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Constructivism, Realism, and Philosophical Method

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1. INTRODUCTION

1.1. Constructivism and Realism

Post-positivist philosophy of science has gone in three directions: toward more sophisticated versions of empiricism (e.g., van Fraassen 1980), toward social constructivism (e.g., Kuhn 1970), and toward scientific realism (Boyd 1983, 1990a; Putnam 1972, 1975a, 1975b). Defenders of the latter positions affirm, while sophisticated empiricists continue the tradition of positivists by denying, that the typical product of successful scientific research embodies knowledge of unobservable phenomena—that scientists routinely do "metaphysics" in the positivists' pejorative sense of the term. Realists and constructivists differ in that the former hold, while the latter deny, that the phenomena studied by scientists exist and have the properties they do independently of our adoption of theories, conceptual frameworks, or paradigms. Thus, while realism and constructivism are both antiempiricist positions, constructivism shares with later positivism a tendency largely absent from realism of treating large-scale theoretical claims in science as in some important sense conventional. In the present essay I will be concerned with the dispute between constructivism and realism. I have three aims: to articulate the best arguments for realism against sophisticated versions of constructivism, to explore the implications of those arguments for our understanding of the issue of conventionality generally, and to explore some broader issues of philosophical method which are raised by the dispute between realists and constructivists.

1.2. Versions of Constructivism

The target of my arguments will be constructivist conceptions of science of the sort whose influence was guaranteed by Kuhn's *The Structure of Scientific*

Revolutions (1970). The general slogan "Science is the social construction of reality" and similar expressions of constructivist sentiment have a variety of interpretations, more than one of them suggested by Kuhn's own insights into scientific practice, and I will be concerned here with just one among them. Sometimes when students of science portray science as the social construction of reality, they mean to emphasize the extent to which the actual production of scientific texts, instruments, institutions, and so on is a social enterprise subject to the same sorts of analyses—political, sociological, literary, anthropological, and so on—as any other social enterprise whose output includes texts or other cultural artifacts (let us call this doctrine science-as-social-practice constructivism, SSP constructivism). Sometimes they mean to offer a debunking critique as well: perhaps that the content of scientific theories is determined almost exclusively by facts about power both within the scientific community itself and within the broader society (let us call this debunking constructivism).

The constructivism with which I will be concerned here (let us call it "Neo-Kantian constructivism," "N-K constructivism," by way of indicating something of its motivation but without prejudice regarding questions of Kant scholarship) is different. According to Neo-Kantian constructivism, consideration of, for example, the theory-dependence of scientific observation and methods, or the existence of mutually irreducible conceptual schemes or of mutually incommensurable paradigms in the sciences, indicates that there is something misleading, but not literally false, about the claim that in scientific work scientists *discover* what *the world* is like. The implicatures of that way of describing science reflect a conception according to which the structures which scientists discover are, independently of any scientific activity, "out there" in "the world" available for "discovery." This conception the Neo-Kantian constructivist denies: in some deep sense the structures studied by scientists are imposed on the world, in the sense of being reflections of the conceptual schemes they employ.

But according to N-K constructivists, it would be misleading (indeed, a straight-out error) to say, with a certain debunking tradition, that the internal politics of the scientific community or external pressures *and not the world* determine the content of scientific theories. While the phenomena of political determination identified by debunking constructivists sometimes determine the content of scientific theories, the sort of social construction which N-K constructivists emphasize is supposed to be a universal feature of scientific investigation, and it is not appropriately described by denying that "the way the world is" can determine the content of scientific theories. Two considerations indicate to N-K constructivists that scientific theories are often brought into approximate conformity with "the way the world is." First, the successful establishment of a scientific research tradition (or "paradigm") requires the cooperation of nature: research traditions are viable only if they allow their participants to succeed in actual experimental practice by, for example, predicting unexpected results or predicting expected ones with increasing numerical precision.

Just as important is the N-K constructivists' more general (and "Kantian") epistemological conception according to which social construction of reality is a necessary condition for systematic investigation. It is a consequence of the alleged ubiquity of social construction that the socially constructed reality which scientists study is as real as studiable things can get. There is no more real set of things in themselves for us to study, and thus no debunking of scientific investigation is entailed by the insistence that the reality scientists study is socially constructed.

Each of the three (or more) conceptions of science as a matter of social construction is worthy of serious elaboration and criticism. I focus here on N-K constructivism for two reasons. In the first place, it seems right to think of logical empiricism, scientific realism, and social constructivism as competing conceptions of the nature and of the limits of scientific knowledge, corresponding to broader empiricist, realist, and "Kantian" traditions in epistemology and metaphysics. If logical empiricism and scientific realism are thought of as theses about genuine knowledge in science (and not, for example, about how frequently such knowledge is produced by actual institutionalized scientific practice), then each is compatible with SSP constructivism and each is compatible with all but the most extreme version of debunking constructivism. That is, each is compatible with any versions of debunking constructivism which do not deny that some genuine scientific knowledge—in the sense of beliefs controlled in a suitable way by the way things actually are—is *possible*, however rarely (if at all) it is produced by institutionalized scientific practice. By contrast, both logical empiricism and scientific realism are incompatible with N-K constructivism, and it is reasonable to see N-K constructivism as the manifestation of a "Kantian" epistemological and metaphysical conception in contemporary philosophy of science. It is the version of social constructivism we want to look at if we are to see how significant general philosophical tendencies are played out in the philosophy of science.

There is another reason for focusing on N-K constructivism. One feature of the literature, both within professional analytic philosophy of science and in related areas of history, sociology, and literary theory, has been a tendency to conflate the three conceptions of social construction. For example, especially in the literature outside professional philosophy of science, it is often taken for granted that a demonstration of SSP constructivism precludes a realist or empiricist interpretation in favor of debunking constructivism or N-K constructivism.

There is likewise a tendency, in the professional philosophical literature as well as in the literature in other intellectual disciplines concerned with science as an object of study, to fail to distinguish clearly between debunking and N-K constructivism. Each of these tendencies, it seems to me, makes it harder for researchers to assess the merits of the three different doctrines. One of the consequences, I believe, is that a central problem facing debunking constructivists has been inadequately examined. It is, moreover, a problem whose solution at least arguably depends on an assessment of the philosophical merits of N-K constructivism.

Here is the problem: For all but the most extreme debunking constructivist it will seem important to distinguish between those cases in which the actual structure of the world plays some important role in determining the content of scientific doctrines, so that some genuine knowledge is achieved, and those cases in which it does not. If a realist (or, for that matter, an empiricist) conception of scientific knowledge is appropriate, the intended contrast can be straightforwardly defined. If, in contrast, an N-K constructivist conception of genuine scientific knowledge is correct, the moderate debunking constructivist will need to provide some formulation of the distinction between those episodes of "social construction of reality" in which the relevant social processes of consensus formation in science are to be thought of as *really* constructing reality and those episodes in which the establishment of consensus is to be debunked.

This problem is an especially acute one for the many thinkers who seem to have adopted both debunking and N-K constructivism in response to a recognition of the ideological role frequently played by scientific doctrines and the associated ideological determination of their content. If episodes of consensus formation in science cannot be so nicely categorized, then such thinkers run the serious risk of having, in consequence of their N-K constructivism, to treat as true the findings of just those episodes of theory construction which they otherwise seek to debunk.

I am inclined to doubt that a principled solution to this problem is available to the N-K constructivist. I am thus concerned to provide an adequate justification for the adoption of a realist rather than an N-K constructivist conception of genuine scientific knowledge, not merely to advance our understanding of foundational issues in the epistemology of science but to provide a basis for drawing the required distinction between genuine scientific knowledge and the sort of social construction worthy of debunking. It seems to me that the insights of many debunking constructivists are too important—politically and morally as well as intellectually—to be muddled by N-K constructivism. In "socially constructing" racial differences, nineteenth-century biologists did not construct a world in which those of African descent are biologically suited to a subordinate role, however much they constructed theories to that effect, nor have their latter-day followers done so—any more than those same biologists (or we) have socially constructed a world in which the place of women is determined by biological necessity.

1.3. The Need for a New Realist Critique of Constructivism

It might seem that mounting a defense of realism against N-K constructivism is not timely. After all, the articulation of distinctly realistic and naturalistic conceptions of reference and of kind definitions (e.g., Kripke 1972, Putnam 1975a) has significantly undermined the N-K constructivist arguments of Kuhn and Hanson, as has the articulation of distinctly realistic accounts of the appropriateness of theory-dependent methods (e.g., Putnam 1972, Boyd 1983). Arguably the realist's concern should now be with SSP and debunking constructivism and her task should be to show that the plausible versions of each of these positions are compatible with (and perhaps even entail) a realist conception of genuine knowledge.

I agree about the importance of the latter task, but it seems to me that there are reasons to believe that the available realist critiques of N-K constructivism are inadequate. In recent years "pluralist" or "relativist" conceptions closely related to the social constructivism of Kuhn and Hanson (e.g., Goodman 1978) have grown in influence, and I am inclined to think that these conceptions and other sophisticated versions of N-K constructivism are not adequately addressed by the extant realist critiques of views like those of Hanson and Kuhn. In brief, what I will argue is that there are plausible versions of constructivism which are not committed to the semantic or methodological conceptions to which anticonstructivist arguments grounded in naturalistic theories of definition and reference provide an adequate rebuttal, and whose epistemological and metaphysical claims are not fully rebutted by realist accounts of theory-dependent methods. What these versions of constructivism have in common is that they reflect ways of understanding conventionality which are more complex—and more plausible—than those which underlie earlier debates about constructivism. I will put forward here what I think to be the strongest arguments against the more plausible versions of constructivism. While these arguments have not, so far as I know, been made so fully explicit as I intend to make them, they do, I hope, capture the considerations that incline many philosophers of science to reject constructivism without fully exploring its more sophisticated variants.

