confirmation of the reliability hypothesis.

Consider first the issue of the degree of confirmation of the reliability hypothesis. The hypothesis that the scientific method is instrumentally reliable asserts that that method tends to produce acceptance of instrumentally reliable theories. The reliability of a theory in turn is a matter not only of its past predictive successes but also of its future predictive success. Now the observational evidence that supports the reliability hypothesis consists of the past and present predictive successes of (many of) the theories whose acceptance has been dictated by the scientific method. For these past successes to count as evidence for the instrumental reliability of the scientific method, they surely must be understood first as counting as evidence for the future (approximate) instrumental reliability of most of the theories in question.

The conviction that the methods of science are instrumentally reliable turns on the belief that those methods have led us to accept theories that tended themselves to be instrumentally reliable. We can make this latter judgment only if we take the past predictive successes of the relevant theories as evidence for their future instrumental reliability; that is, only if we are already prepared to make the ordinary scientific judgment that past predictive successes of the sort actually available warrant our belief in the inductive generalizations about observables embodied in the theories in question. But this is just the sort of theory-dependent judgment which the reliability hypothesis is supposed to justify. If the ordinary scientific justifications for assigning the generalizations in question a high
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degree of confirmation are inadequate because they depend upon abductions to theoretical explanations, then the second-order inductive justification of scientists' inductions by appeal to the reliability hypothesis fails to help. The decision to assign the reliability hypothesis a high degree of confirmation on the available evidence rests upon the very theory-dependent judgments about the degree of confirmation of ordinary scientific theories which the empiricist we are considering cannot accept as justificatory.

We may also see how theoretical considerations regarding "projectability" are involved in the confirmation of the reliability hypothesis. When philosophers of whatever persuasion assert that the methods of science are instrumentally (or theoretically, for that matter) reliable, their claim is of very little interest if nothing can be said about which methods are the methods in question. Indeed, without at least a preliminary specification of the methods in question, it would be difficult to have any evidence whatsoever for the reliability thesis. Moreover, it will not do to countenance as 'methods of science' just any regularities that may be discerned in the practice of scientists. If the reliability thesis is to be correctly formulated, one must identify those features of scientific practice that contribute to its instrumental reliability. This is a nontrivial intellectual problem, as one may see by examining the various different attempts—behaviorist, reductionist, and functionalist—to explain what a scientific foundation for psychology would look like.

Insofar as the confirmation of the reliability hypothesis is concerned, the issue is not so much a matter of how easy or difficult it is to identify the reliability-making features of scientific practice, but rather over what sorts of considerations would have to go into a justification for a proposed identification of those features. Recall that we are considering the options open to the empiricist who rejects abductive inferences as nonjustificatory but who agrees that the actual inductive methods of science (the instrumentally reliable methods) are theory-dependent and rest in practice upon abductive inferences. It is reasonable to ask of this empiricist—as it would be reasonable to ask of any other philosopher who had identified the same theory-dependent methods as the methods of sciences—what justification can be offered for the identification of these particular methods as the reliability-making features of scientific practice.

The problem of providing a justification for a particular proposed identification of such features represents, as regards the formulation of the reliability hypothesis, a special case of the problem of projectability. This may be seen easily if we employ a variant of the empiricists' favorite argument that theory choice is underdetermined by observational data.

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Suppose that you believe that past scientific practice has certain reliability-making general features which should form the basis for a suitable formulation of the reliability hypothesis. There have been, to date, only finitely many methodological judgments in the whole history of science. Even if you know which of these judgments have contributed to the reliability of past scientific practice, there will still be infinitely many different methodological judgments—infinitely many different sets of principles for theory choice, experimental design, data assessment, and so forth—which would have dictated the conclusions of those finitely many past methodological judgments. The choice of any one of these infinitely many methodologies represents a particular solution to the problem of projectability for the investigator interested in finding an appropriate formulation of the reliability hypothesis. Alternative choices yield different versions of the reliability hypothesis and represent different estimates of what the reliability-making general features of past scientific practice have been.

If what I have suggested earlier is true, then the solution to this particular case, of the problem of projectability, might be expected to depend upon theoretical considerations. Indeed, this proves to be the case. Remember that the empiricist we are considering accepts the ordinary theory-dependent methods of the working scientist as the reliability-making features of scientific practice. Let us consider an illustrative example of such methods. It is by now widely acknowledged that sound scientific methodology dictates that "measurement procedures" for physical magnitudes should be revised in light of new theoretical "discoveries." (I use quotation marks to indicate that the empiricist need not take the notions of measurement or theoretical discovery at face value. What is important is that the application of this principle in practice has a significant effect upon the inductive generalizations about observables which scientists accept.) Let P be the methodological principle that says that one should follow the dictates of the best confirmed theory in (re)designing measurement procedures. What justifies us in taking P to be one of the reliability-making features of scientific practice? Why should we not subsume the finitely many cases to date of successful applications of this principle under some other quite different maxim with which they are all consistent?

