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Realism, Approximate Truth, and Philosophical Method

1. Introduction

1.1. Realism and Approximate Truth

Scientific realists hold that the characteristic product of successful scientific research is knowledge of largely theory-independent phenomena and that such knowledge is possible (indeed actual) even in those cases in which the relevant phenomena are not, in any non-question-begging sense, observable (Boyd 1982). The characteristic philosophical arguments for scientific realism embody the claim that certain central principles of scientific methodology require a realist explication. In its most completely developed form, this sort of abductive argument embodies the claim that a realist conception of scientific inquiry is required in order to justify, or to explain the reliability with respect to instrumental knowledge of, all of the basic methodological principles of mature scientific inquiry (Boyd 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c; Byerly and Lazara 1973; Putnam 1972, 1975a, 1975b).

The realist who offers such arguments is not committed to the view that rationally applied scientific method will always lead to progress towards the truth, still less to the view that such progress would have the exact truth as an asymptotic limit (Boyd 1982, 1988). Nevertheless it would be difficult to defend scientific realism without portraying the central developments of twentieth-century physical science, for example, as involving a dialectical and progressive interaction of theoretical and methodological commitments (Boyd 1982, 1983).

A defense of realism along these lines requires that two things. In the first place, the realist must be able to defend a historical thesis regarding the recent history of relevant sciences according to which their intellectual achievements involve *approximate* theoretical knowledge and according to which theoretical progress within them has been (to a large extent) a process of (not necessarily converging) *approximation*. No realist conception that does not treat theoretical knowledge and theoretical progress as involving approximations to the truth is

even prima facie compatible with the actual history of science. The realist must, therefore, employ a conception of approximate theoretical knowledge and of theoretical progress through approximation that makes historical sense of the recent development of scientific theories.

Secondly, the realist must be able to establish that her historical appeal to approximate theoretical knowledge and to theoretical progress by successive approximation is appropriate by philosophical as well as by historical standards. Neither the realist's historical account nor her appeal to it in the defense of scientific realism as a philosophical thesis should be undermined by any of the distinctly philosophical considerations characteristic of anti-realist positions in the philosophy of science. Important challenges to scientific realism arise from doubts that a realist conception of approximate truth and of the growth of approximate knowledge is available that satisfies both of these constraints. The appropriate realist responses to these challenges and the philosophical implications of those responses are the subject of the present essay.

1.2. Challenges to a Realist Treatment of Approximation

A number of philosophers (realists included) have had serious concerns about the realist's ability to provide an adequate account of the development of scientific theories as involving the growth of approximate theoretical knowledge. The *locus classicus* of objections to realism reflecting these concerns is surely Laudan 1981 (see also Fine 1984). That there should be such concerns is, in significant measure, a reflection of the striking difference between the depth of our understanding of the notion of (exact) truth and that of our understanding of approximate truth.

Since the work of Tarski in the 1930s we have had a systematic, general, and topic-and-context-independent mathematical and philosophical theory of (exact) truth. By contrast there is no generally accepted general and systematic theory of approximate truth. We have available from the various special sciences a very large number of well-worked-out examples of particular instances of approximation but the details in theses cases depend not only on contingent and often esoteric facts about the relevant natural phenomena, but also upon the particular context of application within which the approximate theories and models are to be applied. In part because of the complexities created by such topic and context dependence, we do not have as clear a general understanding of what the epistemological relevance of appeals to approximate truth should be. Moreover, as we shall see, the dependence of the relevant details upon a posteriori theoretical claims raises special problems of philosophical method when an appeal to conception of approximate truth is to be made in the course of a defense of scientific realism.

I have argued elsewhere (Boyd 1982, 1983, 1985a, 1985b, 1985c, 1988) that the scientific realist must adopt distinctly naturalistic conceptions of philosophical methodology and of central issues in epistemology and metaphysics. My aim in

the present paper will be to show how the distinctly naturalistic arguments for realism that I have developed in the papers cited can be extended to provide an adequate realist treatment of approximate truth.

Instead of replying to particular anti-realist arguments in the literature, I shall respond to four objections that capture, I believe, the deep philosophical concerns that the realist's conception of approximate theoretical knowledge properly occasion. My expectation is that the responses to those objections will provide an adequate basis for a realist's response to other objections regarding her conception of approximate truth and approximate knowledge. The objections I shall consider are these:

1. (*The historical objection*) Realists are simply mistaken as a matter of historical fact: many important scientific advances seem to have been grounded in what (by realist standards) were deep errors in background theories. Approximately true background theoretical knowledge is thus not required to explain reliability of scientific practices.

2. (*The triviality objection*) The the realist might reply (following Hardin and Rosenberg 1982, for example) about many of the advances in question that the relevant background theories were *to some extent* or *in some respects* approximately true.

Here the realist's philosophical project is in danger of being reduced to *triviality*. The problem is that we lack altogether a general theory of approximation: we have no general characterization of what it is for a sentence to be approximately true, to be approximately true to a specified degree or in a specified respect, or to be more nearly true (in specific respects or in general) than some other sentence. If we had such a general theory then the realist could appeal to it in refining the thesis that the relevant historical episodes reflect some respects of approximation to the truth. As it is we are faced with the fact that *any* consistent theory is approximately true in some respects or other, and *any* sequence of such theories will reflect progress towards the truth in some respects or other.

3. (*The contrivance objection*) The realist might next reply by distinguishing between relevant and irrelevant respects of approximation to the truth regarding matters theoretical, and by claiming that the growth of scientific knowledge characteristically involves the former. Here the realist avoids triviality at the expense of a contrived or ad hoc conception of approximate truth, indeed at the expense of both contrivance (objection 3) and circularity (see objection 4).

The contrivance in question arises from the important difference just mentioned between extant theories of truth and of approximate truth respectively. In the case of truth *simpliciter* Tarski's strategy for defining truth (Tarski 1951) provides a uniform treatment that is largely independent of the particular subject matter or of the particular historical episodes or context of application under consideration. By contrast, our conception of relevant approximation reflects

considerations specific to the particular theory or theories, historical settings, and contexts of application under consideration.

Thus, for example, if the realist sees relativistic mechanics as growing out of previously acquired approximate theoretical knowledge her conception of the relevant respects of approximation reflected in Newtonian mechanics will emphasize numerical accuracy for systems of particles with relative velocities low with respect to that of light, the identification of, and the development of reliable measurement procedures for, various physical magnitudes, and the central role assigned to certain fundamental laws. It will de-emphasize, for example, numerical accuracy for high relative velocities, or of soundness of the Newtonian theoretical conception of space and time.

Here the distinctions between relevant and irrelevant respects of approximation reflect judgements, based on current theoretical conceptions, about the respects in which Newtonian mechanics happened to be approximately true, and similarly theory-dependent judgments about the role that such approximations played in the successful development of relativistic mechanics. Since we lack a general theory of approximation, the realist's appeal to relevant respects of approximation in response to the triviality objection will always have to be grounded in just this sort of topic-and-episode-sensitive conception.

We can now see why the realist's treatment of respects of approximation will involve an ad hoc or contrived element. For each of the episodes of scientific inquiry typically considered by philosophers of science there is a standard realist picture (or, at any rate, a narrow range of such pictures) of how the relevant approximations to the truth have gone and what contributions, if any, they have made to the subsequent growth of scientific knowledge. The realist, in defining the relevant sense(s) of approximation, will rely on such a picture. But such a picture merely reflects the realist research tradition in the history and philosophy of science. Since there is no topic-and-episode-neutral conception of relevant approximation with respect to which her proposed definitions may be assessed, the realist will simply be presupposing the soundness of the "findings" of her own tradition when she defines the difference(s) between relevant and irrelevant respects of approximation. It is no surprise—and certainly no basis for an abductive argument for realism—that the realist can construct a realist account of approximate truth when she is permitted to beg questions in so thoroughgoing a way.

4. (*The circularity objection*). There is some precedent in scientific inquiry, especially historical inquiry, for explanatory concepts that lack topic-and-episode-neutral general specifications of the sort alluded to above: sometimes theoretical considerations that resist incorporation into a fully general definition can justify the (topic-and-episode-nonneutral) ways in which such concepts are applied in particular cases. Let us suppose for the sake of argument that this is the case with respect to the employment of the concept of approximate truth in the various historical explanations of scientific progress (or its absence) that are

offered in the realist tradition. Even if the realist's accounts of the relevant episodes are thus methodologically acceptable *as explanations in the history of science,* they will involve an unacceptable circularity if they are understood to address the *philosophical* issue between scientific realists and anti-realists.

Here's why: Any realist explanation of the growth of knowledge and of reliable methodology in a particular field must involve an account of the kinds of epistemically relevant causal interactions that exist(ed) between members of the relevant scientific community and the features of the world that were (or are) the alleged objects of their study. Thus for example, a realist account of such developments in atomic theory will incorporate a causal account of how scientists gain(ed) epistemic access to various subatomic particles and the realist's claim that atomic theory is about such unobservable theory-independent particles will depend on that account (see sections 2.1.3 and 2.1.4). The realist's account of epistemic access to subatomic particles will be grounded in the best available theory of such particles together with related contemporary physical theories.