The arguments in question are methodologically interesting—at least I find them interesting—because, while not in any obvious way entailing a naturalistic conception of philosophical method, they involve a certain kind of a posteriori scientific assessment of constructivist claims. I will explicate the relevant sort of scientific assessment and compare its operation with that reflected in the traditional logical-empiricist concern to hold philosophical accounts subject to the requirement that they offer a "rational reconstruction" of actual science. One outcome of this investigation is the articulation of a conception of the dialectics of philosophical argumentation which indicates how distinctly philosophical considerations properly interact with considerations arising from other disciplines.

2. CLASSICAL NEO-KANTIAN CONSTRUCTIVISM

2.1. *Two and a Half Traditional Arguments for Constructivism*

In this and succeeding sections of part 2, I propose to lay out and evaluate the classical arguments for and against N-K constructivism—those arguments which have commanded the interest of philosophers from the first articulation of contemporary N-K approaches by Hanson and Kuhn. Although I will cite the work of many of the key figures, I do not intend to be providing a historical survey of arguments for and against constructivism. Instead, I will try to identify the best and most plausible features of the arguments and considerations, explicit or tacit, that have influenced philosophers' views on these matters. I turn first to the classical arguments for constructivism.

All of the traditional arguments for (N-K) constructivism rest on the important observation that all of the fundamental methods of science, from the most basic observational procedures to the most elaborate standards for the assessment of evidence, are deeply and irretrievably theory-dependent. They differ in the extent to which they depend as well on special alleged historical consequences of theory-dependence. The following typology sorts the traditional arguments into two and a half basic categories.

The Basic Epistemological Argument from Theory-Dependence. Into this category fall the various arguments that justify an N-K constructivist conception of scientific knowledge by appealing to the fact of deep theory-dependence of scientific methods and exploring its epistemological implications. These are the key Neo-*Kantian* epistemological arguments for constructivism. They reason that the methods of actual science are so deeply theory-dependent that the only sort of reality for whose discovery they would be appropriate would be a reality partly constituted by the theoretical tradition within which scientific research takes place. Since, in my view, it is important not to underestimate the force of such arguments, I want to indicate something of the origins of their persuasive force.

In the first place, it is important to see that the methods of scientific research are not merely deeply theory-dependent, they appear to be such that their application would not be rationally justifiable except on the assumption of the truth or the approximate truth of the theories upon which they depend. Thus, insofar as we take (some) scientific research to be a basically rational activity, we, like the scientists who engage in that research, must be taking for granted the (perhaps approximate) truth of the theories that underwrite their methods.

Second, the theory-dependence of scientific methods is not somehow restricted to *derived* rather than *fundamental* methodological principles. It is, of course, no surprise that in developed sciences some (or most) of the methods scientists employ are justified by appeal to features of previously established

theories. It might seem, however, that if the development and confirmation of theories in the relevant scientific traditions are fully explored, then it will turn out to be true, either in fact or in an appropriate rational reconstruction, that the traditions can be seen as having been first established by the application of theory-independent fundamental *methods to theory-independent observations* and as subsequently developing by the application at any given time of only those theory-dependent methods ratified by earlier theoretical discoveries. Were such a story true, then "in principle" we could take inductive inferences in sciences as governed by the underlying theory-independent methodological principles, treating theory-dependent methods somewhat on the model of derived inference rules in deductive logic.

What Hanson, (especially) Kuhn, and others have shown is that this picture cannot be sustained. When recognizably scientific methods emerge within a discipline, they emerge as part of a package that includes theoretical conceptions necessary to ratify them, rather than as initially theory-independent principles that ground the initial adoption of theoretical conceptions.

Moreover, not only are methodological principles deeply dependent on theories, the theories they depend on are often deep. I mean by that that the theoretical presuppositions of scientific methods are not, generally, almost unproblematical, if still a posteriori, propositions like "like causes have like effects," "every event has a cause," or "there is order in nature." Instead, the methods within a scientific discipline are typically grounded in foundational theoretical principles peculiar to that discipline's special concerns. As Kuhn suggests, scientists' judgments about the nature of the problems to be solved and the forms of acceptable solutions (that is, their judgments of projectibility) are typically determined by a metaphysical picture of what the world they study is ultimately like.

In consequence, the methodology of science will seem, with respect to the testing of fundamental assumptions at least, disturbingly *circular*. We may make precise both the nature and depth of the circularity, and the seriousness of the disturbance it creates, by examining with some care the recent fate of foundationalist conceptions of knowledge. Modern epistemology has been largely dominated by positions that can be characterized as "foundationalist": all knowledge is seen as ultimately grounded in certain foundational beliefs that have an epistemically privileged position—they are a priori, or self-warranting, or incorrigible, or something of the sort. Other true beliefs are instances of knowledge only if they can be justified by appeals to foundational knowledge. Similarly, the basic inferential principles that are legitimate for justifying nonfoundational knowledge claims can themselves be shown a priori to be rational.

We may fruitfully think of foundationalism as consisting of two parts, *premise foundationalism*, which holds that all knowledge is justifiable from a core of epistemically privileged foundational beliefs, and *inference-rule foundationalism*, which holds that the principles of justifiable inference are ultimately reducible to inferential principles that are a priori justifiable.

Recent works in naturalistic epistemology (see, e.g., Armstrong 1973; Goldman 1967, 1976; Quine 1969a, 1969b) indicate that foundationalism cannot be entirely correct. For the crucial case of perceptual knowledge, there seem to be (in typical cases at least) neither premises (foundational or otherwise) nor inferences; instead perceptual knowledge obtains when perceptual beliefs are produced by epistemically reliable mechanisms. Even if this analysis is challenged and it is insisted that justification of some sort is crucial in cases of perceptual knowledge, it is clear that there will be nothing like the traditional foundationalist's vision of knowledge of the external world grounded in premises as secure as, for example, those about sense data, and justified by appeal to a priori defensible inference principles.

Even where premises and inferences are unproblematically relevant, the notion of justification does not appear to be as epistemically central as traditional foundationalists thought: it seems to be the reliable production of belief that distinguishes cases of knowledge from other cases of true belief. Justification appears to be relevant because of the causal role which the seeking and giving of justifications play in reliable belief production (or regulation; see Boyd 1982).

Despite these setbacks, it might seem that some appropriate version of foundationalism provides us with an approximately correct picture of knowledge. If we think of ordinary perceptual beliefs, obtained under appropriate conditions, as suitably privileged, for example, and if we tolerate inference rules whose presuppositions only "the skeptic" would challenge, then a modest foundationalism might seem to capture pretty well the intuitive notion that knowledge claims must be noncircularly or non-question-beggingly defensible, however poorly it underwrites the refutation of skepticism.

We are now in a position to see just how and why the "circularity" with respect to fundamental principles unearthed by constructivists is so disturbing. What it suggests is that even modest foundationalism fails, even as a good first approximation to a theory of knowledge, not because the most basic available premises are insufficiently privileged but because inference-rule foundationalism appears to be profoundly mistaken. The basic inferential principles that are reflected in scientific methodology rest on deep and sometimes controversial theoretical principles which someone could reject—and which some have rejected—without the slightest hint of philosophical skepticism.

Now foundationalism is an especially plausible philosophical position, especially if it is understood in the proposed modest way and as an analysis of the notion of non-question-begging justification rather than as part of a scheme for refuting the skeptic. Thus the discovery of the deep theory-dependence of

methods appears to threaten an especially plausible and central part of our conception of knowledge.

It poses a closely related problem as well. We are used to thinking of the establishment of the first successful research traditions within the various scientific disciplines as, in the first instance, insofar as internal factors are concerned, the result of the adoption of appropriate scientific methods. It is the reliability of those methods which we expect will explain the successes of researchers in obtaining an approximately correct theoretical picture of the relevant phenomena. This explanation is apparently precluded by a recognition of the deep theory-dependence of scientific methods. Indeed, it seems to get things more or less backward. Since methods possessing the reliability characteristic of those of recent successful science rest upon approximate theoretical knowledge rather than on a priori or commonsensical principles, the emergence of epistemically successful scientific methods must have depended upon the logically, epistemically, and historically contingent emergence of a relevantly approximately true theoretical tradition rather than vice versa. 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 which itself is theory-independent. There is no such methodology.

Thus the theory-dependence of methods poses the *start-up problem*—how are we to explain the first emergence of approximately true theories within a research tradition, and thus the emergence of the reliable methods they determine, if not by reference to the prior establishment of noncontingently reliable methods? What seems to be indicated is a sort of *radical contingency* in the epistemology of science: not only does the reliability of scientific methods rest on highly contingent presuppositions but it is, in a philosophically important (*and nonskeptical*) sense, an *accident* that in the early stages of a successful scientific tradition relevantly approximately true theories and the associated reliable methods emerge at all (for further discussion see Boyd 1982, 1990a).

Modest foundationalism is extremely plausible, and solving the start-up problem by appealing largely to accident or luck seems implausible. No doubt these facts explain part of the attractiveness of debunking constructivism: if scientific methods are circular in such a way that scientific knowledge claims cannot be accepted without rejecting modest foundationalism, and without treating the first systematic successes of scientific research as accidents, then so much the worse for scientific knowledge claims.

What is important for our purposes is that the N-K constructivist interpretation of scientific knowledge to a significant extent ameliorates these difficulties and restores the possibility of a modest foundationalism. If basic laws of nature are to be seen as, in some deep sense, imposed on nature by our social conventions and practices, then the most basic theory-dependent methods may well be justified, if not a priori, then at any rate by appeal to principles that have a distinctly privileged epistemic standing. Other more specific methods that depend on plainly a posteriori theoretical considerations might then be treated as reflecting derived inference rules just as the foundationalist project requires. Similarly, the start-up problem will seem somewhat more tractable: at least part of the explanation of how the first successfully established paradigmatic theories came to approximate the truth about natural phenomena will lie in the fact that the acceptance of those theories *constitutes* the reality of the phenomena in question.