Recalling that an appeal to the reliability hypothesis is inappropriate here, since what is at issue is the formulation and confirmation of that hypothesis, it is hard to see how our reasons for accepting P as reliability-making could be other than a summary of the ordinary reasons scientists have for accepting various applications of P. But these are theory-dependent reasons—roughly, they amount to the idea that the best theories
represent results of the best (abductive) inferences regarding the unobservable magnitudes in question, and that, therefore, these theories are likely to provide approximately true accounts of how to measure those magnitudes. But, theoretical reasons of this sort are just those which the empiricist considers nonjustificatory. Worse yet, if we are to accept \( P \), and not just some particular applications of \( P \), as reliability-making it would seem that our justification for accepting \( P \) must involve not just the scientists' theoretical reasons for particular applications of \( P \) but the scientific realist's reasons for thinking \( P \) generally reliable. If the empiricist forgoes appeals to the abductive inferences of ordinary scientific practice on the grounds that such inferences are nonjustificatory, then it is hard to see how she can make scientifically sound judgments about which methods are scientific or about how even to formulate the reliability hypothesis.

It is worth noting that the empiricist we are considering gets into this particular difficulty largely because she accepts the results of recent philosophical and historical scholarship, which strongly suggest that the real methods of science are theory-dependent and rest in practice on abductive inferences of the sort unacceptable to empiricists. What appears to be true is that the consistent empiricist cannot hold both that the inductive methods of scientists are justified, insofar as generalizations about observables are concerned and, at the same time, accept the best recent work on the question of what those methods actually are.

I conclude that the empiricist who rejects abductive inferences is probably unable to avoid, in any plausible way, the conclusion that the inductive inferences that scientists make about observables are unjustified. Nevertheless, even if this is so, Fine's criticism of abductive arguments for realism still has force. If what is at issue is the legitimacy of abductive inferences to theoretical explanations in general, then there is a kind of circularity in the appeal to a particular abduction of this sort in the defense of scientific realism. I suggested earlier in this paper that while standard rebuttals to empiricist antirealism provide some reason to believe that scientific realism is true, these rebuttals fail to respond to the strong epistemological challenge that empiricist antirealism offers. Should we take the circularity which Fine discerns to indicate that the same is true for the abductive argument for scientific realism as a component in the best explanation for the instrumental reliability of scientific method? I want to argue that the answer should be no.

If abduction were prima facie suspect, in the way that palm reading or horoscope casting now are, then surely it would be inappropriate to appeal to some particular abductive inference in defense of abductive inference in general. Abduction is, however, prima facie legitimate; it is seen as suspect only in the light of certain distinctly empiricist epistemological considerations. To assess the import of the circularity of appealing to abduction in replying to empiricist antirealism, we must examine more closely the relation between the particular abductive inferences in question, and the empiricist's arguments against realism.

I suggest that an assessment of the import of the circularity in question should focus not on the legitimacy of the realist's abductive inference considered in isolation, but rather on the relative merits of the overall accounts of scientific knowledge which the empiricist and the realist defend. Such an assessment strategy is familiar from many areas of intellectual inquiry, scientific and scholarly. Defenders of rival positions often reach their distinctive conclusions via forms of inference which their rivals think unjustified. The pair-wise theory-neutral procedure for addressing such disputes typically consists in an assessment of the overall adequacy of the theories put forward, rather than in an assessment of the particular controversial inference forms considered in isolation.

If we consider the present dispute in this light, then there are two especially important considerations. First, the empiricist's objection to abductive inferences (at least to those that yield conclusions about unobservable phenomena) rests upon the powerful and sophisticated epistemological argument rehearsed in my discussion of empiricist antirealism. That argument depends upon the evidential indistinguishability thesis. Moreover, the evidential indistinguishability thesis itself is put forward by empiricists (tacitly or explicitly) on the understanding that it captures the truth reflected in the doctrine of knowledge empiricism: the doctrine that all factual knowledge must be grounded in observation. If either knowledge empiricism is basically false, or the indistinguishability thesis represents a seriously misleading interpretation of it, then the empiricist's argument against abduction to theoretical explanation fails.