Suppose now that the realist's explanation of the development of some field, including the relevant account of epistemic access, is advanced in defense of realism as a philosophical thesis. Plainly the resulting defense of realism is cogent only if the realist's explanation, and her account of epistemic access in particular, are understood *realistically.* For example, only if the account of epistemic access to subatomic particles is understood realistically is the realist's case that atomic theory has an unobservable and theory-independent subject matter advanced. But, on the realist's own account, her explanation and the account of epistemic access it incorporates are ordinary scientific theories themselves grounded in the very research tradition regarding which a defense of realism is sought. To insist on a realistic interpretation of the realist's explanation would thus *presuppose* realism regarding the tradition in question. Thus the realist's appeal to her explanation of the development of instrumentally reliable methodology in an abductive argument for realism as a philosophical thesis is question-beggingly circular.

1.3. An Argumentative Strategy

The challenges we are considering seem to fall into two classes: The first three represent an essentially prephilosophical critique of the realist's historical explanations: they deny that the realist's conception of the role of approximate truth regarding theoretical matters in the growth of scientific knowledge represents the best explanation for the relevant episodes in the history of science. The fourth offers a distinctly philosophical challenge: it argues that even if the *realist's* account of the growth of scientific knowledge does provide the best explanation, inductive inference to *realism* begs the philosophical question at issue.

After some philosophical preliminaries, I propose to respond to the challenges in two distinct stages corresponding to these two classes . In the first stage of my

response, I treat the characteristic realist explanatory appeal to approximate truth as an ordinary piece of historical explanation. I identify a general methodological problem of *parametric specification* in explanatory contexts of which the deeper problems raised by the first three challenges are special cases, and I identify the generally appropriate solution to that problem. I then indicate why it is plausible that the realist's explanatory appeal to approximate truth satisfies the methodological demands dictated by the solution in question.

With respect to the fourth challenge, I assume for the sake of argument that the realist's historical explanations have been confirmed and I inquire whether they are to be understood realistically or whether instead such an understanding—which is essential to the realist's case—begs the question against the anti-realist. Here too I argue that the methodological question regarding the realist's appeal to approximate truth—in this case a question about *philosophical* method—is a special case of a more general methodological question about the appropriate interaction between philosophical considerations and empirical findings in the philosophy of science. I define the notion of a large-scale *philosophical package* and I indicate why the incorporation of realistically understood scientific theories into the realist philosophical package is compatible with (and indeed required by) an adequate *and noncircular* defense of the realist package against rival philosophical conceptions.

On now to the philosophical preliminaries.

2. Philosophical Preliminaries

2.1. The Abductive Argument for Scientific Realism

The challenges we are considering arise in the context of a class of abductive arguments for realism according to which we must recognize approximate knowledge of unobservable (and appropriately mind-independent) "theoretical entities" in order to adequately explain the growth of even instrumental knowledge in recent science. To assess the realist's arguments and the appeals to the notion of approximate truth embodied in them, we need an understanding of just what those arguments are. In what follows of this section I'll indicate, in broad outline, how the abductive arguments for realism go.

2.1.1. Objective Knowledge from Theory Dependent Method

By the "instrumental reliability" of a scientific theory I mean the extent of its capacity to make approximately true observational predictions about observable phenomena—the extent of its approximate empirical adequacy. By the "instrumental reliability" of some body of methods I mean the extent to which their practice is conducive to the acceptance of instrumentally reliable theories. The abductive arguments for scientific realism take place in a dialectical situation in

which scientific realists and their philosophical opponents largely agree that the methods of actual recent scientific practice are significantly instrumentally reliable.

The abductive arguments for realism are in the first instance directed against the empiricist who denies the possibility of "theoretical" knowledge—knowledge of "unobservables." Against the empiricist the realist argues that only by accepting the reality of approximate theoretical knowledge can we adequately explain the (uncontested) instrumental reliability of apparently theory-dependent scientific methods. In the present paper I shall focus my attention primarily on the dispute between realists and empiricists, reserving attention to the corresponding dispute between realists and constructivists largely to a later paper. I discuss the realism-constructivism dispute briefly in section 2.4 and briefly discuss the distinctly constructivist version of the circularity objection in section 4.3.

The case for realism lies largely in the recognition of the extraordinary role that theoretical considerations play in actual (and patently successful) scientific practice. To take the most striking example, scientists routinely modify or extend operational "measurement" or "detection" procedures for "theoretical" magnitudes or entities on the basis of new theoretical developments. The reliability and justifiability of this sort of methodology is perfectly explicable on the realist's conception of measurement and of theoretical progress. Accounts of the revisability of operational procedures that are compatible with an empiricist position appear inadequate to explain the way in which theory-dependent revisions of "measurement" and "detection" procedures make a positive methodological contribution to the progress of science.

There are two important consequences of the realist explanation for the reliability of the methodology in question. First, scientific research, when it is successful, is *cumulative by successive (but not necessarily convergent) approximations to the truth.* Second, this cumulative development is possible because *there is a dialectical relationship between current theory and the methodology for its improvement.* The approximate truth of current theories explains why our existing measurement procedures are (approximately) reliable. That reliability, in turn, helps to explain why our experimental or observational investigations are successful in uncovering new theoretical knowledge, which, in turn, may produce improvements in measurement techniques, etc.

Theory dependence of methods and the consequent dialectical interaction of theory and method are entirely general features of all aspects of scientific methodology—principles of experimental design, choices of research problems, standards for the assessment of experimental evidence and for assessing the quality and methodological import of explanations, principles governing theory choice, and rules for the use of theoretical language. In all cases there is a pattern of dialectical interaction between accepted theories and associated methods of just the sort exemplified in the case of the theory dependence of measurement and de-

tection procedures. Moreover, this pattern of theory dependence contributes to the reliability of scientific methodology rather than detracting from it (Boyd 1972, 1973, 1979, 1980, 1982, 1983, 1985a, 1985b, 1985c; Kuhn 1970, Putnam 1972, 1975a, 1975b; Van Fraassen 1980).

According to the realist, the only scientifically plausible explanation for the reliability of a scientific methodology that is so theory dependent is a thoroughgoingly realistic explanation: Scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge *precisely because, and to the extent that, currently accepted theories are relevantly approximately true.* Scientific method provides a paradigm-dependent paradigm-modification strategy: a strategy for modifying or amending our existing theories and methods in the light of further research that is such that its methodological principles at any given time will themselves depend upon the theoretical picture provided by the currently accepted theories. If the body of accepted theories is itself relevantly sufficiently approximately true, then this methodology operates to produce a subsequent dialectical improvement both in our knowledge of the world and in our methodology itself. It is not possible, according to the realist, to explain even the instrumental reliability of actual recent scientific practice without invoking this explanation and without adopting the realistic conception of scientific knowledge that it entails (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c).

2.1.2. Projectability, Evidence, Theoretical Plausibility and the Evidential Indistinguishability Thesis

If the realist's abductive argument is correct, a dramatic rethinking of our notion of scientific evidence is required. Consider the question of the "degree of confirmation" of a theory given a body of observational evidence. To a very good first approximation, a theory receives significant evidential support from a body of successful predictions (or other evidentially favorable observations) just in case (a) the theory is itself "projectable" (see Goodman 1973), (b) the observations in question pit the theory's predictions (or, in other contexts, its explanations) against those of its projectable rivals; and (c) in the relevant experiments or observational settings, there have been suitable controls for those possible artifactual influences that are themselves suggested by projectable theories of those settings (Boyd 1982, 1983, and especially 1985a).

Central to the realist's argument is the observation that projectability judgments are, in fact, judgments of theoretical plausibility: we treat as projectable those proposals that relevantly resemble our existing theories (where the determination of the relevant respects of resemblance is itself a theoretical issue). The reliability of this conservative preference is explained by the approximate truth of existing theories, and one consequence of this explanation is that *judgments of theoretical plausibility are evidential.* The fact that a proposed theory is plausible in the light of previously confirmed theories is some evidence for its (ap-

proximate) truth. Judgments of theoretical plausibility are matters of inductive inference from (partly) theoretical premises to theoretical conclusions; precisely these inferences justify, and explain the reliability of, "inductive inference to the best explanation" (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c) .

The claim that judgments of theoretical plausibility are evidential affords the realist a reply to the deepest empiricist argument against realism. The empiricist appeals (tacitly or explicitly) to a principle that I have called the *evidential indistinguishability thesis.* In its most plausible form it holds that for any two empirically equivalent total sciences, the empirical support or disconfirmation that one receives, given a given body of observational data, will be just the same as that received by the other. The empiricist's conclusion that knowledge of unobservables is impossible is a straightforward application of this thesis, which can be thought of as an empiricist analysis of the claim that all scientific knowledge is empirical knowledge. The realist accepts the latter claim but rejects the empiricist analysis. Instead, the realist holds, evidential considerations regarding theoretical plausibility are indirectly experimental and can serve to distinguish total sciences that embody or naturally extend the current total science (that are favored by those considerations) from empirically equivalent total sciences which significantly depart from the prevailing total science (which such considerations reject as unprojectable). [See Boyd 1982, 1983, and section 2.2.]

2.1.3 . Natural Definitions

Locke speculates at several places in Book IV of the *Essay* (see, e.g., IV, iii, 25) that when kinds of substances are defined, as empiricism requires, by purely conventional "nominal essences," it will be impossible to have a general science of, say, chemistry. There is no reason to believe that kinds defined by nominal essences will reflect actual causal structure and thus be apt for the formulation or confirmation of general knowledge of substances. Only if we are able to sort substances according to their hidden real essences will systematic general knowledge of substances be possible.