It is these considerations which, I suggest, make it plausible that the theory-dependence of scientific methods is such that if they are to be understood as discovery procedures, the reality they are used to discover must be thought of as constituted by the adoption of the relevant theories and methods. Only such an interpretation preserves a modest foundationalism in the philosophy of science and (thereby) permits an epistemically satisfying solution to the start-up problem.

One final point about the basic argument from theory-dependence is important here. I have suggested that the thrust of the argument should be understood as an attempt to preserve an eminently plausible version of foundationalism in the light of potentially embarrassing facts about the actual history of science. Of course this argument for constructivism would be unconvincing if it were possible by other more modest means to avoid the rejection of modest foundationalism. I believe that it is not. I have argued (Boyd 1989, 1990a, 1991) that scientific realism entails—given overwhelmingly plausible scientific and philosophical assumptions—just the sort of antifoundationalism from which N-K constructivism saves us.

It might seem that an empiricist conception of scientific theories would fare better in this regard, given the centrality of foundationalist assumptions in empiricist epistemology. I have argued elsewhere (Boyd 1990a, 1991) that this is not the case. So deeply theory-dependent are the actual methods of science that the most plausible *empiricist* treatment of them will treat their reliability as an empirical matter and their justification as consequently a posteriori. Instead of portraying theory-dependent methods as presuming the approximate truth of the background theories upon which they depend, the plausible empiricist position will treat them as grounded in a second-order induction about the reliability of inductive methods in science of the sort suggested by Quine (1969a). Since the conclusions of such inductions about induction are just about as unobvious and subject-matter-specific as the background theories whose methodological import they reconstruct, the plausible empiricist will reach as pessimistic a conclusion about inference-rule foundationalism as will the realist. Only the N-K constructivist saves modest foundationalism.

I conclude, therefore, about the basic argument from theory-dependence that, when properly formulated, it rests on the correct assessment that only N-K constructivism can reconcile the recognition of such genuine scientific

knowledge as we appear to have with the acceptance of a modest and independently plausible version of foundationalism.

One and a Half Arguments from Incommensurability. In this category I place the arguments, anticipated in Hanson 1958 and developed in Kuhn 1970 and elsewhere, which seek to establish that the methodological and conceptual distance between successive stages in certain central scientific traditions is so great as to preclude any interpretation according to which they have a common subject matter. If the traditions are historically central enough (and Kuhn's candidates certainly are), the demonstration of such incommensurability would make impossible any defense of scientific realism along any currently developed lines and would almost certainly compromise the position of any empiricist who adopted the response to theory-dependent methods suggested above.

It is useful to distinguish between two components of the alleged incommensurability between such stages, *semantic incommensurability* (the doctrine that the conceptual gap between the relevant stages precludes a common reference for the terms they employ in common) and *methodological incommensurability* (the doctrine that no rational methods acceptable within each of the two relevant stages are sufficient for the resolution of the dispute between them). Central to the defense of the first of these doctrines has been the conception that the most fundamental laws containing a theoretical term, and perhaps the most central methodological principles governing its use, should be thought of as providing its definition so that changes in such laws and such principles represent a change in subject matter.

The arguments for methodological incommensurability have been more complex, but they all revolve around demonstrations that certain changes in theoretical conceptions (or "paradigms") have departed from plausible models of scientific rationality in important ways: There are never "crucial experiments" whose relevance is accepted by proponents of the earlier and later paradigms and whose outcome is decisive by the standards of each group. Instead, the results of individual experiments are always subject to significantly differing interpretations. Decisions of scientists to adopt the new paradigm have the character of changes in allegiance or outlook or career commitment more than that of a measured response to decisive evidence. Equally rational and distinguished scientists make different judgments about which allegiance to adopt. Full acceptance of the new paradigm often waits until the holdouts (who are often older scientists) have largely died or retired rather than being occasioned by some especially convincing body of experiments. The "textbook" picture according to which the new paradigm is decisively confirmed by the available data emerges only after the victors write new textbooks; it does not describe the process of transition between paradigms.

All of these (and similar) features of revolutionary transformation in science, the constructivist argues, fail to fit the picture of progress leading to increased knowledge of a theory-independent world. We might ask, "What must the world be like if the procedures of normal science are to be discovery procedures?" Since, according to the constructivist, scientific revolutions cannot be construed as episodes of discovery, we must think of the periods of normal science which they delimit as involving the investigation of quite different sets of socially constructed phenomena. A constructivist interpretation is necessary if we are to understand each of the episodes of normal science which precede and succeed a scientific revolution as involving the establishment of genuine knowledge: N-K constructivism emerges as the only alternative to debunking constructivism.

It seems to me that these two arguments are not best understood as providing independent considerations favoring N-K constructivism; each, by itself, makes at best a rather weak case for constructivism. Consider the case of the argument from semantic incommensurability. Even without the development of sophisticated realist (or empiricist) alternatives to the underlying theory of the definitions of theoretical terms, a number of considerations cast doubt on the conclusion that changes in fundamental laws must be taken as indicating a shift in reference or in subject matter. In the first place, the range of examples of apparent reference by (or in the face of) misdescription outside science is considerable so that one's confidence that fundamental laws must fix reference by exact and essentially analytic description should be limited.

There are, moreover, numerous examples within science in which changes in the most fundamental laws involving less "fundamental" entities or magnitudes do not seem to have involved a change in subject matter. We are not, for example, inclined to think that an apparent discovery that a disease has a dietary rather than a bacterial cause must be diagnosed as a change in subject matter, nor are we at all inclined to think that apparent disputes about the mechanisms of speciation must always reflect instead changes in the extension of the term "species." Such examples suggest that even in scientific cases fundamental laws are not always to be thought of as providing analytic or otherwise unrevisable definitions of their constituent terms. These considerations do not *entail* that the semantic theory underlying the argument from semantic incommensurability is mistaken for the sorts of cases involved in scientific revolutions, but they do cast doubt on its plausibility.

There are likewise reasons to doubt that the argument from methodological incommensurability is sound. There are a number of models of the ways in which the rationality of the scientific community supervenes on the rationality of individual scientists, and of dialectics of rational assessment of experimental evidence, which can accommodate the troubling facts about the epistemology and politics of scientific revolutions to a realist or empiricist conception of scientific progress. Such models can easily portray both the idiosyncratic and programmatic features of scientists' shifts in allegiance during "revolutions"

and the dialectical complexity of the assessment of novel data as generally contributory to the epistemic success of scientists in studying the (theory-independent) world. Thus any successful deployment of the argument from methodological incommensurability would require rebuttals to these alternative models of revolutionary episodes.

Despite these weaknesses, the arguments from incommensurability have played a very serious role in recent philosophy of science. In part, that is so because they indicate fundamental weaknesses or difficulties in the deeply influential empiricist conceptions of scientific knowledge and of the semantics of scientific terms. But it would be a mistake to see their impact as exclusively negative. Instead, I suggest, while neither argument is by itself especially convincing, taken together they spell out in a mutually reinforcing way the details of an important nondebunking alternative to realist and empiricist conceptions of progress in science (hence: one and a half arguments from incommensurability).

2.2. *Two and a Half Classical Rebuttals*

Realist rebuttals to the classical arguments for N-K constructivism can likewise be classified into two broad categories embodying responses to the basic epistemic argument and to the arguments from incommensurability.

Realist Treatments of the Epistemology of Theory-Dependent Methods. In seeking to identify classical realist rebuttals to the basic epistemic argument from the theory-dependence of methods it is important to remember that both N-K constructivism and contemporary scientific realism arose largely as commentaries on the inability of traditional empiricist conceptions of science to take adequate account of the theory-dependence of actual scientific methods. Far from defending realism against difficulties raised by theory-dependence, realist philosophers of science are probably better understood as embracing the fact of theory-dependence as the basis of an argument for realism.

Against the epistemological argument for constructivism, I suggest, the classical realist rebuttal (I have in mind here the lines of argument represented in, for example, Putnam 1962, 1975a) is best thought of as involving a strategy for seeing theory-dependent methods, realistically interpreted, as *guarantors of*, rather than *obstacles to*, knowledge of a theory-independent reality. Here the crucial idea is that such methods should be seen as establishing the basis for scientists' *epistemically relevant causal contact* with their subject matter. The clearest illustration of this conception is that provided by a realist treatment of the theory-dependence of measurement procedures (see, e.g., Byerly and Lazara 1973) according to which scientists employ available approximate knowledge of "theoretical entities" in order to devise procedures for measuring or detecting them and their properties, thereby providing the basis for improvements in theoretical knowledge and in subsequent measuring procedures.

In general an account of the epistemology of science developed so as to sustain the realist conception of the positive contribution of theory-dependent methods in this way will portray theory-dependent methods (which is to say, in fact, all the methods of science) as reflecting a theory-dependent theory-modification strategy in which, if things go well (partial and approximate) theoretical knowledge is exploited to develop methods for the acquisition of new (partial and approximate) knowledge, in turn leading to better methods, and so on. Such an account then envisions a dialectical interaction between theoretical and methodological developments producing, under favorable circumstances, mutually reinforcing progress in both arenas (Boyd 1982, 1990a).

It is important to understand the strengths and weaknesses of this classical rebuttal. It answers the puzzling question "How might methods as theory-dependent as those of science provide knowledge of a theory-independent world?" by offering an epistemically favorable but realist account of the operation of those methods, one according to which their operation systematically guides researchers toward (approximate) truth. Insofar as the epistemic challenge to realism is seen as arising from the threat of radically contingent conception of the epistemology of science, the situation is different. The classical rebuttal in no way avoids the radical contingency that seems to plague (or at any rate to accompany) a realist or empiricist treatment of deeply theory-dependent methods. The theory-dependent theory-modification strategy embodied in scientific methods is portrayed as a theory-improvement strategy only when the method-determining background theories are relevantly approximately true, so that inference-rule foundationalism is abandoned and a radically contingent solution to the start-up problem is entailed. The realist who, like the constructivist, asks, "What must the world be like if the procedures of normal science are to be discovery procedures?" must answer, "A world in which, as a highly contingent matter of fact, suitably approximately true theories arose whose acceptance established reliable methods rather than being a consequence of their operation." (For an alternative diagnosis of the situation of the realist with respect to this issue see the challenging analysis in Miller 1987.)

