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ON THE CURRENT STATUS OF THE ISSUE OF SCIENTIFIC REALISM[1]

1. INTRODUCTION

The aim of the present 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 which avoids the inadequacies in question.

2. SCIENTIFIC REALISM DEFINED

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

(i) "Theoretical terms" in scientific theories (i.e., non-observational terms) should be thought of as putatively referring expressions; scientific theories should be interpreted "realistically".

(ii) 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.

(iii) The historical progress of mature sciences is largely a matter of successively 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.

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

Critics of realism in the empiricist tradition typically deny (i) and (ii), and qualify their acceptance of (iii) so as to avoid commitment to the

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possibility of theoretical knowledge (but van Fraassen, 1980, accepts (i)). Anti-realists in the constructivist tradition, like Kuhn (1970) deny (iv); they may well affirm (i)–(iii) on the understanding that the "reality" which scientific theories describe is somehow a social and intellectual "construct". As Kuhn (1970) and Hanson (1958) both argue, a constructivist perspective limits, however, the scope of application of (iii), 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". Smart's version of scientific realism (Smart 1963) departs from the typical conception in that he rejects (ii), 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 non-evidential, 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 (i)–(iv). The key anti-realist arguments, the standard rebuttals to them in the literature, and certain weaknesses in these rebuttals are summarized in chart form in Table I.

3. ANTI-REALISM IN THE EMPIRICIST TRADITION

There is a single, simple, and very powerful epistemological argument which 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 which 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 disconfirmed by any possible observational evidence. Therefore no scientific evidence can bear on the question of which of these theories provides the correct account of unobservable phenomena; at best, it might be possible to confirm or dis-

TABLE I

The Basic Anti-Realist Arguments, the Standard Rebuttals, and their Weaknesses.

Anti-Realist Argument	*Standard Rebuttal*	*Weakness*
1. Empiricist Argument: Empirically Equivalent theories are evidentially indistinguishable, so knowledge cannot extend to "unobservables".	1. a. No sharp distinction between "observables" and unobservables.	1. a. (i) Sharp distinction can be drawn in a well-motivated way. (ii) In any event, distinction need not be sharp.
	1. b. Empiricist Argument ignores the role of "auxiliary hypotheses" in assessing empirical equivalence.	1. b. Empiricist Argument can be reformulated to apply to "total sciences".
	1. c. The "no miracles" argument: If scientific theories weren't (approximately) true, it would be miraculous that they yield such accurate observational predictions.	1. c. Does not address the crucial epistemological claim of the empiricist argument: that since factual knowledge is grounded in experience, it can extend only to observable phenomena.
2. Constructivist Arguments: 2. a. Scientific methodology is so theory-dependent that it is at best a construction procedure, not a discovery procedure.	2. a. Pair-wise theory-neutrality of method: for any two rival theories, there are experimental tests based on a method legitimized by both theories.	2. a. Does not address the epistemological point that theory-dependent methodology must be a construction procedure.
2. b. Consecutive "paradigms" in the history of science are not logically commensurable in the way they would be if they embodied theories about a paradigm-independent world. (Kuhn, 1970).	2. b. It is possible to give an account of continuity or reference for theoretical terms which allows for commensurability of paradigms.	2. b. If the anti-realist *epistemological* argument (2. a.) is sound, then such continuity of reference is itself a construct, or at best a matter of continuity of reference to constructs, so that the realist's conception of scientific knowledge of theory-independent reality is still not vindicated.

confirm 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 knowledge of unobservable phenomena is thus 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 "simplest" "model" for "pragmatic" reasons, but if evidence in science is experimental 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 Table I, 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 1971) that factual knowledge must always be grounded in experiences; that there is no *a priori* factual knowledge. (As I shall argue in section 6, the evidential indistinguishability thesis is the wrong formulation of the important epistemological truth in that doctrine; still, it represents the way in which empiricist philosophers of science – and most other empiricists for that matter – have understood the fundamental doctrine of empiricist epistemology.)