Locke was right (at any rate so the realist thinks). Only when kinds (properties, relations, magnitudes, etc.) are defined by natural rather than conventional definitions is it possible to obtain the theory-dependent solutions to the problem of projectability just described (Putnam 1975a; Quine 1969a; Boyd 1979, 1982, 1983). It is thus central to the realist's abductive argument that most scientific terms be seen as possessing natural rather than conventional definitions. Such terms are defined in terms of properties, relations, etc., that render the kinds (etc.) to which they refer appropriate to particular sorts of scientific or practical reasoning. In the case of such terms, proposed definitions are always in principle revisable in the light of new evidence or new theoretical developments, and it is possible for people to refer to the same kind (property, magnitude, etc.) by a term while disagreeing about what its correct a posteriori natural definition is. This last

consequence of the naturalistic conception of definitions is essential to the realist's dialectical conception of the development of scientific knowledge and methods. The realist will (at least typically) need to portray developments in which mature scientific communities change their conception of the definitions of kinds, relations, magnitudes, etc., as dialectical advances (or, if things go badly, setbacks) rather than as changes of subject matter (Putnam 1972, 1975a, 1975b; Boyd 1979, 1980, 1988). (For more on naturalistic definitions see section 2.5.)

2.1.4. Reference and Epistemic Access

If the traditional empiricist account of definition is to be abandoned for scientific terms in favor of a naturalistic account, then a naturalistic conception of reference is required for such terms. An account of the appropriate sort is provided by recent causal theories of reference (see, e.g., Feigl 1956, Kripke 1972, Putnam 1975a). The reference of a term is established by causal connections of the right sort between the use of the term and (instances of) its referent.

The connection between naturalistic theories of reference and of knowledge (see section 2.2) is quite intimate: reference is itself an epistemic notion and the sorts of causal connections that are relevant to reference are just those that are involved in the reliable regulation of belief (Boyd 1979, 1982). *Roughly,* and for nondegenerate cases, a term t refers to a kind (property, relation, etc.) k, just in case there exist causal mechanisms whose tendency is to bring it about, over time, that what is predicated of the term t will be approximately true of k. In such a case, we may think of the properties of k as regulating the use of t, and we may think of what is said using t as providing us with socially coordinated *epistemic access* to k. t refers to k (in nondegenerate cases), just in case the socially coordinated use of t provides significant epistemic access to k, and not to other kinds (properties, etc.) (Boyd 1979, 1982). The mechanisms of reference *just are* the mechanisms of reliable belief regulation .

Thus, just as the realist conception requires, two different terms, or the same term in two historically different settings, may afford epistemic access to, and thus may refer to, the same kind (property, etc.) even though the definitions associated with them by the relevant linguistic communities are quite different or even inconsistent.

One further feature of the naturalistic conception of reference is important to an understanding to the realist's conception of the growth of approximate knowledge. In many scientifically important cases the use of a term may afford epistemic access to more than one kind (property, relation, . . .), but our knowledge may be insufficient for us to recognize that this is so, and we may consequently have a conception of, as it seems to us, one kind (etc.) that conflates information regarding several distinct kinds.

Field (1973, 1974) calls the relation thus established between a term and several kinds (etc.) *partial denotation,* and he calls the revision of language usage

to eliminate such cases of ambiguity *denotational refinement*. On the realist's conception of the growth of approximate knowledge one sort of approximate knowledge is that represented by a body of sentences involving a partially denoting term when what is predicated of that term in these sentences represents methodologically important approximations to the truth regarding one or more of the relevant *partial denotata* considered individually. In such cases, one characteristic form of subsequent improvement in approximation is the discovery of the ambiguity and the consequent denotational refinement (see Boyd 1979).

2.2. Naturalism and Radical Contingency in Epistemology

Modern epistemology has been largely dominated by "foundationalist" conceptions: all knowledge is seen as grounded in certain foundational beliefs that have an epistemically privileged position. Other true beliefs are instances of knowledge only if they can be justified by appeals to foundational knowledge. It is an a priori question which beliefs fall in the privileged class. Similarly, the basic inferential principles that are legitimate for justifying nonfoundational knowledge claims can be justified a priori; it is moreover an a priori question about a given inference whether it meets the standards set by those principles or not. We may fruitfully think of foundationalism as consisting of two parts, *premise foundationalism* which holds that all knowledge is justifiable from an a priori specifiable core of foundational beliefs, and *inference foundationalism*, which holds that principles of justifiable inference are reducible to inferential principles that are *a priori justifiable* and whose application is *a priori checkable.*

Recent work in "naturalistic epistemology" (see, e.g., Armstrong 1973; Goldman 1967, 1976; Quine 1969b) strongly suggests that foundationalism is fundamentally mistaken. For the typical case of perceptual knowledge, there seem to be neither premises nor inferences; instead perceptual knowledge obtains when perceptual beliefs are produced by epistemically reliable mechanisms. Even where premises and inferences occur, it seems to be the reliable production of belief that distinguishes cases of knowledge from other cases of true belief. A variety of naturalistic considerations suggest that there are no beliefs that are epistemically privileged in the way foundationalism seems to require.

I have argued (see Boyd 1982, 1983, 1985a, 1985b,1985c) that the abductive defense of scientific realism requires an even more thoroughgoing naturalism in epistemology and, consequently, an even more thoroughgoing rejection of foundationalism. In particular *all* of the significant methodological principles of inductive inference in science are profoundly theory dependent. They are a reliable guide to the truth only because, and to the extent that, the relevant background theories are relevantly approximately true. They are not reducible to some more basic rules whose reliability as a guide to the truth is independent of the truth of background theories. Since it is a contingent empirical matter which background

theories are approximately true, the justifiability of scientific principles of inference rests ultimately on a contingent matter of empirical fact, just as the epistemic role of the senses rests upon the contingent empirical fact that the senses are reliable detectors of external phenomena. Thus inference foundationalism is radicallIy false; there are no a priori justifiable rules of nondeductive inference, and it is an a posteriori question about any such inference whether or not it is justifiable. The epistemology of empirical science is an empirical science (Boyd 1982, 1983, 1985a, 1985b).

One consequence of this radical contingency of scientific methods is important to the realist's conception of the growth of approximate knowledge. The emergence of successful modern scientific methodology as we know it depended upon the logically, epistemically, and historically contingent emergence of a relevantly approximately true theoretical tradition. It is not possible to understand the initial emergence of such a tradition as the consequence of some more abstractly conceived scientific or rational methodology that itself is theory independent. There is no such methodology. The theoretical innovations that established the first successful paradigm within a particular scientific discipline must be thought of as the beginnings of successful methodology within the field, not as consequences of it (for a further discussion see Boyd 1982).

Note that radical contingency in epistemology is central to the realist's case against empiricism. Against the evidential indistinguishability thesis the realist argues that plausibility judgments grounded in the current total science afford evidential distinctions between empirically equivalent total sciences. But, according to the realist's account, it is not the *currency* of the current total science that makes plausibility judgments with respect to it epistemically reliable but its approximate truth. That a time should have arisen in which total sciences embodied relevant approximations to the truth is of course radically contingent. Thus central to the realist's rebuttal to empiricism are the epistemological principles that reflect that contingency.

2.3. Metaphysics and 'Metaphysics'

Logical positivists employed the term 'metaphysics' for the sort of inquiry about "unobservables" that verificationism led them to reject. Most of what has traditionally fallen under that term was 'metaphysics' in the positivists' sense, but so was inquiry about, e.g., the atomic structure of matter. If scientific realism is right, then it follows that scientists routinely do successful 'metaphysics'. With respect to metaphysics (as philosophers and others ordinarily use the term) the situation is more complex.

If scientific realism is true for any of the standard reasons then scientists have discovered the real essences of chemical kinds (Kripke 1971, 1972) and have thus done some real metaphysics. Moreover, the fact that scientific knowledge of un-

observables is possible makes it a serious question whether or not scientific findings have (or will have) resolved some traditional metaphysical questions. Certainly the recent near consensus in favor of a materialist conception of mind reflects a realist understanding of the possibility of experimental metaphysics. Nevertheless it does not follow from scientific realism that scientists routinely tend to get the right answers to the distinctly metaphysical questions that are the special concern of philosophers even when their methods lead them to adopt theories that reflect answers to such question.

In particular, when a scientific realist proposes to explain the reliability of the scientific methods employed at a particular historical moment by appealing to the approximate truth of the background theories accepted at that time, she need not hold that the metaphysical conceptions embodied in those theories represent a good approximation by philosophical standards. Two examples will illustrate the point.

Consider the way in which the reliability of the methods by which Darwin's account in the *Origin* was assessed is to be explained by reference to the approximate truth of much of the prevailing background biological theory. A great deal was known, for example, about species—not just facts about particular species, but about anatomical, behavioral, genetic and biogeographical generalizations that can only be formulated in terms of the notion of a species. The realist will hold that the approximations to the truth embodied in this lore of species is part of what explains the reliability of the research methods in biology employed by Darwin and his contemporaries.

Prior to Darwin's work the prevailing conception made species membership in the first instance a property of individuals; after Darwin we have correctly seen a species as in the first instance a family of populations. The background biological theories of Darwin's era got it profoundly wrong about the metaphysics of species. Nevertheless, the classificatory practices of pre-Darwinian biologists were reliable enough to serve to establish the rich and significantly accurate lore about species upon which the reliability of methodology in early evolutionary theory crucially depended—or, at any rate, so the realist may reasonably maintain.

Similarly, the realist will want to explain the reliability of the methods by which physicists assessed early developments in quantum theory by appealing to respects in which the prequantum theory of, say, atoms and subatomic particles was approximately true. She will appeal to the correct identification of various subatomic particles and of (many of) the fundamental physical magnitudes, to the availability of reliable procedures for the detection of those particles and for the measurement of various of their physical properties, and to the classical insights reflected both in the formulation of the equation for the time-evolution of quantum mechanical systems and in the techniques employed in practice in picking the appropriate Hamiltonian for quantum mechanical systems.