Second, the empiricist aims at a selectively skeptical account of scientific knowledge: knowledge of unobservables is impossible, but inductive generalizations about observables are sometimes epistemologically legitimate. It turns out, however, that the empiricist's commitment to knowledge empiricism, together with her adoption of the evidential indistinguishability thesis as an interpretation of it, threaten to dictate the unwelcome and implausible conclusion that even inductive inferences regarding observables are always unjustified.

The rebuttals to empiricist antirealism discussed earlier strengthen the case for realism as an account of the structure of scientific knowledge, yet they provide no direct argument either against knowledge empiricism or against the evidential indistinguishability thesis as an interpretation of it. The situation of the abductive argument for scientific realism sketched
previously is quite different. If we accept the abductive inference to a distinctly realistic account of scientific methodology, then we can see why the evidential indistinguishability thesis is false. Moreover, we can see that the distinctly realistic conception of scientific methodology retains the central core of the doctrine of knowledge empiricism: all factual knowledge does depend upon observation; there are no a priori factual statements immune from empirical refutation.

I think it is fair to say that, given the difficulties that plague empiricist antirealism in the philosophy of science, the only philosophically cogent reason for rejecting scientific realism in favor of instrumentalism, or some other variant of empiricism, lies in the conviction that only from an empiricist perspective can one be faithful to the basic idea that factual knowledge must be experimental knowledge, that is, to the grain of truth in knowledge empiricism. The abductive argument for scientific realism that we are considering is best thought of as a component of an alternative realistic conception of scientific knowledge which preserves the empiricist insight that factual knowledge rests on the senses without the cost of an inadequate and potentially wholly skeptical treatment of scientific inquiry.

I suggested (in the section "Scientific Realism and Metaphilosophy") that the crucial feature of this alternative conception of knowledge is its naturalism. In particular, the special relation of the senses to knowledge is seen, in this conception, as resting on logically contingent facts about the role of the senses in the reliable production or regulation of belief. Here, an analogy between the naturalistic defense of scientific realism against empiricist antirealism and the naturalistic defense of knowledge of external objects against empiricist phenomenalism is revealing. The phenomenalist rejects realism about (observable) external objects, relying on an application of the sense-datum version of the evidential indistinguishability thesis. The indistinguishability thesis itself is understood as the appropriate interpretation of the fundamental truth embodied in the doctrine of knowledge empiricism. The causal theorist of knowledge does not reject the basic doctrine of the epistemic primacy of the senses but, instead, insists that the truth of that doctrine, insofar as it concerns perceptual knowledge, is really a reflection of the logically contingent fact that the senses are causally reliable detectors of external objects. Sensory experience provides reliable evidence for propositions only when it arises from suitable causal connections to the subject matter of the propositions in question. The sense-datum form of the indistinguishability thesis is, therefore, false and inadequately expresses the fundamental truth of knowledge empiricism.

The causal theorist’s critique of phenomenalism rests upon what her empiricist opponent would characterize as an illegitimate abductive inference to external objects as the explanation for facts about sensations. The causal theorist’s position does not, however, stand or fall on the strength of that abduction taken in isolation. Instead, the alternative empiricist and naturalist conceptions of knowledge and, especially, of the epistemic role of the senses, must be evaluated as rival philosophical theories. The very grave difficulties that phenomenalism faces in explaining ordinary perceptual knowledge strongly suggest that the naturalist’s causal theory of perceptual knowledge is preferable.

The situation with respect to the dispute between the empiricist anti-real and the scientific realist who subscribes to the argument sketched earlier (in section "Defending Scientific Realism") is exactly analogous. The antirealist’s position rests upon an application of the indistinguishability thesis, which in turn is offered as an explication of knowledge empiricism. The scientific realist, like the causal theorist of perception, accepts the insight of knowledge empiricism while denying that the indistinguishability thesis captures that insight. The causal theorist maintains that the truth of knowledge empiricism, insofar as it applies to perceptual knowledge, is a reflection of a logically contingent fact about the reliability of the senses as detectors. Analogously, the scientific realist maintains that the truth of knowledge empiricism, insofar as experimental knowledge in the sciences is concerned, is a reflection not only of the logically contingent reliability of the senses as detectors but also of the logically and historically contingent emergence of a theoretical tradition relevantly approximately true enough to make theory-dependent experimental practice a reliable mechanism for belief regulation. Like the causal theorist’s rebuttal to phenomenalism, the scientific realist’s rebuttal to empiricist antirealism rests upon what her opponent would regard as an illegitimate abductive inference. In this case, like the previous one, however, the scientific realist’s position does not stand or fall on the strength of that abduction considered in isolation. Rather, what is to be assessed are the relative merits of empiricism epistemology and the emerging naturalistic epistemology of which the realist’s conception of scientific knowledge is one of the more distinctive and controversial parts.