Let us turn now to the standard rebuttals to the anti-realist application of the indistinguishability thesis. Perhaps the most commonplace rebuttal 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 anti-realist argument therefore rests upon an arbitrary distinction (see, for example, Maxwell, 1962).[2] 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 anti-realism. *If* scientific realism has somehow been established, then it may well be evident that the distinction in question is epistemologically arbitrary: *if* we are able to confirm theories of, say, electrons, 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, on the other hand, it is scientific realism which 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 non-arbitrary. Suppose that there are entities which represent borderline cases of observability and suppose that there are cases in which it's 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 anti-realist 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 disconfirmation of observational (that is, sense datum) predictions which are *deduced* from the theory. Quite so, but the fact remains that some experiences 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 empiricist 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 inter-subjective 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 neither shows 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 my provide us with reason to think that there must be *something* wrong with the phenomenalist's argument against scientific realism, but they 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 which we are considering displays a weakness which is common to all of the rebuttals to anti-empiricist arguments described in Table I. 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 anti-realist 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, 1962, that entities which 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 can be previously established by procedures which do not beg the question between empiricists and scientific realists. Roughly at least, the latter proposal can be put this way: Let O_1 be the class of entities which are observable to the typical unaided senses; for any n, let O_{n+1} be the class of entities which are detectable by procedures whose legitimacy can be established on the basis of theories which 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 which 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 unobservability is sufficient to sustain a significant anti-realist 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" (see Harman 1965) 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 are unproblematical 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 those equations which 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 $O_n = O_{n+1}$ 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 which 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, electrical and magnetic fields, etc.) fall into the category of entities for which legitimization would be question-begging. Therefore the central claims of anti-realist 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, e.g., light microscopes in scientific observations.

We may apparently conclude the following about the rebuttal to empiricist anti-realist arguments which turns on the claim that the distinction between observable entities and unobservables 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 non-arbitrary. 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 which might make it reasonable to suppose that there is something problematical 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 the 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, neutrinos, etc. Surely this shows that the sort 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 in Table I. 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 which make this argument plausible are typically those in which predictions quite different from the ones which were involved in the initial confirmation of a theory – and especially predictions which are arrived at by calculations which 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. (This may be the argument which Putnam 1978 attributes to Boyd, forthcoming (b).) Cases in which what is predicted are the 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 anti-realism has considerable force (indeed, it is probably the argument which reconstructs the reason why most scientific realists are realists). But it suffers from the same defect which 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) flows 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 anti-realism and it does seem to directly challenge 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 which are appropriate to test are those which 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 consequence 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 premisses, the theories (so expanded) are no longer empirically equivalent.

There is almost no doubt that considerations of this sort rebut any verificationist 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 anti-realistic 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 (see Boyd 1982).

One objection which 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 that they have therefore 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 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 anti-realist conclusion of empiricists, and it certainly seems reasonable to hold that some 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 which 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 (Boyd 1979, 1982, forthcoming (b)), but this argument is not anticipated in the standard rebuttals to empiricist anti-realism.

We 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.

4. CONSTRUCTIVIST ANTI-REALISM

There is a single basic empiricist argument against realism and it is an argument of striking simplicity and power. In the case of constructivist anti-realism the situation is much more complex. In part, at least, this is so because constructivist philosophers of science have typically been led to anti-realist 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 anti-realist conclusions. Nevertheless, it is possible, I believe, to identify the common thread in all of these diverse arguments. Roughly, the constructivist anti-realist 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, what sorts of evidence they require before accepting a theory, ... all of these 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, in order for this sort of theory-dependent methodology to constitute a vehicle for gaining knowledge? The answer, according to the constructivist, is that the world which scientists study must be, in some robust sense, defined or constituted by, or "constructed" from, the theoretical tradition in which the scientific community in question works. If the world which 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 which scientists use are a way of finding out what's 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 which represent the sort of radical "breaks" in tradition at issue are "incommensurable" (this is Kuhn's term, see Kuhn 1970). 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 (Kuhn 1970) this argument incorporates the claim that the semantics of the two consecutive theories changes to such an extent that those terms which 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 anti-realist 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 which is neutral with respect to the theories in question. Thus, so it is argued, 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" which pits one rival theory against another need not be biased, since such an experiment can be conducted on the basis of a methodology which – 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 which 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 anti-realism. Consider first the claim that for any two rival theories there is a methodology for testing them which is neutral with respect to the issues on which they differ ("pair-wise theory neutrality of method" in

Table I). It is generally true that – for theoretical rivalries which 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" (Kuhn 1970). 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 which influence methodological judgments. Nevertheless, pair-wise theory neutrality of method does not provide a reason to reject the anti-realist conclusions of the constructivist.