Indeed she will want to portray much of the early development of quantum the-

ory as the gradual extension of the range of phenomena for which an adequate quantum mechanical treatment had been provided. On such an account, at any given stage in the early development of quantum theory, the proposed models for physical systems were always a mixture of distinctly quantum mechanical components together with essentially classical (or relativistic) components awaiting later quantum mechanical reformulation. The realist will want to explain the reliability and justifiability of this sort of development by appealing to the respects of approximation to the truth of classical mechanics itself and of the successive stages in the development of the quantum theory.

Consider now the classical conception of atomic phenomena understood as a contribution to philosophical metaphysics. Arguably the metaphysical component of that conception is some sort of mechanistic atomism: a picture of discrete and unproblematically individuated particles and their associated fields interacting in a deterministic fashion without action at a distance. Our current quantum mechanical conception of matter rejects each component of this picture: for the atomist's discrete particles we substitute entities with wavelike features for which particlelike individuation is sometimes impossible; we reject determinism; and we acknowledge that there are nonlocal effects that would surely be precluded by the classical philosophical rejection of action at a distance. Classical conceptions of the atomic world were, let us agree, poor approximations to the truth in metaphysics. Does this preclude their having been good enough approximations in other respects to sustain the realist's account of the development of quantum theory?

Plainly not. Whatever other objections there may be to the realist's account, it is not a cogent objection that the classical conception that her account treats as relevantly approximately true is not good metaphysics. All she need do is to explain how the metaphysical errors in the classical conception failed to vitiate the methodological contribution of its genuine insights. To this end she might, e.g., appeal to the respects in which subatomic particles are (classical) particlelike, to the determinism of the time-evolution of quantum mechanical systems prior to measurement, and to the wide variety of phenomena that do not significantly exhibit the effects of nonlocal "action at a distance." Perhaps in the case of the development of evolutionary theory and certainly in the case of quantum mechanics, the realist's account will have scientists doing 'metaphysics' with some significant success; in neither case must she portray them as doing good metaphysics.

The cases just discussed illustrate an additional point. In each case, if the metaphysical criticism of the earlier theoretical tradition is sound, then it embodied, in addition to metaphysical errors, errors about the logical form of certain key propositions. Conspecificity is a relation between populations, not individuals; so pre-Darwinian biology embodied a mistake about the logical type of propositions regarding species membership. Similarly, quantum mechanics requires that we think of the classically acknowledged physical magnitudes as cor-

responding to Hermitian operators rather than to vector- or scalar-valued functions; in consequence classical mechanics is mistaken about the logical form of, e.g., attributions of position or momentum to particles. Neither error undermines the contribution that the approximate truth of the earlier theory is said to have made to the methodology by which the latter theory was developed and confirmed. The realist need attribute to successful background theories neither metaphysical success nor logical exactitude. Approximation need not be philosophically clean. (Note that the distinctly realist naturalistic semantic conceptions are operative in this discussion. What evolutionary theory and quantum mechanics have taught us is that, as we might say, "there are no classical species" and "there are no classical particles." Only naturalistic alternatives to the empiricist conceptions of definitions and reference permit the realist to say—as the account just given requires—that nevertheless Darwinian species and the particle-like phenomena acknowledged by quantum mechanics were the subjects of the relevant classical investigations.)

2.4. Realism Causation and Mind Independence

The realist conception of science contrasts with various neo-Kantian constructivist conceptions according to which when scientific theories address fundamental questions there is a deep element of social construction of reality reflected in what they say. It is sometimes said that realists and constructivists differ over the extent to which the reality studied by scientists is "mind independent" or is "theory independent." In order to understand the demands placed on the realist by what we have called the "circularity objection," we require some understanding of what is distinctly realist about the realist's explanatory appeal to approximate truth of theoretical presuppositions, given that the constructivist shares with the realist the conviction that scientific progress involves theoretical as well as instrumental knowledge and that scientific methods are deeply theory dependent. In the present essay I'll touch on this issue only briefly.

2.4.1. Defining Mind Independence

The realist and the constructivist each reject the Humean and verificationist claim that reference to hidden mechanisms, essences, and causal powers is, on "rational reconstruction," eliminable from the findings of science. They agree scientists' methods and conceptions are determined by ineliminably metaphysical conceptions about the basic sorts of mechanisms, processes, and forces that operate to produce the phenomena under study and that this dependence is not merely a psychological quirk of the "context of invention" to be rationally reconstructed away in the "context of confirmation." They agree too in rejecting the eliminative Humean or regularity account of the causal powers and relations discovered by scientific inquiry. So where does the difference lie, what is the import of the ques-

tion of the mind or theory independence of reality given that both parties reject empiricism?

The answer, subject to an important qualification, is that the realist denies, while the constructivist affirms, that the adoption of theories, paradigms, conceptual frameworks, perspectives—or the having of associated interests, intentions, purposes, etc.—in some way constitutes, or contributes to the constitution of, the causal powers of, and the causal relations between, the objects scientists study in the context of those theories, interests, etc. Of course (here is the qualification) the realist does not deny that the adoption of theories, etc., and the having of projects or interests, are themselves causal phenomena and thus contribute *causally* to the establishment of, for example, those causal factors that are explanatory in, for example, the history, philosophy and sociology of science and that in consequence the adoption of a theory in such a discipline could contribute causally to the causal powers and relations that are the subject matter of the theory itself. What the realist denies is that there is some further sort of contribution (logical, conceptual, socially constructive, or the like) which the adoption of theories or the having of interests makes to the establishment of causal powers and relations.

Thus the realist denies the *noncausal* contribution of minds and (the adoption of) theories to the establishment of causal powers and relations, whereas the constructivist insists that such a contribution is fundamental. While the present paper focuses primarily on the realist's abductive argument against empiricism, it is important to note two constraints that a suitably developed realist explanation of the reliability of scientific methods must meet if there is to be any prospect of its serving as the basis for a rebuttal to constructivism. In the first place, the definitions of natural kinds, categories, etc., to which the realist's explanation makes essential reference are, in a certain sense, interest dependent. The properties and causal powers that are relevant to explanation or prediction depend on the practical or theoretical projects being undertaken. Thus appropriateness of definitions and conceptual frameworks depends upon the interests with respect to which they are to be employed. The realist must acknowledge this fact in a way which is compatible with denying that the interest dependence in question involves any noncausal contribution of the adoption of interests or projects to the causal powers of the objects of scientific study.

Similarly, as Quine and others have reminded us, even when an agenda of interests and projects is fixed, there may be several ways of defining kinds and categories—of "cutting the world at its joints"—that are equally adequate to the task of reflecting explanatorily significant causal relations (even as the realist understands those relations). It may sometimes happen that the theoretical commitments of two such frameworks will appear to involve conflicting metaphysical conceptions. The choice between such frameworks will be, for the realist, arbitrary. Thus the realist's account of approximation must not treat one such

framework as more nearly approximately true than the others, despite apparent metaphysical conflicts; certainly it must not treat the adoption of one rather than another as contributing noncausally to the establishment of causal relations or to similar settling of matters metaphysical.

It is by no means uncontroversial that arbitrariness and the interest dependence of kinds can be treated in the way the realist requires. For the purposes of the present essay I'll assume that an appropriate realist treatment is possible, while acknowledging that, in an essay in which constructivism rather then empiricism was the primary target, the question would require more extensive treatment. Two other issues regarding mind independence deserve our brief attention.

2.4.2. Mind Independence and the Causal Role of Minds

We have seen that the realist acknowledges the causal role of mental phenomena (since, e.g., she explains the reliability of scientific method by reference to the causal powers of approximately true beliefs) and differs from constructivists only in that she denies such phenomena a noncausal role in constituting causal structure. Nevertheless there are cases in which the attribution of a plainly causal role to mental phenomena has been seen as supporting constructivism. Two such cases deserve attention. First, scholars who are impressed by the social role of ideology often claim that "human nature" and the "natures" of various socially defined groups are "social constructions," and often they appear to mean by this, at least in the first instance, that the actual psychological capacities and tendencies exhibited by people generally or by members of socially defined groups are significantly determined by the ideologically established beliefs about psychological tendencies and capacities that are accepted in their own culture—determined in such a way as to tend to make their psychologies conform to the ideology.

Interestingly, many who make such claims seem to take this mode of social construction to be appropriate to a constructivist conception of reality and of knowledge. Plainly this is not so. Whatever the independent evidence for constructivism, the fact that culturally transmitted stereotypes causally influence the actual psychological makeup of those stereotyped provides no evidence of the sort of *non*causal determination of causal structure by minds or theories that the constructivist requires.

The second case concerns solutions to the problem of defining the notion of measurement in quantum mechanics. According to one important conception, part of what characterizes measurements is that they are epistemically relevant interactions so that measurement is defined in terms of knowledge—that is in terms of something (one component of which is) mental—and it is a special sort of interaction with a knowing system that produces discontinuous changes in physical state and results in sharp values for measured quantities. It is sometimes added that the explanation for the fact that measurements are not governed by

Schrödinger's equation is that they involve interactions between a physical system (whose isolated time evolution is governed by that equation) and a nonphysical mind. Whether or not the second suggestion is adopted, it is sometimes suggested that the special role of knowing systems thus identified refutes realism because it shows that the phenomena studied by scientists – in particular the results of their experimental measurements – are mind dependent. Reflection shows that this interpretation (even in its dualist version) simply assigns a distinctive causal role to certain mental phenomena. No noncausal social construction of causal structure is suggested. Indeed, the development of quantum mechanics might well be cited as the most dramatic recent demonstration of our *inability* to define causal reality in accordance with our conceptual schemes (for an excellent discussion see McMullin 1984).