In this regard, it is worth remarking that the plausibility of knowledge empiricism has, no doubt, always rested upon two considerations: a recognition of the central causal role of the senses in information gathering, and a recognition of the success of experimental science. It is doubtful if consistent empiricism can recognize either of these phenomena. If this proves to be the case, then the alternative realistic and naturalistic conception of the epistemic role of the senses must surely capture what truth there is in knowledge empiricism.
that is plausible in the light of the current total science. Moreover, we take such a hypothesis to represent the inductive generalization about observables that corresponds to the observational consequences derivable from the hypothesis itself, together with the theories that constitute the existing total science. Once such a hypothesis has been accepted, we countenance further expansion and modification of the inductive generalizations about observables that it warrants as our total science itself changes and develops. Thus we are able to identify as projectable an extraordinary variety of patterns among observables representing empirical generalizations of great power, scope, and precision.

In addition to the methods for identifying inductively appropriate empirical generalizations, the methods employed in mature sciences for the experimental and observational testing of such generalizations—methods for the design of experiments and of instrumentation, for the establishment of appropriate controls, and for the assessment of degrees of confirmation—are also profoundly theory-dependent. It is the instrumental reliability of all of these various theory-dependent methods—methods whose characteristic reliability is displayed typically only in mature (and, often, relatively recent) science—for which, according to the argument we are considering, the only plausible explanation rests upon a realistic conception of scientific knowledge. What is claimed is that when, in the historical development of any particular science, its theory-dependent methodological practices come to display the sort of intricacy and instrumental reliability characteristic, say, of modern physical or chemical practice, only the realistic account of scientific knowledge described earlier in this essay will provide an adequate explanation of that reliability. No claim is made that the more limited inductive success of earlier scientific practice must always be explained in the same way. Nor is it claimed, even in the case of inquiry in mature sciences, that the approximate theoretical knowledge upon which the instrumental reliability of methodology depends must represent fundamental knowledge, or knowledge of the ultimate essences of the phenomena in question. The abductive argument for realism does not require that the approximate theoretical knowledge which scientists possess must embody correct answers to those questions that scientists or philosophers might consider most basic or fundamental. All that is claimed is that the instrumental reliability of the methodology of mature sciences depends upon the development of a theoretical tradition that embodies approximate knowledge of unobservable as well as observable phenomena. It is this claim, after all, which the empiricist denies.

Similarly, it is not a thesis of the version of scientific realism defended here that there is one completely true theory which would be the "asym-
totic limit" of scientific theorizing if science were pursued long enough. As antireductionist materialists have long insisted, there is no reason to believe that true theories are all special cases of some most fundamental theory, even if materialism is true. Different levels of description or of functional organization characterize different, perfectly real, natural phenomena. Even if one understands by a "theory" something like a "total science"—a set of sentences that may embody descriptions of phenomena at various levels of functional or structural organization—it does not follow from the sort of realism defended here that the true theory would be the asymptotic limit of scientific inquiry.

In the first place, even for theories that describe phenomena at the same level of organization, and even for theories that are in some sense complete in their description of the relevant phenomena, it does not follow from a realistic conception of science that there must be a single true theory. In particular, it does not follow that there must be a single true ontology for the most basic level of physical theory (assuming that there is such a level). What is entailed is that if there are two entirely true and suitably complete theories of basic physical phenomena, they must be ontologically equivalent in the sense that the entities, powers, properties, states, and so forth, which form the ontology of any one, must themselves be causally realized by the entities that form the ontology of the other. On the standard positivist analysis of ontological equivalence, this would entail that the two theories must be syntactically reducible to each other and, thus, that they be linguistic variants of the same theory. Such a positivist analysis of ontological equivalence is mistaken and is, in fact, simply a reflection of an antirealist conception of causal relations. On a realist conception of ontological equivalence, no such conclusion follows, so that it is perfectly conceivable that scientific research might converge to one of two such theories, while the ontological conceptions central to the second might be quite literally inexpressible, given the descriptive resources of the first theoretical tradition.