Remember that what the constructivist argues is that a general methodology which is predicated upon a particular theoretical tradition, and which is theory-determined to its core, cannot be understood as a methodology for discovering features of a world which is not in some significant way defined by that tradition. All that the doctrine of the existence of pair-wise theory neutral methods asserts is that – within the theoretical and methodological tradition in question – there are available experimental procedures which 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 doesn't 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 (see Boyd 1979). 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 pre-revolutionary theory and the post-revolutionary one is governed by a continuously evolving standard of scientific rationality. If these suppositions are true, then much of what Kuhn, for example, had claimed about the history of science will be mistaken: post-revolutionary scientists will (contrary to Kuhn) be building on the theoretical achievements of their pre-revolutionary 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 unrebutted. 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 which 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 anti-realist; 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 anti-realism, it is by no means true that the standard rebuttals to the constructivist 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 pro-realist rebuttals to constructivist anti-realism 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.

5. EMPIRICISM AND CONSTRUCTIVISM

Kuhn (1970) 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 (Kuhn 1970, pp. 101—102; see Boyd 1979 for a discussion). Similarly, Carnap's mature positivism of the early 1950's has much in common with Kuhn's views. In particular, Carnap (1950) 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 (see Schlick, 1932/33 for an anticipation of Carnap's later position). 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 traditon or paradigm. But they agree with logical empiricists in holding that any rational constraint on theory acceptance which is not purely pragmatic and which does not accord with the evidential indistinguishability thesis must be essentially 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 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 1950's do not disagree about the *semantic* commensurability 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 subtler and historically more accurate version of Carnap's (Boyd, 1979, esp. pp. 397–398).

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 which 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 which, whether it is ultimately compatible with empiricism or not, is certainly emphasized by constructivists much more than by empiricists, and which is especially relevant when one considers the role of scientific methodology in producing instrumental knowledge. It was early recognized 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, Goodman (1973) has, following Locke, raised a question which 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 "projectibility" 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 (for further discussion of this issue see Quine 1969; Boyd 1979, 1980, 1982). This question is interesting precisely because, given any finite body of data, there are infinitely many different general theories which are logically consistent with those data (indeed, there will be infinitely many such theories which are pairwise empirically in-equivalent, given the existing total science as a source of auxiliary hypotheses).

What Kuhn and other constructivists insist (correctly, I believe) is that judgments of projectibility and of degrees of confirmation are quite profoundly dependent upon the theories which 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 projectibility 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 (lexicographically) the same theoretical term play an important epistemological role in scientific methodology. This is evident since such commonplaces 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 which 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 (see Boyd 1982, forthcoming (b) for a discussion).

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 which 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 which judgments of univocality for theoretical terms make to the instrumental reliability of scientific methodology.

We have thus identified two questions which pose especially sharp challenges to both empiricist and constructivist conceptions of science: why are theory-dependent standards for assessing projectibility 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 provides the basis for a new and more effective defense of scientific realism.

6. DEFENDING SCIENTIFIC REALISM

I have elsewhere (Boyd 1972, 1973, 1979, 1982, forthcoming (a), forthcoming (b)) offered a defense of scientific realism against empiricist anti-realism 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. 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 anti-realist arguments.

The proposal that scientific realism might be required in order to adequately explain the instrumental reliability of scientific methodology can be motivated by re-examining the principal constructivist argument against scientific realism (2a in Table I). 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's true?" She answers: "The world would have to be largely defined or constituted by the theoretical tradition which 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 (see Boyd 1982).

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 projectibility 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 anti-realism. It remains to see whether it has the weaknesses of more traditonal responses to empiricist anti-realism, but let us first examine its relevance to constructivism.