2.5. Homeostatic Property-Cluster Definitions, Realism and Bivalence

There is an established practice of identifying realism regarding a body of inquiry with the view that all of the sentences in the vocabulary employed within it have determinate mind-independent truth values and such a conception of realism places a significant constraint on any realist account of the growth of approximate knowledge. We have just seen that the requirement of mind independence must be carefully qualified. Moreover, the role in approximation that the realist assigns to partial denotation and to denotational refinement (see 2.1.4) precludes any understanding according to which scientific statements must have determinate truth value: statements involving partially denoting expressions might be true on one denotational refinement and false on another.

There is a quite different way in which a realist conception of scientific language predicts failures of bivalence, and it is important to our understanding of the realist's explanatory project both because it reflects another dimension of dialectical complexity in the realist's account of approximation and because it provides the philosophical machinery for a deeper analysis of the underlying notion of scientific rationality.

The sorts of essential definition of substances anticipated by Locke and reflected in the currently accepted natural definitions of chemical kinds by molecular formulas (e.g., "water = H_2O") appear to specify necessary and sufficient conditions for membership in the kind in question. Recent *non*naturalistic property-cluster or criterial attribute theories in the "ordinary language" tradition suggest the possibility of definitions that do not provide necessary and sufficient conditions. Instead, some terms are said to be defined by a collection of properties such that the possession of an adequate number of those properties is sufficient for falling within the extension of the term. It is supposed to be a conceptual (and thus an a priori) matter what properties belong in the cluster and which combinations of them are sufficient for falling under the term. However,

it is usually insisted that the kinds corresponding to such terms are "open textured," so that there is some indeterminacy in extension legitimately associated with property-cluster or criterial attribute definitions. The "imprecision" or "vagueness" of such definitions is seen as a perfectly appropriate feature of ordinary linguistic usage, in contrast to the artificial precision suggested by rigidly formalistic positivist conceptions of proper language use.

I doubt that there are any terms whose definitions actually fit the ordinary-language model, because I doubt that there are any significant "conceptual truths" at all. I believe however that terms with somewhat similar definitions are commonplace in the special sciences that study complex phenomena. Here's what I think often happens (I formulate the account for monadic property terms; the account is intended to apply in the obvious way to the cases of terms for polyadic relations, magnitudes, etc):

(i) There is a family F of properties that are contingently clustered in nature in the sense that they co-occur in an important number of cases.

(ii) Their co-occurrence is, at least typically, the result of what may be metaphorically (sometimes literally) described as a sort of *homeostasis*. Either the presence of some of the properties in F tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes that tend to maintain the presence of the properties in F, or both.

(iii) The homeostatic clustering of the properties in F is causally important: there are (theoretically or practically) important effects that are produced by a conjoint occurrence of (many of) the properties in F together with (some or all of) the underlying mechanisms in question.

(iv) There is a kind term *t* that is applied to things in which the homeostatic clustering of most of the properties in F occurs.

(v) *t* has no analytic definition; rather all or part of the homeostatic cluster F, together with some or all of the mechanisms that underlie it, provide the natural definition of *t*. The question of just which properties and mechanisms belong in the definition of *t* is an a posteriori question—often a difficult theoretical one.

(vi) Imperfect homeostasis is nomologically possible or actual: some thing may display some but not all of the properties in F; some but not all of the relevant underlying homeostatic mechanisms may be present.

(vii) In such cases, the relative importance of the various properties in F and of the various mechanisms in determining whether the thing falls under *t*—if it can be determined at all—is a theoretical rather than a conceptual issue.

(viii) Moreover, there will be many cases of extensional vagueness that are such that they are not resolvable even given all the relevant facts and all the true theories. There will be things that display some but not all of the properties in F (and/or in which some but not all of the relevant homeostatic mechanisms operate) such that no rational considerations dictate whether or not they are to be classed under *t*, assuming that a dichotomous choice is to be made.

(ix) The causal importance of the homeostatic property cluster F together with the relevant underlying homeostatic mechanisms is such that the kind or property denoted by t is a natural kind (see section 2.1.3).

(x) No refinement of usage that replaces t by a significantly less extensionally vague term will preserve the naturalness of the kind referred to. Any such refinement would either require that we treat as important distinctions that are irrelevant to causal explanation or to induction, or that we ignore similarities that are important in just these ways.

(xi) The homeostatic property cluster that serves to define *t* is not individuated extensionally. Instead, the property cluster is individuated like a (type or token) historical object or process: certain changes over time (or in space) in the property cluster or in the underlying homeostatic mechanisms preserve the identity of the defining cluster. In consequence, the properties that determine the conditions for falling under *t* may vary over time (or space), *while* t *continues to have the same definition.* The historicity of the individuation criterion for the definitional property cluster reflects the explanatory or inductive significance (for the relevant branches of theoretical or practical inquiry) of the historical development of the property cluster and of the causal factors that produce it, and considerations of explanatory and inductive significance determine the appropriate standards of individuation for the property cluster itself. The historicity of the individuation conditions for the property cluster is thus essential for the naturalness of the kind to which t refers.

The paradigm cases of natural kinds—biological species—are examples of homeostatic-cluster kinds. The appropriateness of any particular biological species for induction and explanation in biology depends upon the imperfectly shared and homeostatically related morphological, physiological and behavioral features that characterize its members. The definitional role of mechanisms of homeostasis is reflected in the role of interbreeding in the modern species concept; for sexually reproducing species, the exchange of genetic material between populations is thought by some evolutionary biologists to be essential to the homeostatic unity of the other properties characteristic of the species, and it is thus reflected in the species definition that they propose (see Mayr 1970). The *necessary* indeterminacy in extension of species terms is a consequence of evolutionary theory, as Darwin observed: speciation depends on the existence of populations that are intermediate between the parent species and the emerging one. Any "refinement" of classification that artificially eliminated the resulting indeterminacy in classification would obscure the central fact about speciation upon which the cogency of evolutionary theory depends.

Similarly, the property-cluster and homeostatic mechanisms that define a species must be individuated nonextensionally as a processlike historical entity. It is universally recognized that selection for characters that enhance reproductive isolation from related species is a significant factor in phyletic evolution, and it

is one which necessarily alters over time the species's defining property cluster and homeostatic mechanisms (Mayr 1970).

It follows that a consistently developed scientific realism *predicts* indeterminacy for those natural kind or property terms that refer to complex homeostatic phenomena; such indeterminacy is a necessary consequence of "cutting the world at its (largely mind-independent) joints" (contrast, e.g., Putnam 1983 on "metaphysical realism" and vagueness). Realists' accounts of approximation need not honor bivalence even when partial denotation is not at issue. Similarly, scientific realism predicts the existence of nonextensionally individuated definitional clusters for at least some natural kinds, and thus it treats as legitimate vehicles for the growth of approximate knowledge linguistic practices that would, from a more traditional empiricist perspective, look like diachronic inconsistencies in the standards for the application of such natural kind terms.

Moreover, the homeostatic-cluster conception of definitions may permit a more perspicuous formulation of the central explanatory thesis of scientific realism. I have argued elsewhere (Boyd 1979, 1982, 1983) for an understanding of knowledge and of reference according to which (although I did not use this terminology) the relations 'x knows that y' and 'x refers to y' possess homeostatic property-cluster definitions. I will suggest in section 3.7 that scientific rationality has a homeostatic property-cluster definition and that the realist's explanation for the reliability of scientific methods is best understood as the crucial component in an explanation of the homeostatic unity of scientific rationality.

Not all challenges to realism that arise from considerations about bivalence require in rebuttal an appeal to the possibility of actual bivalence failure. For example, the measurement problem in quantum mechanics is sometimes put by saying that quantum systems lack determinate values of classical magnitudes prior to measurement, and the problem is to characterize the interactions that relieve the indeterminacy with respect to a particular magnitude. Sometimes the alleged indeterminacy prior to measurement is seen as an indication of the failure of realism. Realism is seen as predicting determinacy for (premeasurement) values of classical magnitudes.

In response the realist need not appeal to the possibility of a realist explanation for failures of bivalence. There are two ways of understanding the claim about a physical system that it possesses a determinate value of a classical magnitude, a determinate component of orbital angular momentum, for example. On the first understanding, that claim is understood to incorporate the classical *mis*conception of the logical status of statements about angular momentum, in which case the statement is always false, in however many respects special cases of such statements may also have been usefully approximately true. Alternatively, the statement may be interpreted as attributing to the system an eigenstate of the relevant operator, in which case it need not be false, but it has, depending on the system

in question, some determinate truth value. On careful analysis there is no bivalence failure here to explain.

3. Approximate Truth and Parametric Specification: The Realist's Explanation as Ordinary Science

3.1. The Status of the Realist's Explanation

Recall that the argumentative strategy proposed in section 1.3 calls for us to first assess the evidence for the realist's explanation for the instrumental reliability of scientific methods considered as an ordinary scientific hypothesis. If the realist's explanation appears well confirmed, then there will remain the further and more distinctly philosophical task of determining whether or not, with respect to the realist's explanation itself, it is legitimate to adopt the realist interpretation without which no defense of a realist position in the philosophy of science is forthcoming.