Insofar as the classical realist rebuttal responds to the challenge of radical contingency (rather than just to the question of how theory-dependent methods can be seen as contributing to knowledge of a theory-independent world), it is almost certainly best understood as justifying radical contingency in the epistemology of science by assimilating it to a broader naturalistic antifoundationalism justified independently by appeal to naturalistic conceptions of perceptual knowledge, everyday natural knowledge, "folk" psychological knowledge, moral knowledge, and so on. Thus, to a far greater extent than has been widely recognized, scientific realism must be thought of as a component of a general naturalistic and antifoundationalist epistemology. (I develop this theme in part 5.)

The Classical Rebuttals to Incommensurability Arguments. Against the arguments from incommensurability, the classical realist rebuttals to constructivism can be seen, with certain important qualifications, as resting on two conceptions: (a) causal or naturalistic theories of reference and of kind definitions (Putnam 1975a, Kripke 1972, Boyd 1979) which provide the resources necessary to defend, in a fashion appropriate to the actual history of science, the denial that conceptual changes during "scientific revolutions" entail changes in subject matter, and (b) arguments to the effect that, for the actual episodes in the history of science identified as revolutionary by defenders of incommensurability, there obtained, to a relevantly good approximation, *pairwise theory-neutrality of methods*. According to arguments in this second class, although there are no general and theory-independent methods adequate to resolve the differences between pre- and postrevolutionary theoretical conceptions (or to do anything else interesting for that matter), there have always been methods whose justification is neutral between the conflicting claims of the pre- and postrevolutionary conceptions which rationally dictate the choice of the latter conception in most or all of its relevant details. (I have it in mind that an appeal to approximate pairwise theory neutrality of methods captures central argumentative strategies of, e.g., Putnam 1962, Shapere 1964, and Scheffler 1967.)

Now for the qualifications. In the first place, the theories of reference and of kind definitions which have classically been advanced against arguments from semantic incommensurability have displayed a mix of naturalistic or causal elements on the one hand and descriptivist or conventional elements on the other. What almost all such conceptions share with the positions of, for example, Kripke (1972) and Putnam (1975a) is that they acknowledge the important role, in fixing the reference of scientific terms and in defining scientific kinds (properties, magnitudes, etc.), of nonconventional (non-"nominalist") features of linguistic and scientific practice — features that reflect a strategy of deferring to the actual causal structure of the world in classificatory, inductive, and explanatory practice (for a general account of the relation between such deference and scientific practice see, e.g., Putnam 1975a, 1975b; Boyd 1979, 1990a, 1990b). Even among philosophers who are critical of "pure" causal theories of reference, there is near consensus in favor of "mixed" theories recognizing such deference and near consensus about the appropriateness of such theories for rebutting (many) claims of semantic incommensurability.

Qualifications are also required with respect to the claim that classical realist rebuttals to arguments from methodological incommensurability posit pairwise theory neutrality of methods. As I suggested earlier, the ways in which rationality of the scientific community supervenes on the rationality of individual scientists is complex, and one of the complexities is that, without compromising either individual or collective rationality, scientists within a tradition may differ significantly in their methodological standards and conceptions. Indeed it is arguable (from almost any philosophical perspective) that such divergence of methodological perspectives and the similar divergence on theoretical matters which sustains and follows from it are essential to collective scientific rationality. In consequence, it would be mistaken to think of a plausible realist rebuttal as resting, for example, on the claim that all of the principal methods that underwrite the acceptance of a new theoretical perspective or paradigm are acceptable to all of the serious or rational defenders of its predecessors. What realists are best understood as claiming is that all or most of the evidential considerations which persuade those who adopt the new conception are certified as evidentially relevant by theoretical and methodological considerations rationally accepted by a substantial fraction of the opposition and that, over time, the evidence which has accumulated becomes persuasive by all or almost all of the evidential standards which the earlier conception underwrites. This pattern of overlapping methodologies stretching over "revolutionary" episodes, the realist argues, makes a realist historical explanation of such episodes as reflections of the growth of knowledge about a common world preferable to any explanation that invokes wholesale semantic and methodological discontinuity.

It is almost certainly also essential to this classical realist rebuttal to claim that the pattern of overlapping methodologies reflects a convincing pattern of *mutual ratification* between consecutive stages in the development of the relevant 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 conceptions as approximately true in just those respects which 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 scientists' estimates of the degrees and respects of approximation of both the earlier innovative proposals and their predecessors, the initially discernible 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*, even in cases of "scientific revolutions," which, the realist will argue, justifies our accepting the realist conception of justification reflected in (a) and (b) (see Boyd 1988, 1990a); it will also be important for the realist's case to insist that the qualified methodological commensurability which the historical record exhibits is all the commensurability that a realist should expect (see Boyd 1988, part 5).

Importantly, the classical rebuttals to semantic and to methodological in-

commensurability are closely related. On the one hand, the sorts of referential continuity endorsed by the former are just those required to sustain the latter. On the other hand, the reference-sustaining mechanisms causal or descriptive—and the conceptions of kind definitions for particular cases posited by naturalistic semantic conceptions are just those which are apparently indicated by the picture of the growth of knowledge offered in rebuttal to the argument from methodological incommensurability. They, like antirealist arguments from incommensurability, should be thought of as mutually supporting components of single philosophical conception offered as an alternative to the constructivists' conception of scientific revolutions, rather than as independent criticisms of it (hence, one and a half rebuttals to constructivism on the issue of incommensurability).

One more point about the classical rebuttals to constructivism will prove to be important to our consideration of the second-generation options open to sophisticated constructivists and their realist critics. The details of the classical realist rebuttal to incommensurability, I suggest, are important for a full articulation and development of scientific realism *but not for establishing a prima facie case against the incommensurability arguments*. Instead the largely example-rather-than-theory-driven considerations that so much reduced philosophers' confidence in the analytic-synthetic distinction, especially with respect to scientific propositions, operated in the case of semantic and methodological incommensurability as well, so that, even in the absence of definitive and fully articulated realist semantic and methodological conceptions underwriting an appropriately qualified finding of pairwise theory neutrality of methods, there still existed good, if not entirely compelling, reasons to suppose that such conceptions would be forthcoming. Indeed, the number of plausible semantic and epistemological conceptions that underwrite an appropriate finding of commensurability is so large, and the arguments from incommensurability are so dependent on rigid positivist caricatures of the semantics and epistemology of theoretical inquiry, that it has been for a long time reasonable to doubt the cogency of those arguments.

By contrast, I suggest, the case for realism against the basic epistemological argument for constructivism does really require something like the articulation of an alterative realist *theory* of confirmation and of the foundations of the epistemology of science. This is so because accepting a realist conception of scientific knowledge over either an empiricist or a constructivist conception requires the rejection of extremely plausible epistemological principles. In order to reject key empiricist arguments against the possibility of knowledge of "unobservables," the realist must abandon even the most plausible versions of the extremely plausible position that empirically equivalent theories are always equally well supported or refuted by any given body of experimental evidence (see Boyd 1983, 1989). Rebutting the constructivist conception of scientific knowledge requires the realist to abandon not only the evidential indistinguishability thesis just mentioned but an extremely plausible version of modest foundationalism as well.

In consequence, an adequate defense of scientific realism against the basic epistemological argument really requires the articulation of a distinctly realist (and naturalistic) epistemological theory adequate to justify the abandonment of these two plausible epistemological theses. I have argued elsewhere (Boyd 1982, 1989, 1990a) that an appropriate epistemological theory is available. Nevertheless, neither the theory I propose nor any other version of epistemological naturalism is uncontroversial, and—as I have indicated earlier—a naturalistic epistemology adequate to underwrite scientific realism will need to reject modest foundationalism in a way in which, for example, a naturalistic conception of everyday knowledge might well not. I conclude therefore that the basic epistemological argument for N-K constructivism is considerably more powerful than the arguments from incommensurability and hence that versions of N-K constructivism which do not posit the sorts of incommensurability anticipated by those latter arguments would pose a serious and interesting challenge to scientific realism.

3. SOPHISTICATED NEO-KANTIAN CONSTRUCTIVISM

3.1. Three and a Half Arguments for Sophisticated Constructivism

A sophisticated N-K constructivism that avoids positing semantic and methodological incommensurability across scientific revolutions is, I shall presently argue, certainly possible and is thus certainly a potential rival to empiricist and realist conceptions of scientific knowledge. The defender of such a constructivism will have available the argumentative resources of the basic epistemological argument without the burden of defending apparently refuted claims of incommensurability. In assessing sophisticated constructivism it will, of course, be important to examine realist rebuttals to the basic epistemological argument—that is, to assess the relative merits of realist naturalism and constructivism as epistemological theories. It will also be important, however, to take account of the less technical considerations which philosophers and others have thought of as favoring constructivism and to see to what extent these considerations may favor or compromise sophisticated constructivism or its realist alternatives.

I have claimed that the arguments from incommensurability for N-K constructivism are weak and that the variety of plausible rebuttals to them is great. Still it remains true that the primary arguments for constructivism discussed in the literature are the arguments from incommensurability and that constructivist conceptions of science, and closely related relativist conceptions, continue to exercise considerable (and perhaps growing) influence. It is reasonable to ask what explains this continued influence. Several explanations suggest themselves. In the first place, the distinction between N-K constructivism and other doctrines affirming the "social construction of reality" has not always been sharply drawn, and N-K constructivism has no doubt gained some support that properly belongs to the more plausible versions of those other doctrines.