First, it should be observed that the argument for realism which 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 which (sometimes) give rise to "scientific revolutions" cannot be reflections of a fully paradigm-dependent world: anomalies are defined as observations which 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 realiability 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, moreover, clear that if scientific realism is defended in this way, then the more traditional rebuttals to constructivist anti-realism are rendered fully effective. If the fundamental epistemological thrust of constructivism is mistaken, then (as I indicated in section 4) 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 anti-realism. 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 which 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 traditon 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 which reflects the existing tradition. The evidential indistinguishability thesis is therefore false, and the basic empiricist anti-realist argument is fully rebutted. (See Boyd 1979, 1980, 1982, forthcoming (a), forthcoming (b), for discussion of these points.)

It might seem that this 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 (Boyd 1972, 1973, 1979, 1982, forthcoming (a), forthcoming (b)). Secondly, 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 (see Boyd 1982, forthcoming (a), forthcoming (b)).

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 "projectibility" 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 (Boyd 1982, see also Quine 1969). 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 (Feigl 1956, Kripke 1972, Putnam 1975) 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.

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" (Boyd 1979, 1982, forthcoming (b)). Roughly, a (type) term *t* refers to some entity *e* just in the case where 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 (Boyd 1979, 1982).

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. Which the relevant sorts of causal relations are is to be determined by epistemology, construed as an empirical investigation into the mechanisms of reliable belief regulation (Boyd 1982). It is thus an empirical question, not a "conceptual" one, whether two such tokens are univocal.

Because the epistemic access account of reference can account for 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 (Boyd 1979, 1982), there is every reason to believe that the epistemic access account can explain why the ordinary standards for judging univocality which prevail in science are reliable indicators of actual co-referentiality. 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 the contribution of univocality judgments to the reliability of scientific methodology which is fully in accord with the general realist conception of scientific methodology described here (see Boyd 1982, forthcoming (b)).

Finally, the epistemic access account provides a precise formulation of the crucial realist claim that (perhaps despite changes in law-clusters) there is typically continuity of reference across "scientific revolutions" (Boyd 1979). Indeed, it permits us to integrate cases of what Field (1973) calls "partial denotation" into a general theory of reference and thus to treat cases of "denotational refinement" (Field 1973) as establishing referential continuity in the relevant sense (Boyd 1979).

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 anti-realism which suffers none of the shortcomings of the more traditional rebuttals, while at the same time accommodating the insights which the more traditional rebuttals provide.

7. SCIENTIFIC REALISM AND META-PHILOSOPHY

I have examined traditional rebuttals to anti-realist 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 which 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 which does diagnose the epistemological errors in anti-realist arguments. If the conception of scientific knowledge and language which I have 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 that it is fair to say that scientific realists have had a conception of their dispute with empiricist and (more recently) with constructivist anti-realists 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 anti-realists 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 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 assimilate 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 conflict over preferred mode of speech. (Nagel 1961, p. 151–152).

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 science 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 which empiricists and constructivists cannot share – can instrumental knowledge be explained.

The distinctly realist conception of the methodology of science departs even further 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 defensible *a priori*. Thus, for example, almost all empiricists have thought that "knowledge empiricism" represented an *a priori* truth about knowledge, and that the most basic principles of inductive reasoning, whatever they are, can be defended *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 which defines our actual methodological principles reflects an approximately true account of the natural world. On that assumption, scientific methods will lead to successively more accurate theories and to successively more reliable methodological practices (for a discussion of limitations of this process of successive approximation see Boyd 1982, fn. 4). 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 sufficiently good approximation to the truth. The realist, as I have portrayed her, 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 perception or other "naturalistic" epistemologists, the scientific realist must deny that the most basic principles of inductive inference or justification are defensible *a priori*. In a word, the scientific realist must see epistemology as an *empirical* science (see Boyd 1982 for a discussion of the relation between scientific realism and other recent naturalistic trends in epistemology).

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 co-referential is, for example, a purely empirical question which 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 a 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 just are not going to be any important analytic or conceptual truths about any scientifically interesting subject matter (Boyd 1982).

If these controversial consequences of a thorough-going 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 reliable guides to the truth – but it will be no less an empirical science for being normative in this way.