This approach presupposes that the realist's explanation has the form of an ordinary causal explanation in science subject to confirmation or disconfirmation by ordinary scientific standards. Two considerations might suggest that it does not. First, some philosophical explanations of epistemic matters seem noncausal; this is true, for example, of some transcendental explanations and of some "ordinary language" analyses of notions like "evidence," "reliable," "justification," and the like. Secondly, there are ways of thinking of the notions of truth and approximate truth (disquotational analyses, for example) that make them noncausal.

The realist's conception of the epistemology and semantics of scientific theories does not raise any of these problems. Truth is definable from "primitive denotation" (Field 1974), and denotation, on the realist's account, is an epistemic and thus a causal matter; truth is correspondence truth and correspondence is a matter of complex causal interactions. Similarly, to talk of respects of approximation to the truth is to talk of respects of similarity and difference between actual causal situations and certain possible ones. It is philosophically challenging to give a general account of the nature of such comparisons with counterfactual possibilities, but such comparisons are so routine a feature of ordinary causal reasoning in science (including reasoning about the reliability of particular methods) that there is no reason to suppose that they raise difficulties in the present context.

Likewise the explanatory claims of the realist are perfectly ordinary causal claims. Under certain sorts of historical and social circumstances individually and socially held beliefs are said to exhibit a particular causal power—a tendency to generate methods that are (causally) conducive to the establishment of approximate knowledge—when they are in causally relevant ways approximately true. However controversial, this is an ordinary causal thesis about the interactions of scientific communities and the rest of the world. We may reasonably inquire

about how it fares by ordinary scientific standards of evidence. It is to this issue that we now turn our attention.

3.2. Does the History of Science Immediately Refute the Realist's Explanation?

According to the historical objection, the realist's explanation for the reliability of scientific method is refuted by the fact that there have been episodes in the history of science during which methodological practices were successful, but during which the relevant background theories were not, by contemporary standards, approximately true as the realist's explanation requires. The realist's response comes in two parts.

First, the realist's explanation does not require that scientists, even during periods of mature inquiry, be especially good at doing metaphysics. The realist need not necessarily show about any episode in the history of science that the relevant background theories are close to the truth on metaphysical matters. The realist's position is not compromised by any respects of error in earlier background theories that do not undermine her appeal to the specific respects of approximation regarding unobservable phenomena that are crucial to her explanation of the reliability of methods during that episode.

Second, the realist's account of the methods of science predicts that there will be early stages in the history of *any* currently mature science in which the relevant background theories will have been too far from the truth to ensure the sort of reliability of methods that is characteristic of mature sciences. This conclusion is a consequence of the radical contingency in epistemology dictated by the realist explanation for the reliability of scientific methods and, in particular, of the claim that it is, in an epistemically important sense, accidental that the earliest relevantly approximately true theories arise within any scientific discipline. Of course I do not mean that no historical explanations are possible for particular early successes, but only that, according to the realist, the explanation cannot involve appeal to the operation of rational methods with anything like the reliability of the methods of (what from the contemporary point of view are) theoretically more mature stages in the same sciences.

In sum, the realist's explanation is vulnerable to straightforward refutation by the phenomenon of successful science guided by deeply false background theories only if (a) the relevant historical episodes involve the operation of methods that exhibit the profound and routine reliability of judgments of projectability and related matters characteristic of the most mature sciences in the twentieth century, and (b) the respects of falsity in the relevant background theories are not merely deep but such as to preclude an explanation of that reliability by appeal to the respects in which those theories are approximately true. The tendency in recent empiricist philosophy of science towards realism reflects precisely the op-

posite conception: philosophers were tempted by realism precisely because they thought they could see how to offer a realist explanation of the reliability of methodological practices in highly successful science, and they lost their confidence in alternative empiricist "rational reconstructions" of those methods. In any event what I envision as the realist's reply to the historical objection is simply that there aren't actual cases satisfying (a) and (b). Realism is, after all, supposed to be an empirical thesis, and here is one of the empirical claims upon which it rests.

3.3. Triviality, Contrivance, and the Methodology of Parametric Specification

Against the charge of immediate historical falsification, the realist replies by insisting, as the logic of her explanations dictates anyway, that her thesis is that background theories in mature sciences must be seen as approximately true in relevant respects. As we saw in section 1.2, the realist now faces the challenge that her explanations are trivial: that any consistent theory is true in some respects, and that she has offered no general theory of the relative importance of respects of approximate truth. Here the reply is the obvious one that the respects of approximation that are important are those that are required to sustain the realist's distinctive explanation of the reliability of scientific methods and that it is with respect to these that approximations to the truth are claimed. The reply is successful just in case the charge of contrivance can be met: just in case, that is, the realist can argue that, even in the absence of a general context- and episode-neutral account of degrees of approximation, her appeal to respects of approximation appropriate to her own theoretical project does not constitute an ad hoc and thus methodologically inappropriate contrivance.

In order to assess the prospects for the realist's explanations we need to know what distinguishes such contrivances from methodologically appropriate appeals to context-specific specifications of causal variables. Fortunately the question is not esoteric; frequently, especially in the context of historical explanations, we confirm theories that appeal to context-dependent specifications of causal parameters and the methodology for avoiding ad hoc theorizing is well understood. Consider for example explanations in evolutionary theory. There are a variety of possible evolutionary mechanisms – individual selection, kin selection, genetic drift, selection for pleiotropically linked traits, etc. – for no one of which does evolutionary theory provide a context-independent prediction of its influence in any particular evolutionary episode. Moreover, in particular evolutionary episodes several of these factors may operate, and there is no context-independent way of predicting their relative influence. Still, the modern evolutionary explanation for the diversity of life is well confirmed. What methodological principles permit us to treat the explanations provided by evolutionary theory

as appropriate rather than ad hoc, and as appropriate for "inductive inference to the best explanation"?

The answer is pretty clear. What we require of the various individual evolutionary explanations for particular features of living organisms is that they cohere not only with each other but with the independent results of inquiry in the related scientific disciplines: geology, genetics, developmental biology, animal behavior, atmospheric sciences, oceanography, anthropology, etc. This requirement of integration of the various particular explanations into the broader framework of scientific knowledge constitutes our methodological safeguard against the possibility that the apparent explanatory successes of evolutionary theory are reflections of mere contrivance. This pattern is quite general: particular explanations provide evidence for a broader theory whose explanatory resources they exploit just in case theory-dependent evidential standards, including requirements of theoretical integration, dictate the acceptance of the particular explanations, and just in case the success of those individual explanations lends inductive support for the causal claims of the broader theory (Boyd 1985b).

Exactly the same standards apply, of course, to the realist's broad explanation for the reliability of scientific methods. The charge of contrivance is met just in case the realist's explanations for the reliability of methods in particular episodes, including the context-dependent specifications of respects of approximation they contain, are independently supported by scientific evidence, and in particular that they pass the test of coherence with the rest of established scientific theory, and (this is the easier part) just in case these particular realist explanations lend inductive support to the broader realist explanatory picture of scientific epistemology.

3.4. The Local Coherence of Realism

Are the individual realist explanations for the reliability of specific scientific methods well confirmed and do they in particular cohere appropriately with the rest of science? Do they inductively support the general realist conception of the growth of approximate knowledge? At an important level of analysis the answer to both questions must be "obviously yes."

The particular realist explanations of the reliability of methods fall roughly into two categories. In the first category are the theoretical explanations for the reliability of particular measurement and computational procedures and for the reliability of various sorts of controls and other features of the design of experimental and observational studies. In the second category are the theoretical explanations for the reliability of the judgments of projectability which determine the broader outlines of rational experimental and observational inquiry. Explanations in either category may be either static or dialectical. By a static explanation I understand an explanation that explains the reliability of some piece of methodology by appeal to the approximate truth of some theories that have been long es-

tablished at the time of the relevant methodological judgments; dialectical explanations explain the reliability of some novel feature of methodology or of some revision of a previously established methodological practice by appealing to changes in theoretical outlook that bring about a closer approximation to the truth along relevant lines.

At any given time in the history of recent science, individual realist explanations in the first category both static and dialectical look just like well-established pieces of boringly normal science: they are the sorts of claims that are routinely made explicit in the methods sections of papers in the empirical sciences, in which scientists explain the appropriateness of research design. Most explanations of the static sort and almost all of the dialectical ones will embody reference to context-specific degrees and respects of approximation in the current theoretical conception or its immediate predecessors. Those explicit pieces of scientific theorizing are not produced in service of any philosophical or historical project, realist or otherwise. In the better established sciences they are apparently as well confirmed as anything gets; certainly there is no evidence that they fail to cohere with the rest of established science. That, after all, is what made such pieces of ordinary science so disturbing to empiricists. The prospect that they are vulnerable to the contrivance objection is vanishingly remote.

Scientists seem rarely to investigate explicitly the causal question of the reliability of particular projectability judgments *under that description.* They do however offer justifications for their own methodological judgments, critiques of such judgments by others, and proposals for changes in such judgments. Such justifications are made explicit in published papers, in referees' reports, in grant proposals, in the introductory parts of experimental papers, and in theoretical papers and books and the judgments they justify are *in fact* judgments of projectability of the sort to which the realist explanation refers. It is all but the consensus position among students of the logic of scientific inference (e.g., Hanson 1958, Kuhn 1970, Quine 1969a; Van Fraassen 1980) that ordinary scientific standards of reasoning treat these projectability judgments as inductive inferences from background theories, just as realism requires. Here again the justifications in question routinely appeal to context-specific respects of approximation, especially in cases in which they mirror realist explanations of the dialectical sort. There is again no prospect that scientists' reasoning in such cases is contrived to serve a philosophical purpose nor is there any reason to hold that the requirement of coherence with the rest of science is not honored in such reasoning – indeed it is in reasoning of this sort that the requirement of coherence finds its expression in ordinary science!