I am inclined, however, to think that there is another important reason for the continued influence of constructivism. Many people, I believe, are convinced that, however well or badly the technical arguments from incommensurability may fare, broader philosophical considerations favor constructivism. The more general considerations favoring constructivism, I believe, are those which suggest that constructivism is required in order to account adequately for a variety of important features of science and of the relations between scientific inquiry and other human activities, *even when recognition of those* features may be logically compatible with the affirmation of an alternative conception of scientific knowledge.

One especially clear case of the latter sort of consideration is almost certainly the tacit recognition of the force of the basic epistemological argument discussed earlier. The revolutionary episodes in the history of science which underwrite claims of incommensurability do indicate quite clearly the profound theory-dependence of scientific methods, so it is reasonable to suppose that those who advance, or are persuaded by, arguments from incommensurability are also tacitly influenced by the more persuasive basic epistemological argument from theory-dependence.

In addition to the considerations captured by the basic epistemological argument there are, I believe, considerations of two other sorts which are widely thought to support constructivism.

Consideration of Unobvious Conventionality or Historicity in Representation. Here I have in mind the suspicion (linked to concerns about ontological pluralism discussed below) that there may well be, and probably are, features of our scientific picture of the world which appear to reflect fundamental features of nature but which are, in fact, artifacts of conventional or otherwise merely historically determined features of our conceptual schemes. I have in mind the sort of thing that is true of most of our conception of taxa above the species level if cladists are right. Such possibilities raise questions in general about the cogency of the distinction between features of our representational apparatus and genuine features of a representation-independent reality.

Consideration of Two Sorts of Pluralism. *Ontological Pluralism.* Here I have in mind the (justly) influential idea that the conceptual scheme necessary for adequately describing the world is underdetermined by the task of matching theory to causal structure so that there will be several different ways of "carving up" the world which are equally scientifically legitimate. This point can be amplified by indicating two dimensions to the pluralism thus identified.

In the first place it is true that *between* different scientific disciplines there will be different ways of carving up the world answering to the different interests and concerns of the various disciplines. It is also true that even *within* a single discipline there will be a plurality of adequate conceptual schemes. Especially in a dialectical situation in which it is widely held that realism entails both the interest-independence of natural kinds and categories, and the existence of a single true theory (with a single appropriate conceptual scheme), these considerations of ontological pluralism make constructivism seem an attractive option.

Since, where the phenomenon of ontological pluralism obtains within disciplines, it will be in some sense a conventional or merely historical matter which conceptual scheme scientists employ, such pluralism is perhaps best seen as a particularly striking and philosophically provocative case of unobvious conventionality in scientific representation. Similarly, the interest-and-discipline-dependence of kind definitions makes kind definitions determined in part by historical factors, so that this phenomenon too may be viewed as an important special case of unobvious historicality.

Cultural Pluralism. Here I have in mind the analogous, but in a way deeper, point that the theories and practices of cultures different from one's own are likely to embody strikingly different conceptual schemes and apparent ontological commitments without thereby being shown to be irrational. In taking considerations of this sort to tell in favor of constructivism, philosophers and others are participants in what is by now a long and deeply influential tradition of relativism in the name of tolerance.

The most important fact about these latter considerations favoring constructivism is that, like the basic epistemological argument from theory-dependence and unlike the arguments from incommensurability, none of them has been decisively rebutted by arguments which all or almost all realists would now accept. At least arguably an adequate realist response to the concerns about unexpected conventionality and ontological pluralism would require the adoption of a distinctly realist and non-Humean conception of causation, of reduction, and of supervenience which would not be fully acceptable to many scientific realists (see Boyd 1985b, 1989). Similarly, a cogent realist response to the concerns about cultural pluralism may well ultimately depend on the naturalistic and anti-(modest) foundationalist realist rebuttal to the basic epistemological argument (Boyd 1989, 1990a, 1991). I conclude that in assessing the relative merits of realism and sophisticated N-K constructivism we need to take seriously three and a half arguments for constructivism: the basic epistemological argument from theory-dependence and two and a half less-technical arguments—the argument from cultural pluralism and the (one and a half) closely related arguments from considerations of unexpected conventionality and historicality and of ontological pluralism.

3.2. Constructivism without Analyticity: How to Be a Sophisticated Constructivist

Insofar as the available rebuttals to the arguments from incommensurability rest on recent developments in philosophical theory, they rest primarily on the articulation of alternatives to the traditional empiricist conception that the definitions of general terms should be provided by analytic sentences or "L-truths." It is the ease with which one can articulate and defend alternatives to this conception that explains the ease with which such rebuttals can be developed.

It might seem that any version of N-K constructivism, however little committed to incommensurability, would be vulnerable to the refutation in the light of recent critiques of analyticity. After all, we are by now used to thinking of social conventions regarding cognitive matters as being reflected in the analyticity or truth by convention of some body of sentences. The constructivist, in treating certain features of reality as matters of social convention, must, it would seem, treat certain theoretical claims or other scientific principles as analytic or otherwise true by convention. The burden of proof would then lie with her to show that the relevant claims of conventionality are not as vulnerable as others have so often been.

It is important to recognize that what really matters to the thesis of conventionality or social construction in science is not analyticity or linguistic conventionality but rather a sort of *historicality*. What matters is whether fundamental factual descriptions in science represent structures whose existence and properties are in the relevant sense independent of the historical development of the research or practical traditions in which they are studied, or whether instead what is true about the world scientists study is determined in relevant ways by features of the conceptual structure which, as a matter of historical fact, has developed within those traditions. Is truth a matter of being faithful to the world "out there" or is it instead a matter of being faithful to certain traditions *and thus to the only studiable world there is?*

If constructivism is understood as the affirmation of the latter answer, then the commitment to anything like analyticity of some set of theoretical statements or other principles is, I suggest, entirely dispensable. Consider what sort of conventionality the constructivist must posit as operating within a tradition of inquiry if she is to retain the ontological thrust of N-K constructivism with respect to that tradition while avoiding implausible commitment to the unrevisability of any particular theoretical principles or other doctrines. What she requires is that the metaphysical picture represented in the relevant theories or other doctrines within the tradition be *in broad outline* a matter of convention but that the conventionality involved be such that the rules of rational inference internal to the tradition itself permit quite radical revisions in laws or other principles as a result of new data, theoretical innovations, or other developments acknowledged as epistemically relevant within the tradition itself.

Let us call the required sort of conventionality or historicality *dialectically complex conventionality*. It is all but certain that dialectically complex conventionality is not only possible but actual. Consider for example the wide range of traditions of theological inquiry which we would now describe as mythological (I think that all theological traditions should be so classified, but nothing in my use of this example depends on such an assumption). It is profoundly unlikely that all such traditions possess a tradition-independent subject matter. Almost equally unlikely is the historical thesis that each such tradition is founded on a set of analytic or otherwise unrevisable principles. Indeed, given the extent to which such traditions are known to be influenced by changing cultural, philosophical, scientific, political, and diplomatic factors, it would be an unlikely historical thesis that any such tradition is so founded. Thus it is reasonable to suppose that our understanding of the semantics of any such tradition involves the recognition of just the sort of conventionality which the N-K constructivist requires. Of course, the question will remain whether or not the constructivist can defend the thesis that relevant instances of this sort of conventionality are world-constituting in the relevant metaphysical sense, but—given the actual history of intellectual and practical inquiry—it seems that dialectically complex conventionality is a better candidate for this role than conventionality grounded in anything like analyticity.

It might be objected that the judgment that the required sort of dialectically complex conventionality is possible is philosophically premature since we do not have a secure theory of univocity for terms governed by such conventionality. Perhaps no account of univocity for complex traditions of the sort in question will underwrite the required judgments of continuity of subject matter, and we will be forced to recognize that only dialectically simple conceptions of conventionality grounded in notions like that of analyticity will support diachronic judgments of univocity.

It is true, of course, that there is no single theory of univocity for (as a realist would put it) nonreferring terms. But here, as in the case of the search for semantic theories to ground a rebuttal to arguments from semantic incommensurability, we suffer from an embarrassment of riches. Almost any theory one can think of, from a "property cluster" account to an account that mimics causal theories of reference by emphasizing continuities in referential intent, will ground a quite plausible first approximation to the required theory of univocity. We have every reason then to expect that an appropriate theory is possible.

Dialectically complex conventionality is almost certainly a real phenomenon, and it is not theoretically intractable. It follows that a sophisticated constructivist conception of science may be understood as asserting that such conventionality characterizes the ontological commitments of even the most mature sciences and that such conventionality has metaphysical import. Such a constructivism need not be burdened with the assumptions regarding analyticity and semantic and methodological incommensurability which make classical constructivism vulnerable to decisive refutation.

3.3. Sophisticated Constructivism and Commensurability

If sophisticated N-K constructivism can avoid just the conclusions about incommensurability which embarrass the classical version, it is reasonable to ask just what conclusions about commensurability and incommensurability sophisticated constructivism can accommodate. Two conclusions seem clear from the considerations rehearsed above.

With respect to the question of semantic commensurability the sophisticated constructivist can certainly accept any philosophically and historically plausible diagnosis to which a realist might be attracted. *Indeed, and this is the important point, the constructivist can appropriate the causal theory of reference as an account of the ground of judgments of coreferentiality made within any given research tradition, so that she can say and defend anything about the referential semantics of actual scientific theories which a realist can say and defend.* Of course she will hold that the reference-determining causal relations are themselves social constructs, but since that is something she says about all causal relations, no special problems need infect her conception of semantic commensurability.

Moreover, precisely because the sophisticated constructivist need not be burdened with implausible judgments of semantic incommensurability, she may similarly make and defend any judgment about methodological incommensurability which a realist could make and defend.