8. ISSUES OF PHILOSOPHICAL METHOD

In this section, I shall discuss two issues of philosophical methodology raised by the arguments for scientific realism described in section 6. First, I shall discuss at some length an important challenge raised by Arthur Fine against the basic strategy of those arguments. I shall then discuss, somewhat more briefly, certain questions about the ways in which evidence from the history of science bears upon the arguments in question.

In a recent paper, Fine (in press) raises a number of interesting objections to the arguments for scientific realism which I have outlined in section 6. (I am extremely grateful to Professor Fine for the opportunity to read a pre-publication copy of his paper.) Of these objections one is particularly striking because it challenges not the details of the argument for realism, but its basic philosophical strategy. I shall now turn my attention to this objection.

Fine's objection is extremely simple and elegant. The proposed defense of realism precedes 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 which 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 section 3. 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 which 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 which adduces 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 section 6. 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 in so far as the dispute between realists and empiricists is concerned – a debate over the legitimacy of inductive inferences 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 anti-realists.

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 inferences 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 anti-realist 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 will no longer lie exclusively on the realist's side: the anti-realist must justify the proposed limitation on an otherwise legitimate principle of inductive inference.

This difficulty for the anti-realist 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 epistemologically 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 (Boyd 1972, 1973, 1979, 1980, 1982) that this theory-dependence extends to the methods which scientists employ in making inductive generalizations about observable phenomena. Both the choice of the generalizations which are seriously advanced and the assessment of the evidence for or against them rest upon theoretical inferences which 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 the "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 which 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 which 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 which scientists arrive at by employing the scientific method. Even though the theoretical reasoning which 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 which 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 place, theoretical considerations are decisive in solving what Goodman (1973) calls the problem of "projectability". Given any finite body of observational data, there are infinitely many different generalizations about observables which 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 which 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. In order 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. Our conviction that the methods of science are instrumentally reliable turns on our conviction that those methods have led us to accept theories which 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 degree of confirmation are inadequate because they depend upon abductions to theoretical explanations, then the 2nd 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 which 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 which contribute to its instrumental reliability. This is a non-trivial 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.

In so far as the confirmation of the reliability hypothesis is concerned, the issue is not so much over 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 non-justificatory 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 easily seen if we employ a variant of the empiricists' favorite argument that theory choice is underdetermined by observational data. 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 only finitely many methodological judgments in the whole history of science to date. Even if you know which of these judgments have contributed to the reliability of past scientific practice, there will still be infinitely many different "methodologies" – infinitely many different sets of principles for theory-choice, experimental design, data assessment, etc. – 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 the 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 which 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 which 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 non-justificatory. 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 (see Boyd 1982). If the empiricist forgoes appeals to the abductive inferences of ordinary scientific practice, on the grounds that such inferences are non-justificatory, then it is hard to see how she can make scientifically sound judgment about which methods are scientific or about how to even 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 empiricists cannot both (a) hold that the inductive methods of scientists are justified in so far as generalizations about observables are concerned, and (b) 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 philosophically plausible way – the conclusion that the inductive inferences which 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 standard rebuttals to empiricist anti-realism, while they provide some reason to believe that scientific realism is true, fail to respond to the strong epistemological challenge which empiricist anti-realism 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. In order to assess the import of the circularity of appealing to abduction in replying to empiricist anti-realism, we must examine more closely the relation between the particular abductive inferences in question, and the empiricist's arguments against realism.

I suggest that our 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 "pairwise 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 considerations which are especially important. First, the empiricist's objection to abductive inferences (at least to those which yield conclusions about unobservable phenomena) rests upon the powerful and sophisticated epistemological argument rehearsed in section 3. 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 if the indistinguishability thesis represents a seriously misleading interpretation of it, then the empiricist's argument against abduction to theoretical explanation fails.

Secondly, 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 anti-realism discussed in section 3 strengthen the case for realism as an account of the structure of scientific knowledge, but 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 in section 6 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 that it is fair to say that, given the difficulties which plague empiricist anti-realism 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 wholely skeptical treatment of scientific inquiry.