Here then is the phenomenon of *local coherence*: the explicit and near-explicit findings of ordinary science examined synchronically seem to strongly confirm, if only tacitly, the particular explanations for the reliability of projectability judgments on which the realist's explanatory enterprise rests and they appear to do

so in a way that subjects the context-dependent judgments of relevant respects of approximation which they contain to the appropriate requirement of coherence.

Do the particular realist explanations we are considering, taken together, inductively support the realist conception of scientific epistemology developed in part 2? Here we cannot defer to any particular science except philosophy, but we can observe that the whole tendency to take realism seriously as an alternative to logical empiricism from the mid-1950s on reflects the extremely widespread judgment among philosophers of science that the actual practices of science *appear* to require a realist explanation. I conclude that, if we examine the question *pre*philosophically, there appears to be very good reason to hold that the realist's explanation for the reliability of scientific methods is well confirmed as a scientific hypothesis and in particular, that there is no reason to think that the realist's approach to the problem of parametric specification is any more in doubt than, say, that of the evolutionary biologists who must also rely on specifications not given antecedently by a context-independent formula.

We turn now to the question of what the distinctly philosophical dimension is to the confirmation of the realist's explanatory hypothesis. The elaborate machinery rehearsed in part 2 indicates that a lot is going on philosophically. Some of it is relevant only to the question of circularity, but much is relevant also to a defense of realism as a scientific thesis in the methodological climate created by the philosophical disputes over realism.

3.5. What's Distinctly Philosophical? I: Diachronic Patterns of Inference and of Language Use

Central to the realist's abductive argument for realism is the claim that no alternative exists that adequately explains the reliability of scientific methods or justifies their use. It is possible to *imagine* that a case along these lines for realism—or at any rate against the verificationist insistence that knowledge of unobservables is impossible—could be made by the synchronic examination of only a few episodes in the history of science for which only realist explanations and justifications seem available. Nevertheless the deep plausibility of empiricist epistemological principles, especially the evidential indistinguishability thesis, is so great that it is doubtful that realism about a few isolated cases would, even as a scientific hypothesis, be rationally acceptable. Instead the plausibility of any individual realist explanation seems to rest upon diachronic considerations that provide additional and crucial support for the general realist explanation of the reliability of scientific methods. In effect, the role of these diachronic considerations is to establish that the individual synchronic-realist explanations can be coherently integrated into a scientifically acceptable historical conception of the reliability of scientific methodology.

In particular there are two patterns in the history of science whose recognition

is a distinctive contribution of philosophers and historians in making the case for the realist's explanations. In the first place, there is the utterly commonplace phenomenon of *mutual ratification* between consecutive stages in the development of scientific disciplines. It is routine in the case of theoretical innovations that (a) the new and innovative theoretical proposal is such that the only justification scientists have for accepting it, given the relevant evidence, is that it resolves some scientific problem or question *while preserving certain key features of the earlier theoretical conceptions;* and (b) the new proposal ratifies the earlier conception as approximately true in just those respects that justify their role in its own acceptance. Moreover the patterns of mutual ratification are characteristically seen to be *retrospectively sustained*: although later theoretical innovations typically require a revision in our estimates of the degrees and respects of approximation of both the earlier innovative proposals and their predecessors, the initially discernable relation of mutual ratification is typically sustained as a very good first approximation to the evidentially and methodologically important relations between the innovation and its predecessor theories. It is the ubiquity of this sort of *retrospectively sustained mutual ratification* and the difficulty in "rationally reconstructing" it away with respect to the justification of theoretical innovations that has made the case for realism so plausible.

A second pattern concerns the use of scientific language. The realist conception of projectability requires that the categories that scientists employ in formulating general laws and causal claims typically reflect underlying causal structures rather than conventionally specified nominal essences, and many of the changes in classificatory practice for which individual realist explanations are forthcoming seem to indicate an attempt to obtain a fit between categories and causal structure. It is essential to the case for realism that this pattern in scientific language use be sustained: that the diachronic linguistic behavior of scientists involves an apparent disposition to take the definitions of scientific kinds, relations, magnitudes, etc., to be revisable in the light of new data and new theoretical developments. Thus the identification of just such a pattern of *apparent essentialism* in the actual linguistic practices in scientific communities is an important distinctly philosophical contribution to the case for the realist's explanation of the reliability of scientific method.

3.6. What's Distinctly Philosophical? II: Epistemological, Metaphysical, and Semantic Underpinnings

The ubiquitous patterns of retrospectively sustained mutual ratification and apparent essentialism constitute philosophical reasons to accept the realist's explanation, and the recognition of those patterns was a central factor in the emergence of contemporary scientific realism. Still, their effect would not have been so significant were it not for more theoretical attempts to understand their philosophical

import. The obvious examples here are causal theories of reference and associated naturalistic conceptions of definition. Had it not proven possible to articulate these distinctly philosophical theories, then it might have been rational to hold that the apparently rational theory-and-evidence-driven revision of definitions in science was only apparent, or only apparently rational. The initial case for the realist explanation would have been crucially undermined.

Analogous considerations hold for the epistemological dimension. Both the realist explanations for the reliability of scientific methods in particular cases and the view that the ubiquity of the pattern of mutual ratification supports the broader realist explanation entail that evidential considerations in science are deeply theory dependent. Were it not possible to provide a realist epistemological framework that incorporates this conclusion—and in particular were it not possible to articulate that framework so as to refute the evidential indistinguishability thesis and make palatable the consequent abandonment of foundationalism—then it would have not been rational to take either the particular explanations or the pattern of mutual ratification as significant support for the realist explanation. Thus the development of a nonfoundationalist realist treatment of projectability judgments and the incorporation of that treatment into an independently developing tradition of nonfoundationalist naturalism in epistemology proves to have been essential for the rational acceptance of the realist explanation.

On to metaphysics. The causal theory of reference and the naturalistic conceptions within epistemology with which realist anti-foundationalism can be profitably assimilated all appear to reflect a distinctly non-Humean conception of causal relations. The cogency of these fundamental elements in the defense of the realist's explanation depend therefore (at least prima facie) on the successful articulation of a non-Humean conception of causation (e.g., Boyd 1985b; Mackie 1974; Shoemaker 1980).

Acceptance of the realist's explanation as a scientific theory does not entail the acceptance of scientific realism, since the realist's explanation might itself be interpreted nonrealistically. What I have been suggesting is that nevertheless the realist's explanation is sufficiently novel in its apparent epistemological, semantic, and metaphysical implications that the articulation of just the sort of broader realistic and naturalistic conceptions of (scientific and other) knowledge, of language, and of metaphysics indicated in part 2 is essential for the defense of that explanation.

I think that the picture just presented captures the current case that the realist's explanation for the reliability of scientific methods is a well-confirmed scientific theory, context-dependent specifications of respects of approximation notwithstanding. An even broader philosophical setting for that case is available if we exploit the distinctly naturalistic conception of homeostatic property-cluster definitions outlined in section 2.5

3.7. Realism and the Homeostatic Character of Scientific Rationality

I argued in section 2.5 that lots of natural kinds, properties, etc. possess homeostatic property-cluster definitions, and I suggested that knowledge and reference are among them. I want now to suggest a similar homeostatic cluster treatment of scientific rationality itself. Ordinarily we think of scientific rationality as being exhibited in two different features of the practice of science: the high level of deliberative rationality in the reasoning of researchers, and the spectacular successes of scientific research in understanding and predicting natural phenomena. If foundationalism is mistaken, as it surely seems to be, then the first of these features does not logically entail the second, and the realist explanation may be thought of as explaining why (and when) they reliably co-occur. Here is a kind of homeostasis of the two distinct components of scientific rationality.

Once it is recognized that this co-occurrence is a causal matter, then it is easy to see that at a finer level of analysis there is a family of similar sorts of co-occurrences requiring explanation. The methodological norms in a particular subdiscipline are set not only by the background theoretical findings in that subdiscipline but as well by findings from other subdisciplines and from quite different disciplines altogether. That the methodological norms determined by such a wide range of theories should be unified enough to be a practical guide to successful scientific research requires explanation. Why aren't the resulting methodological norms characteristically irreconcilably conflicting, for instance?

Similarly, scientists working largely independently within different disciplines frequently converge on the same solution to a problem they may not have recognized that they have in common. Why should this happen? Likewise, it often happens that largely independently developing disciplines become ripe for interdisciplinary work, and their largely independently developed theories and methodologies prove (with some difficult but not impossible negotiation) to be integrable. Why is this so frequently possible?

What I propose is that we think of scientific rationality as being defined by the homeostasis of all of these various components of scientific practice and that we should think of the realist explanation of the coincidence of deliberative rationality and theoretical and empirical success as the first step toward a more general realist explanation of the relevant homeostasis. It is even possible that this project could be extended fruitfully to incorporate a naturalistic conception of moral rationality (Boyd 1988; Brink 1984, 1989; Miller 1984b; Railton 1986; Sturgeon 1984a, 1984b).

If the proposal of the present section were to prove successful it would prima facie provide further support for the realist explanation and for the philosophical naturalism that underwrites it. However, we still need to know whether the realist's explanation should itself be understood realistically or whether instead,

as the circularity objection suggests, that would simply beg the question against the anti-realist.