In the second place, it is no part of the realistic conception of science defended here that any such convergence to (even one version of) the exact truth need occur even in the ideal limit of actual scientific practice. There are any number of ways in which our understanding might be forever “bounded away” from the exact truth about some (or even all) aspects of nature.

Finally, the evidential connection is quite subtle between, on the one hand, the historical evidence for continuity of theoretical semantics and for continuity of methods in mature sciences, and, on the other, the thesis of scientific realism. Because scientific realists hold that progress in mature sciences is a reflection of theoretical as well as instrumental prog-

ress and, indeed, that instrumental progress often depends upon theoretical progress, it is essential to the empirical case for realism that historical evidence support the claim that there is the relevant sort of semantic and methodological continuity in the history of mature sciences. For example, it must be possible to see greater continuity and commensurability across "scientific revolutions" than Kuhn acknowledges. When the history of science provides evidence of semantic or methodological continuity in mature sciences, the realist will typically hold that a realist conception of scientific knowledge—together with the appropriate sort of referential continuity for theoretical terms—provides the best explanation for the historical evidence in question.

But it is not part of the strategy for the defense of realism described here to suggest that any substantial prima facie evidence for scientific realism is provided merely by consideration of historical evidence of this sort. The two chief rivals of scientific realism, empiricism and constructivism, are each capable of providing plausible explanations for the apparent semantic and methodological continuity in the history of well-developed and mature sciences. Indeed, they offer variations on the same explanation: the continuity in question is a manifestation of linguistic, conceptual, and methodological conventions (see the earlier section on the similarities between empiricism and constructivism). If we focus our attention solely on the historical evidence for semantic and methodological continuity in the history of science, there seems little reason to prefer the realist’s explanation to that of the constructivist or the empiricist.

According to the realist position discussed here, the choice between the competing explanations for apparent semantic and methodological continuity in mature sciences must rest upon other considerations. Neither the empiricist nor the constructivist can explain the most striking feature of the recent history of science, that is, the instrumental reliability of its methods. Only scientific realism provides the resources for explaining this crucial historical phenomenon. It is for this reason that realism is to be preferred to rival accounts of scientific knowledge, and for this reason that the realist account of semantic and methodological continuity is to be preferred to the alternative account presented in various forms by empiricists and constructivists.

The positive evidence for scientific realism thus rests primarily on features of scientific practice that would be discernible even if one limited one’s examination to very recent science. According to the realist, realism provides the only acceptable explanation for the current instrumental reliability of scientific methodology in mature sciences. Realism does, however, entail interesting conclusions about historical development within mature sciences—that is, within those sciences in which theoreti-
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cal considerations contribute significantly to a high level of instrumental reliability of method. For many sciences, especially the physical sciences, the period of maturity, in this sense, begins long before the recent past. Thus, historical studies of such sciences, of, for example, the extent of semantic and methodological continuity in the history of those sciences, are evidently relevant to the issue of realism. Insofar as a realist perspective proves fruitful in understanding the history of mature sciences, that would provide further evidence for realism; the primary role of historical studies in this area, however, is to subject the claims of realists to possible disconfirmation by historical evidence rather than to provide new kinds of positive evidence favoring realism over its rivals.

There is one important respect in which consideration of the implications of scientific realism regarding the nonrecent history of science does provide additional justification for the acceptance of realism, but here the connection with the assessment of the historical evidence for realism is indirect. What I have in mind is this: it is by reflection on the historical implications of a realist conception of scientific knowledge that we are able to see (a) that the reliability (instrumental or theoretical) of the scientific method rests upon the logically and historically contingent emergence of a suitably approximately true tradition, and (b) that judgments of the plausibility of theories relative to such a tradition are evidential. It is these doctrines, in turn, which enable us to see that the evidential indistinguishability thesis is false, that a theory-dependent methodology need not be merely a construction procedure, and that a realist conception of the epistemology of science can be integrated into, and can serve to justify, a broader naturalistic conception of epistemology and of philosophy itself. It is upon these last two considerations that the case for scientific realism ultimately rests, and it is in its contribution to a naturalistic conception of philosophy that scientific realism makes its greatest contribution to an understanding of the nature of knowledge.

NOTES

3. N. R. Hanson, Patterns of Discovery (Cambridge: Cambridge University Press, 1958).
5. Kuhn, The Structure of Scientific Revolutions.

12. This is Kuhn's term; see Kuhn, The Structure of Scientific Revolutions.
13. Ibid.
24. See Boyd, "Metaphor and Theory Change"; "Materialism without Reduc-
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 I, 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