I have suggested in section 7 (see also Boyd 1982) 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 anti-realism 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, in so far 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 which 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-realist and the scientific realist who subscribes to the argument sketched in section 6 is exactly analogous. The anti-realist'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, in so far 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, in so far 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 (see Boyd 1982). Like the causal theorist's rebuttal to phenomenalism, the scientific realist's rebuttal to empiricist anti-realism 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 empiricist 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.

Let us turn now to the question of the way in which evidence from the history of science bears upon the arguments for scientific realism which we have been discussing. I have emphasized the important role which, according to the version of naturalistic and realistic epistemology discussed in this paper, was played by the historically contingent emergence of research traditions embodying suitably approximately true theories of unobservables. If I am right, it is to the successive development of the approximate truths (theoretical as well as instrumental) embodied in these traditions that we owe the instrumental reliability of current scientific practice. Although it is no part of my thesis that this development was progressive in all particular instances, or occurred uniformly with respect to different disciplines, sub-disciplines, or even problem areas within sub-disciplines, it is essential to the thesis I am defending that there be some measure of referential continuity and successive approximation to the truth in the history of recent science (Boyd 1982). I emphasized in Section 4 of the present essay that if continuity of reference and methodology could not be established in many cases in the history of modern science, the sort of realism I am defending would be strongly undermined.

Because of the centrality of considerations of historical continuity to the abductive argument for scientific realism which we are considering here, I think it important to indicate ways in which historical continuity is *not* involved in that argument. In the first place, it is not a consequence of the position advocated here on behalf of the realist that a successful pattern of inductive generalization at the observational level must *always* rest upon the acceptance of relevantly approximately true background theories. In order for any inductive enterprise to be successful, there must be an appropriate correspondence between the categories in terms of which phenomena are classified, and their relevant causal powers. There is however nothing to prevent scientists or others from hitting upon categories appropriate to some limited class of generalizations by chance rather than as a result of theoretical understanding.

In mature sciences, however, scientists do not solve the problem of "projectability" by the specification of some relatively fixed sets of projectable properties or predicates, theoretical or observational. Instead, we possess a *methodology* for exploiting the full descriptive resources of our *theoretical* concepts to guide inductive inferences at the observational level. Instead of assessing the projectability of particular predicates, we are able to assess the projectability of theoretically characterizable patterns in observational data: we count as projectable any pattern in observational data which corresponds to a theoretical hypothesis which is plausible in the light of the current "total science". Moreover, we take such a hypothesis to represent the inductive generalization about observables which corresponds to the observational consequences derivable from the hypothesis itself, *together with* the theories which constitute the existing total science. Once such a hypothesis has been accepted, we countenance *further* expansion and modification of the inductive generalizations about observables which it warrants as our "total science" itself changes and develops (for a more precies discussion, see Boyd 1982). We are thus 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 in Section 6 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 which 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 which embodies approximate knowledge of unobservable as well as observable phenomena. It is this claim, after all, which the empiricist denies. (See Boyd 1982, especially Sections 2.2 and 3.4.)

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 "asymptotic limit" of scientific theorizing if science were pursued long enough. As anti-reductionist 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 which 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 which describe phenomena at the same "level of organization", and even for theories which 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 etc. which form the ontology of any one must be themselves causally realized by the entities which 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 anti-realist conception of causal relations (Boyd 1982, Section 3.3.; forthcoming (a)). 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 (see Boyd 1982, footnote 4).

Finally, the evidential connection between the historical evidence for continuity of theoretical semantics and of methods in mature sciences, on the one hand, and the thesis of scientific realism on the other, is quite subtle. Because scientific realists hold that progress in mature sciences is a reflection of theoretical as well as instrumental progress 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 (see Boyd 1979 for a more carefully qualified formulation of this claim). 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 Section 5 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 – 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 which 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 theoretical 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. Historical studies of such sciences – of, for example, the extent of semantic and methodological continuity in the history of those sciences – are thus evidentally relevant to the issue of realism. In so far as a realist perspective proves fruitful in understanding the history of mature sciences, that would provide further evidence for realism, but the primary role of historical studies in this area 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 non-recent 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 theoretical 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 realistic 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 latter 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.

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NOTES

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