4. Meeting the Circularity Objection

4.1. Circularity and Philosophical Packages

According to the circularity objection, the realist's explanation for the success of scientific methods, even if well confirmed, cannot without begging the question be interpreted realistically and thus cannot without circularity be treated as confirming scientific realism. The problem posed by this objection is faced not only by the particular defense of realism under consideration but by almost any plausible defense of scientific realism.

The reason is simple: in all but the most trivial cases the defense of realism regarding one or more theories or traditions will require the defense of a theory of epistemic contact that spells out the sort of epistemically relevant causal relations that are supposed to obtain between the subject matter of the theories or traditions and the behavior of the relevant inquirers. Because the realist thesis and the theory of epistemic contact that supports it are causal theses, their confirmation will always depend upon the confirmation of theories (or, for very simple cases, commonplaces) about the causal powers of the entities that are the putative subject matter of the theory or tradition in question. The confirmation of specific theories of epistemic contact will, in turn, depend in part upon theoretical considerations grounded in the best available theories of the relevant subject matter. Such theories will be a vital background assumption against which the evidence for the realist thesis is judged. As we have seen, the theory of epistemic contact, and (thus) the theories upon which its confirmation in turn depends, will themselves have to be understood realistically if they are to help to validate the realist thesis itself. But of course these theories will, in any plausible case, be subject to the same anti-realist assessments as the theory or tradition about which realism is initially in question. Indeed if that theory is a well-established contemporary theory it may *itself* provide the foundations for the relevant theory of epistemic contact! Is this not a point at which the defense of realism begs the question against anti-realists?

Here the answer is "no." If theories of epistemic contact by themselves constituted the sole argument of the realist against anti-realism, if for example the *sole* argument in favor of realism in atomic theory consisted of the articulation of an apparently well-confirmed theory of epistemic contact between scientists and atoms, their properties and their constituent parts, then the question would indeed be begged by the assumption that that theory itself should be understood realistically. The actual role of theories of epistemic contact is quite different.

The issue of realism arises in the form we have been discussing only in the

case of a theory or tradition of inquiry about which there is a prima facie case that it possess a theory-independent (even if unobservable) subject matter. The prima facie case for realism will thus rest upon the apparent confirmation of a (realistically understood) theory of epistemic contact. In the special case of realism defended along the lines proposed here, that theory of contact is the one embodied in the realist's explanation for the reliability of scientific methods. The defense of realism, however, depends not upon the theory of epistemic contact *alone* but upon the ability of realists to incorporate suitably elaborated versions of it into an epistemological, semantic and metaphysical conception of the theory or tradition in question (a *philosophical package*) that is superior to that those available to defender of the various anti-realist conceptions.

Thus, for example, the defense of realism regarding the tradition of atomic theory depends upon the best-confirmed atomic theories providing the basis for an apparently realistic theory of epistemic contact, but it depends as well upon additional, more explicitly philosophical considerations, which legitimize the realist treatment of such a theory. On the version of scientific realism presented here, these additional considerations are of two sorts. First, it is argued that only on a realist construal of atomic theory generally, and of the relevant theory of epistemic contact in particular, is it possible to avoid skepticism about the possibility of purely instrumental knowledge in physics and chemistry: knowledge of a sort acknowledged by empiricists and constructivists as well as by realists. Secondly, it is argued that the picture that emerges from a realist treatment of atomic theory is consonant in its departures from foundationalism and in its treatment of scientific language with other quite independently defensible developments in epistemology and semantic theory.

In such a dialectical setting, the dependence of the realist theories of epistemic contact upon a realist understanding of the theory or tradition in question (or of some closely related theory or tradition) need not constitute begging the question against the anti-realist. Fairness to the case for realism requires that realism be understood in a context provided by a realist interpretation of the apparently best-confirmed realist theories of epistemic contact and of the apparently best-confirmed substantive theories of the alleged (theory-independent) subject matter in question.

Importantly, just the same understanding of the issue is required by fairness to the case *against* realism. Both the empiricists' and the constructivists' anti-realist arguments depend upon the assumption that the realist accepts the prevailing theoretical conception and its associated methodology. The realist is understood to take the properties of the putative socially unconstructed referents of the terms of a theory or tradition to be, at least approximately, those required by (a realist understanding of) the apparently best-confirmed theories of the presumed subject matter and to accept the methodology dictated by them as approximately reliable. On those assumptions (but not without them) the empiricist can reason

that the realist's position commits her to the possibility of investigating the properties of unobservable phenomena, and thus to an epistemological position against which the empiricist has very powerful arguments.

The constructivist anti-realist similarly assumes that the realist accepts a realist interpretation of the prevailing theoretical and methodological conceptions. Only on such an understanding is it clear that the realist is committed to the possibility of investigating a theory-independent reality using theory-dependent methods—just the possibility that the constructivist critique of realism rejects. Thus an adequate treatment of the controversy between realists and either of their standard opponents requires that we accept that the philosophical package offered in defense of realism contains the apparently best-confirmed theories of the alleged subject matter, realistically understood, and in particular that it be understood as incorporating an associated realistically understood conception of epistemic contact.

Once it is seen that no question is begged against the anti-realist by adopting a realist interpretation of the realist's explanation for the reliability of scientific methodology, we are left with the question: Suppose that the realist's explanation is well confirmed, then why would a realist philosophical package incorporating a realist version of that explanation be superior to an empiricist package incorporating the explanation instrumentally interpreted or to a constructivist package incorporating the realist's explanation understood as a piece of social construction? My main aim in the present essay is to show that the realist's appeal to a distinctively realist explanation for the growth of approximate knowledge, incorporating an appropriate context-and-episode-dependent account of relevant respects of approximation, does not involve any triviality, contrivance, or begging of the question—not to finish once and for all the task of defending realism. I will therefore indicate only briefly the outlines of the considerations that seem to me to justify a preference for the realist package over the two alternatives in question.

4.2. Against the Empiricist Package

The key argument for scientific realism according to the program presented here is that realism as a scientific hypothesis presents the only scientifically acceptable explanation for the realiability of scientific methods. The empiricist might be unimpressed by the demand for explanation in this case (Fine 1984, Van Fraassen 1980). Still the realist can also argue that accepting the realist explanation provides as well the only justification we have for accepting the instrumental findings of science (Boyd 1983, 1985a). One possible empiricist response is that we can justify accepting the inductive deliverances of an apparently realistic scientific method as a result of the second-order induction about induction whose conclusion is that reasoning like a realist in science is instrumentally reliable.

Since this conclusion is only about observables, the empiricist can accept it and employ it to justify accepting currently accepted theories as empirically adequate.

Against this rebuttal I have argued (Boyd 1983, 1985a) that the induction in question is demonstrably just as theory dependent as any other in science and is thus unavailable to the empiricist who is adopting the proposed strategy. Here is a possible reply: We justify the second-order induction by a third-order induction about inductions about induction, the third-order induction by appeal to a fourth-order induction, etc. For the nth case the justification for the relevant projectability judgments is provided not by apparently realistic theoretical considerations but by the n+first-order induction.

If I am right this last response is what the incorporation of the realist's explanation into an empiricist philosophical package would require if that package were to provide any even remotely plausible account of the justification of (instrumental) scientific knowledge. I claim that the resulting philosophical package would prove to be only remotely plausible in consequence. Here we have not just infinite regress but infinite ascent: each level of inductive inference is justified by appeal to a more abstract and problematical level of inductive inference. Given that the realist's package already incorporates an alternative, less speculative, and independently justified naturalistic epistemology I predict that it will prove superior.

4.3. Against the Constructivist Package

Response to the sort of constructivist philosophical package that might be constructed so as to include the realist's explanation for the reliability of scientific methods is substantially more difficult. Constructivism is a richer philosophical program than empiricism, and at the same time it incorporates features (often just the ones that add to its richness) whose consistency is disputable. Rather than even beginning to sort out all of the issues that a thoroughgoing realist response to constructivism would have to address, I will just indicate briefly how two quite standard objections to constructivism might be brought to bear on the proposed package.

In the first place, any adequate philosophical package will have to incorporate versions of most of the apparently best-established scientific and methodological findings. The suggestion outlined in section 2.4, that the establishment of social institutions and linguistic conventions does not contribute noncausally to the causal powers of the objects studied by participants in those institutions and conventions, has very deep roots in quite diverse features of our understanding both of causation and of social phenomena. Thus any constructivist philosophical package will be prima facie vulnerable at any point at which it incorporates a distinctly constructivist conception of the social construction of causal relations. The proposed constructivist package would incorporate this doubtful feature into its

version of the naturalistic account of the reliability of scientific methods and thus in to the very center of its basic epistemology. It is doubtful therefore that the proposed package will afford as satisfactory a treatment of absolutely central epistemological issues as its realist rivals.

A second standard objection to constructivism is that the historical fact of anomalies indicates that the world scientists study does not have a structure logically, socially, or conceptually determined by the paradigms or theories they accept. It is beyond the scope of this essay to examine the variants on this objection and the range of possible replies. It cannot be doubted however that it does pose a serious challenge to the acceptability of any constructivist package. Since there are anomalies in methodological matters that exactly parallel those in theoretical matters, the incorporation of a doctrine of social construction of the reliability of scientific method seems hardly to strengthen the constructivist philosophical package.

I conclude that the resources exist for a spirited defense of a realist philosophical package against empiricist and constructivist alternatives, and in particular that the incorporation of a realist interpretation of the realist's explanation of the reliability of scientific methodology strengthens rather than (as the circularity challenge suggests) weakens the realist package.

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