One qualification to theses conclusions may be necessary if we focus our attention on a special notion of *long-range commensurability*. Consider the situation of two different theoretical or practical traditions which, rather than enjoying the relation of predecessor to successor, have developed in relative independence but which have to some extent overlapping subject matters. Neither realism nor constructivism, nor sophisticated empiricism for that matter, predicts methodological commensurability between two such traditions. The mixes of insight and error which they embody may be so mismatched that there are no common methodological principles adequate to resolve the differences between them. Nevertheless there may be the prospect of long-range methodological commensurability: subsequent theoretical developments within the two traditions, perhaps in response to their interaction, may lead to a situation in which methodological commensurability obtains. There are reasons to believe that realism makes a certain extremely qualified prediction of long-term commensurability in circumstances in which sophisticated constructivism need not. After all, if both traditions study the same (socially unconstructed) world, then the world itself can be seen as a causal factor enhancing the likelihood of sufficient theoretical convergence to underwrite methodological commensurability. The difference here is, I believe, important both to the constructivists' treatment of issues of ontological and cultural pluralism (see section 3.4) and to corresponding realist rebuttals (see sections 5.4, 5.6), but it does not diminish the sophisticated constructivist's capacity to mimic plausible realist treatments of more standard questions of commensurability between successive stages in a single research tradition or between components of closely interacting traditions.

3.4. The Virtues of Sophisticated Constructivism

I now propose to indicate those virtues of sophisticated constructivism which, in my view, make it the version of constructivism to have if you are going to be a constructivist and (thus) the version of constructivism to refute if you are going to defend realism. Of course the obvious virtue of sophisticated constructivism is that it does not entail semantic or methodological incommensurability for those key historical cases upon which the most successful features of the classical rebuttal to traditional constructivism rest.

Just as important is the fact that sophisticated constructivism is just as well supported by (an appropriate version of) the basic epistemological argument as traditional constructivism is. Recall that the argument in question portrays constructivism as superior to realism (or sophisticated empiricism) because constructivism alone among these positions allows for the preservation of a modest foundationalism in the light of the actual historical facts about scientific knowledge. The standard constructivist's response to the irremediable theory-dependence of scientific methods should be understood, I have already suggested, as a proposal that the theory-dependent methods of science be seen as falling into two categories. The most basic rules are to be seen as grounded in theoretical principles that are true by social construction and thus a priori or otherwise epistemically privileged. Other theory-dependent inference rules are to be seen as "derived" rules justifiable ultimately by appeal to observational data interpreted according to the epistemically privileged basic rules. Modest inference-rule foundationalism is thus sustained.

Plainly this picture cannot be taken over unchanged by the sophisticated constructivist since where dialectically complex conventionality operates, any one theoretical principle could be rejected in the light of empirical evidence and any potentially basic inference rule thus undermined.

Nevertheless, sophisticated constructivism does seem to restore a modest version of inference-rule foundationalism. While no single theoretical principle and thus no single principle of inductive inference is portrayed as a priori justifiable, we are provided with an a priori or otherwise epistemically elevated justification for the broad theoretical and metaphysical picture that underwrites scientific methods, and thus for the broad methodological strategy of employing theory-dependent methods in the expectation of their general reliability and with the expectation that their subsequent refinement with the development of new knowledge will enhance their reliability. Two considerations suggest that inference-rule foundationalism this modest is appropriate as a component in a general modest foundationalism. In the first place, for most if not all scientific findings there are available converging confirmation strategies that reach the same conclusion on the basis of a variety of different methodology-determining theoretical presuppositions, and for most if not all findings the relevant methods presuppose only the approximate truth of the theoretical principles that underwrite them. Thus the epistemic warrant which sophisticated constructivism envisions for particular scientific findings will be even stronger than the epistemic warrant for the theoretical principles that underwrite the methods employed in any particular experimental or observational confirmation of it.

Moreover, that warrant is, at least arguably, as strong as any modest foundationalist should want. It seems reasonable—especially in a post-Humean world—to be suspicious of any philosophical theory of the ground of inductive inferences which makes the methods employed in making such inferences out to be more secure than they are seen to be by philosophically uncritical scientists and other inductive-inference makers. But even scientists who have forgotten their Hume in their enthusiasm for scientific methodology recognize that particular "fundamental" methods, and the theories they are based on, are revisable.

Sophisticated constructivism positing a dialectically complex conventionality in the ontological commitments of scientific theorizing has excellent prospects as well for availing itself of the other two and a half promising arguments for constructivism. Consider first the argument from the possibility of unobvious conventionality. The argument gets its force from the judgment about certain actual cases in the history of science that they involve(d) undiagnosed conventionality and from the conception that the difficulty in diagnosing such conventionality is in fact explained by its unexpected ubiquity. Whatever the merits of this argumentative strategy, it clearly will not work unless the initial diagnoses of unexpected conventionality can be sustained. If we understand conventionality as grounded in analyticity, then familiar arguments of a Quinean sort will profoundly undermine any such diagnoses. Only a conception positing dialectically complex conventionality could provide the basis for the required historical judgments.

Consider for example the very important claims of cladists that many of the features of traditional taxonomy above the species level are arbitrary or conventional. What is important to cladists' claims is that the sorting of species into higher taxa displays a large measure of historicality—that it is largely the history of classificatory practices and not the fitting of taxonomic categories to actual causal structure which determines the boundaries of higher taxa.

Analyticity of the definitions of higher taxa is not entailed, and it would be entirely inappropriate to offer in rebuttal to cladism a demonstration that no proposed definition of a higher taxon is in principle immune from empirical refutation; even cladists acknowledge that such refutation is possible since they hold proposed taxa to a posteriori standards like strict monophyly. The whole scientific and methodological point of cladism is lost if conventionality is understood as entailing analyticity and so is the pro-constructivist philosophical force of cladists' claims.

Similar conclusions follow with respect to the very similar argument from ontological pluralism. The philosopher who offers Quinean arguments to the effect that more than one scheme of ontological commitments can equally well fit the data and all of our justifiable methodological norms will be most ill-advised to hold that whatever choices a particular scientific community adopts are irrefutable in principle or otherwise rest on analytic foundations.

In the case of the argument from cultural pluralism the superiority of sophisticated constructivism has an additional dimension. Of course the philosopher concerned to advance an N-K constructivist conception of knowledge in order to combat cultural chauvinism will not want to have to hold about her own culture or others that their fundamental conceptions are so rigid as to render basic principles unrevisable in principle. She will, however, want to be able to diagnose semantic and (consequent) methodological incommensurability in those cases in which chauvinism is a serious possibility. We need to see whether the sophisticated constructivist strategy contemplated here will afford her that opportunity.

I have argued that the sophisticated constructivist, employing a dialectically complex notion of conventionality, can mimic the realist with respect to issues of commensurability in the history of science and can thus avoid the prima facie refutation of her position by the actual history of science which threatens the traditional constructivist. With respect to issues of commensurability between divergent cultural traditions, however, she is free to reach diagnoses of semantic incommensurability which a realist, especially a realist who is also a materialist, might reject. Recall that the sophisticated constructivist will posit conventionality within a tradition with respect to just those broad features of its conception of the world which seem so central as to define its epistemology: its basic methods and standards of evidence. In consequence she will treat two traditions as reflecting distinct episodes of the construction of reality—and as manifesting semantic incommensurability—just in those cases in which the case for methodological incommensurability is strongest: in those cases in which there seems to be no prospect for resolving the apparent disagreements between the traditions by appeal to "fair" (that is, tradition-neutral) methods. But, of course, these are just the circumstances in which a concern to preclude the possibility of cultural chauvinism will seem most pressing, and in which a diagnosis of unobvious conventionality and the social construction of reality will be most plausible.

I conclude therefore that sophisticated constructivism avoids decisive refutation by extant realist arguments while optimally satisfying the motives that often underwrite constructivist analyses.

4. DIAGNOSING THE CHALLENGE TO REALISM

4.1. Hidden Conventionality and a Kind of Supervenience

It is an obvious truism that social constructivists and logical empiricists posit unobvious conventionality or historicality in their analyses of scientific theories and research traditions more often than do scientific realists. It is just as obvious why this is: Let us call a methodological practice strongly theory-dependent just in case that practice is dictated by previously accepted claims about unobservable phenomena in such a way that its justification would require treating such claims as embodying approximate knowledge of "unobservables." There are lots of cases of sound methodological practices in the sciences which appear to be strongly theory-dependent. While empiricists and constructivists differ systematically in their response to apparently strongly theory-dependent methods, a common thread of appealing to the conventional characterizes each approach.

Empiricists have traditionally denied that apparent theory-dependence of scientific methods survives "rational reconstruction." They have typically subscribed to some version of inference-rule foundationalism and thus they have often denied (or failed to consider) even the weaker form of theory-dependence which would obtain if some rational methods in science depended irreducibly on a posteriori premises about observables. Of course empiricists have necessarily rejected strong theory-dependence, and one especially attractive strategy for providing the required empiricist reconstruction of cases in which rational methods seem irreducibly to depend on theoretical premises is to grant the dependence but to portray as conventional (as L-truths in Carnap's sense) some of the theoretical principles upon which the rationalization of methodological practices depends, so that no unreduced appearance of strongly theory-dependent methods survives reconstruction. In no case will the posited conventionality be in any sense obvious.

Similarly, but for different reasons, social constructivists respond to apparent strong theory-dependence of methods by treating fundamental theoretical assumptions as reflections of conventionality (or "social construction"). They treat many cases of apparent strong theory-dependence as genuine—as involving methods with deep and irreducible metaphysical presuppositions—but, for the sorts of reasons indicated in the preceding sections, they see the metaphysical reality that is the real subject of those presuppositions as socially constructed. Like empiricists whose response to apparent strong theory-dependence theirs resembles, they typically portray the most basic theoretical principles as conventional or socially constructed, treating less fundamental principles as empirically justified, given the methods justified by the deeper social construction. Thus for constructivists too, in no case will the posited conventionality be in any sense obvious.

Realists, by contrast, typically embrace apparent strong theory-dependence at approximate face value without conventionalist reconstruction. They are thus much less inclined to posit unobvious conventionality than either empiricists or N-K constructivists. What is important for our purposes is that, although conventionalists diagnose hidden conventionality more often than realists, it is denied neither by realists nor by those empiricists who reject the strategy of rational reconstruction just discussed that there are possible (indeed actual) episodes in the history of science in which features of well-confirmed theories which were rationally taken to reflect real features of the world turned out instead to reflect historically contingent (and in that sense conventional) features of the conceptual scheme of the relevant community. Indeed, cases abound in which such a diagnosis would be plausible for any realist or empiricist. If the theoretical justification which Guyot (1987) provides for cladism is convincing, then the cladist diagnosis of a high level of conventionality in the definitions of higher taxa provides a spectacular example. So do some other less inspiring examples from the history of biology. Certainly many nineteenth- and early-twentieth-century discussions of the biology of race and nationality rest on schemes of classification of human populations which turn out to be, from the point of view of biology, conventional, historically contingent, or "socially constructed" in ways that were unexpected by those who employed them, and it would be wildly optimistic to hold that there are no similar cases of undiagnosed conventionality in current biological work on, for example, human social structures.

Thus the difference between realists, empiricists, and constructivists is not over whether hidden conventionality is possible or actual but over, among other things, when (and hence how often) it should be diagnosed. But there is another important question about hidden conventionality, one with respect to which realists and (as I shall presently argue) empiricists find themselves in agreement against N-K constructivists. I have in mind the question of whether or not unexpectedly conventional features of well-confirmed theories should be thought of as—in the relevant sense—reflections of the *reality* which scientists study.

The *agreement* between the three major conceptions of scientific knowledge that hidden conventionality is a real phenomenon is a reflection of general agreement on two points: first, the unproblematical claim that in every case in which a statement in a language is true (or false) its truth (or falsity) supervenes to some extent on the social practices and conventions of the relevant linguistic community, and, second, the almost equally unproblematical claim that the semantics of actual languages is complex enough that the extent and nature of that partial supervenience will not typically be entirely obvious. The *disagreements* between the three conceptions are much subtler. In particular if we focus, as we should in the present case, on the disagreement between realists and empiricists, on the one hand, and constructivists on the other, over whether unexpectedly conventional features of good scientific theories should be thought of as, in the relevant sense, *corresponding to reality*, then what emerges is an abstruse metaphysical issue about the nature of the partial supervenience relation between the truth of the statements those theories embody and the social practices within the communities that accept and employ them. It is with this issue that we must deal if we are to assess the relative merits of realism and sophisticated constructivism.

4.2. *Philosophical Packages*

If sophisticated constructivism can mimic realism in its treatment of episodes in the history of science even to the extent of availing itself of causal theories of reference, and if the disagreements between these positions revolve around relatively speculative issues regarding long-term commensurability and relatively esoteric issues about supervenience relations, it is reasonable to wonder what philosophical methods are appropriate for evaluating the relative merits of the two approaches. In this section I address this question, developing the notion of a *philosophical package*, which I have introduced in several earlier papers (Boyd 1988, 1990a, 1990b).

We are all familiar with detailed and specific arguments advanced in defense of or against philosophical conceptions: realism is epistemologically unsound because theoretical conceptions are underdetermined even by all possible observations, phenomenalism fails because the proposed definitions of physical objects in the sense-datum language must in fact incorporate a posteriori claims about the causal operation of the senses, we must accept a noncognitivist account of moral statements because there is a logical gap between statements of fact and conclusions about duty or obligation . . . (where each of these arguments is to be thought of as spelled out and elaborated). Much of what we do—and ought to do—in philosophy takes the form of the articulation and criticism of such arguments.

It is nevertheless no surprise that single arguments of this sort are rarely (or never) thought to be decisive. Philosophical theses get modified in the light of criticisms, and their defenders may offer revisions in our understanding of related philosophical (or other) matters in order to rebut a criticism or articulate a positive argument. Thus, for example, phenomenalism can be given a respite from the argument just sketched by a defender who adopts an entirely different conception of the semantics of the imagined sense-datum language according to which its terms might be thought of as referring *causally* to naturally occurring regularities in patterns of sensation. Since there is no real sense-datum language, this approach would have to be spelled out in terms of a suitable thesis about the semantics of thought, together with a suitable conception of the connection between thought and actual languages. The phenomenalist who makes the required modification in her account and accepts the associated semantic theses will have a version of phenomenalism which requires no analytic definitions at all.

Here we have a Duhem-Quine phenomenon in philosophical methodology. Scientific theories face the results of observation in bunches; philosophical theories face whatever-it-is-that-philosophical-theories-face in bunches, too. I have argued elsewhere (Boyd 1988, 1990a, 1990b) that, in response to this complication, there is a methodological conception tacitly at work in all of the philosophy of science (and in the rest of philosophy for that matter) according to which the case for any given philosophical position, like scientific realism, logical empiricism, or constructivism, consists not just in the arguments explicitly advanced on its behalf but also in the broader range of conceptions about epistemological, metaphysical, semantic, and other matters that are either necessary to its defense or plausible developments of it. Rational choices between competing philosophical conceptions are in turn based on assessments of the relative merits of the "philosophical packages" thus associated with them.

Thus, for example, the case for an empiricist conception of scientific knowledge rests not only on the primary verificationist arguments in its favor but also on the success of related empiricist treatments of issues of the semantics of theoretical terms, the nature of explanations, the analysis of materialism, and so forth. Similarly the case for realism rests not only on arguments designed to establish realism as the appropriate account of theory-dependent scientific methods but also on the development of distinctly realist conceptions in semantic theory and metaphysics. A rational assessment of the relative merits of these conceptions requires an evaluation of the relative merits of the associated philosophical packages.

What I propose is to employ this explicit formulation of commonsense philosophical methodology in analyzing the relative merits of realist and constructivist conceptions of scientific knowledge.

4.3. Two and a Half Constraints on Conventionalism(s)

In a certain sense all philosophical analyses of science, even realist ones, aim at what positivists called "rational reconstruction": they aim at identifying and highlighting as central those features of science which are most fully rationally justified and at distinguishing these from less rational features that are diagnosed as inessential. In this section I formulate some rational constraints on theories of conventionality in science, thinking of such theories as components in rational reconstructions of scientific knowledge, and thinking of those reconstructions in turn as components in broader philosophical packages. I suggest that we can glean from sound practice in the philosophy of science two and a half constraints on rational reconstructions which have a special bearing on accounts of conventionality in science. In each case what is crucial is that acceptable rational reconstructions must, in a sense to be explained, *reconstruct actual* science. I propose that any adequate rational reconstruction must meet two conditions, one of which has, as a special case, an important constraint on the supervenience relation between truth and (among other things) social practices discussed in section 4.1. Here they are:

Coherence with Actual Science. I have in mind here two closely related constraints on proposed reconstructions. The first requires that, prima facie, the reconstructed versions of scientific theories must be consistent with the apparently best-supported findings arising from actual scientific practice, where the standards of evidence are those prevailing in the apparently best examples of such practice. This requirement is not absolute both because it is permissible for philosophers to make philosophical or scientific criticisms of prevailing methodology and prevailing theories and because philosophical or other cogent reasons may dictate rejecting an apparently well-supported part of current science. Nevertheless, it has been an important and rational feature of practice in the philosophy of science and elsewhere to impose a burden of proof on philosophers whose reconstructions require abandoning apparently sound scientific findings. One example of the operation of this constraint has been the universal acknowledgment among empiricist philosophers that their denial of the possibility of knowledge of unobservables is in greater need of philosophical defense given the apparent success of chemists, using the best available chemical methods, in discovering features of the (unobservable) microstructure of matter.

The closely related constraint is that the specifically philosophical claims that are components of a proposed reconstruction (or are central to its defense) must prima facie also be coherent with (suitably reconstructed) findings of actual science. The most obvious example of the application of this constraint is probably the challenge to early logical empiricists' phenomenalism which arose from the difficulty in assimilating causal theories of perceptual experience, understood as empirical theories, to the phenomenalist conception that physical objects themselves are to be thought of as constructs out of sense data.

This example also illustrates a special case of the two constraints just discussed which is especially important for our present purposes: the constraint of *supervenience relation 'reduction'*. Whenever a theory, philosophical or otherwise, has the consequence that phenomena of one sort supervene on phenomena in some other class, rational methodology requires that, prima facie, the theory should be acceptable only if it is possible, given the best available theories of the relevant sorts of phenomena, to understand how phenomena of the first sort and their causal powers could be appropriately related to phenomena in the proposed supervenience base. What is required is that, in some weak sense of the term "reduction," it be possible to establish an appropriate reduction of the allegedly supervenient phenomena and their properties to the properties and interactions of the phenomena in the alleged supervenience base.

This requirement has two aspects. The first, illustrated in the case of critiques of phenomenalism, is that when it is maintained that phenomena in one class supervene only on phenomena in some second class, it should be possible to explain how the causal powers and properties of phenomena in the first class can be fully accounted for by the powers and properties of phenomena in the second class. The second aspect is more important for our present investigation. Suppose that it is proposed that phenomena in one class are essential components of any supervenience base for phenomena in some second class. Then it must be possible to make scientific sense of the posited necessity. It must be possible to understand why, were phenomena of the first sort relevantly absent or different, phenomena of the second sort would be absent or different. It is this aspect of the supervenience reduction requirement which is tacitly invoked when it is objected to a particular version of behaviorism that some psychological state or other could exist even if the behaviors said to be from a necessary component of any supervenience base are absent. Note that we can recognize a plausible appeal to the supervenience reduction constraint—or any other similar constraint—even if we hold that the resulting challenge to a supervenience claim is ultimately unsuccessful.

Of course this 'reductionistic' requirement applies in the special case in which the supervenience in question is an alleged eliminative or constructivist supervenience of the truth of various factual claims on features of linguistic or conceptual conventions or other aspects of social practice or mental life. When it is claimed that truths about some sort of phenomena supervene largely or exclusively on such matters of linguistic or other convention, and when, according to the best available science, the supervening phenomena have certain causal powers or effects, it must prima facie be possible to offer a scientifically acceptable account of how those powers and effects are realized by the causal capacities of the phenomena in the alleged supervenience base in such a way as to sustain the intended metaphysical (or antimetaphysical) conclusions. It is precisely this requirement which the phenomenalist eliminativist analysis of truths in the "physical object language" was apparently unable to meet.

Closely related to these constraints is another, the requirement of *ratification of reconstructed methods* which has been central in disputes in the philosophy of science. Scientific methods are (often if not always) theory-dependent and we prima facie require of a proposed reconstruction of well-established scientific theories that the reconstructed theories ratify (suitably reconstructed versions of) the actual methods of science. Of course this requirement significantly constrains the acceptance of conceptions of conventionality in science. Thus,

for example, the operationalist doctrine that theoretical terms should be thought of as conventionally defined in terms of fixed laboratory procedures failed as a reconstruction precisely because there proved to be no plausible way of accommodating within an operationalist reconstruction the ways in which rational methods in science permit the relevant sorts of laboratory procedures to be revised and improved in the light of new theoretical developments.

It is important to see that these requirements are both stronger and weaker than a requirement that philosophical theories and the methods they would rationalize be *consistent* with the apparent findings and methods of the best science. On the one hand the requirements set weaker standards than consistency since sufficiently strong philosophical considerations might well justify abandoning apparently well-established findings or methods. Thus logical empiricism is not immediately refuted by the observation that it requires us to abandon the apparently scientifically appropriate methodological judgments that countenance the confirmation of propositions about the unobservable.

On the other hand, more than consistency with the ordinary findings of science is sometimes required. Where, for example, philosophical theses involve supervenience claims of a sort not contemplated in any of the (other? see below) sciences, the "reductionist" requirement requires that we assess the coherence with the best science of claims which no scientist would ordinarily consider. If we reach an adverse verdict regarding a proposed supervenience claim, the reason will be that it does not make good scientific sense, all things considered, rather than that it is inconsistent with a finding of some scientific discipline or other.

A Naturalistic Note on Method. The methodological role played by these constraints illustrates an important methodological point about the "philosophical packages" that represent contending positions in the philosophy of science. One way to formulate this point is to say that such packages are not to be thought of as subject only to purely philosophical criticism: they are subject to additional requirements of appropriate coherence with the findings and methods of the various sciences. An alternative formulation is that philosophical packages should be thought of as including, in addition to distinctly philosophical doctrines, suitable versions of the findings of the various other disciplines with which philosophical inquiry overlaps. The latter formulation is almost certainly better: it is, after all, appropriate relations to suitably reconstructed scientific findings and methods which philosophical doctrines are required to achieve, and the suitability of a reconstruction of scientific findings is partly determined by the philosophical project in whose aid the reconstruction is proposed—that is, by the rest of the philosophical package with respect to which it is formulated. It will thus be more fruitful to think of philosophical packages as incorporating proposed reconstructions of the relevant findings from other disciplines. On this formulation, the two and a half constraints just discussed are to be thought of as reflections of broader requirements of coherence applicable to philosophical packages generally.

However the issue is formulated, what is important is that, quite independently of any general commitment to philosophical naturalism, we must recognize that good philosophical methodology requires of proposals in the philosophy of science an appropriate coherence with the empirical investigation of the natural and social world. Methods in the philosophy of science must be at least to that extent naturalistic.

It remains to see how these naturalistic considerations and other standards for assessing philosophical packages apply to the choice between realist and N-K constructivist packages when the latter packages reflect a dialectically complex conception of conventionality. It is to that question that we now turn our attention.

4.4. Diagnosing the Differences: How to Tell Carnap from Kuhn and Other Interesting Questions

N-K constructivists agree with realists that scientists routinely obtain and employ knowledge of unobservables, "metaphysical" knowledge of the sort logical empiricists thought impossible. They agree as well that the truth of the statements that articulate this knowledge supervenes to some extent on linguistic conventions and other social practices, but they disagree with realists in subtle but nonetheless crucial ways about the nature of that supervenience relation: they differ about the philosophical import of (at least some) conventions. If we are to examine the relative merits of constructivist and realist philosophical packages, we need to have a deeper understanding of the difference in their conceptions of conventionality. One possible approach is suggested by the dispute between realists and traditional constructivists like Kuhn. Traditional constructivists hold that fundamental scientific laws are sometimes (exactly) true by convention whereas it is unlikely that any scientific realist would treat any fundamental law as unrevisably conventional, and this seems to be a deep fact about realism: the realist's naturalistic and Quinean commitments will make her doubt that terms used in any dialectically complex inquiry will possess analytic definitions. We might hope, therefore, to distinguish realists' from constructivists' conceptions of conventionality in terms of the sorts of features of conceptual systems which they think can in principle be conventional: the kinds of things which rationally acceptable conventions can dictate that we accept or do.

Sadly this approach is unlikely to be helpful in the present case. The reason is that the defender of sophisticated constructivism is equipped with a dialectically complex notion of conventionality. Such a conception has two features. In the first place, of course, it avoids the commitment to analyticity and can in fact be incorporated into a semantic theory which *very* closely mirrors that of

the realist with respect to actual cases in the history of science. More importantly, the sorts of features of conceptual systems which sophisticated constructivism treats as conventional in science (roughly: broad features of a metaphysical picture) are the sorts of features which the realist must hold can be (indeed are) matters of convention in some cases of dialectically complex inquiry. Thus, while the almost complete rejection by realists of analyticity may provide a clue to the difference between realist and constructivist conceptions of conventionality, a simple extrapolation of that rejection will not help us to distinguish realists from sophisticated constructivists.

What would be nice to examine would be a case in which realists and sophisticated constructivists agreed exactly about what the conventional features of a tradition of inquiry were but regarding which they differed about the philosophical import of the conventionality they both accepted. Such a case would be provided, for example, if realists and sophisticated constructivists agreed — as they well might — about the conventional elements in, say, ancient Greek theology but differed in that constructivists took the relevant conventionality to be *world-constructing*, in the philosophically relevant sense of that notion. Of course we have no such example to examine: sophisticated constructivism is a position that has yet to be fully articulated, and thus we are not yet in a position to see just what instances of conventionality the sophisticated constructivist would have to take as world-constructing. Instead of using examples of the sort in question to clarify the differences between realists and sophisticated constructivists regarding conventionality, we need to do something like the opposite: to use an understanding of the different conceptions of conventionality to clarify differences in the conceptions of the philosophical applications of that notion.

In consequence I propose to approach the problem of characterizing constructivist-realist differences over conventionality indirectly, by examining a case in which a traditional constructivist and a traditional empiricist do agree almost exactly about what the conventional elements are in a scientific research tradition while differing about the philosophical import of the conventionality they both acknowledge. I propose to ask how to tell Carnap from Kuhn. The question arises because, on the one hand, the later Carnap (of, say, "Empiricism, Semantics, and Ontology," 1950) accepts, in a certain sense, the constructivists' and realists' claim that scientific knowledge extends to knowledge of, for example, electrons, and, on the other hand, Kuhn in *The Structure of Scientific Revolutions* (1970) avoids the apparent realist implications of this conclusion by adopting a conventionalist conception of the semantics of scientific language which is almost exactly that advanced by Carnap in order to avoid the same realist conclusions. Each takes the fundamental laws involving a theoretical term to constitute that term's conventional definition. How, then, is Carnap different from Kuhn? If we understand the basis of the deep differences in philosophical import of two conceptions of conventionality as similar as Carnap's and Kuhn's, I suggest, it will help in diagnosing other deep but subtle differences regarding conventionality.

Insofar as they are taken to be describing (rather than philosophically analyzing) scientific practice, Kuhn may be seen as in large measure persuasively working out the historical, social, and psychological details of the adoption, in a natural-language context, of the sorts of theoretical conventions mirrored by the "L-truths" of the formalized languages appealed to by Carnap. Pretty plainly this conception of the descriptive content of Kuhn's work leaves unaddressed the philosophical features of Kuhn's analysis which result in its distinctive challenge to empiricist (and realist) conceptions of scientific knowledge. What we need to know is what features of Carnap's and Kuhn's positions make the first distinctly empiricist and the latter distinctly (antiempiricist and) social constructivist.

An obvious candidate (and perhaps a point of difference in their descriptions of scientific practice) lies in Kuhn's emphasis on the theory-dependence of observations. There must be something right in focusing on this issue, but recognizing their differences over the theory-dependence of observations by itself is not likely to allow us to fully understand the difference between Carnap and Kuhn or—since this is our ultimate aim—the difference between the treatments of conventionality appropriate to realist, empiricist, and constructivist philosophical packages. The reason is this: There is a variety of ways in which the empiricist can acknowledge the theory-dependence of observations in scientific practice without abandoning hope of a suitably empiricist rational reconstruction of observational practice in science. We have already seen that an appeal to the pairwise theory-neutrality of methods generally (and of observation in particular) may play a role in such a reconstruction. In fact, all that would be needed for an empiricist or a realist reconstruction would be an account according to which the theory-dependence of the methods and vocabulary of observation in science does not preclude our understanding observations and observation reports as providing for science epistemic access to its theory-independent subject matter. What is important is that somehow the N-K conception of scientific conventionality is supposed to obviate the need for such a reconstruction: epistemic access to theory-dependent reality is what scientists are to be seen as achieving.

If we move to the consideration of the structure of philosophical packages, what we see then is that the constructivist philosophical package à la Kuhn is to be equipped so that it treats socially constructed observation of, for example, a socially constructed planet as playing roughly the same role which an empiricist (or realist) package assigns to the (unconstructed) epistemic access to an (unconstructed) planet which it attributes to astronomical observation. Plainly more is going on than just the recognition of the theory-dependence of observation. Whatever else is going on must provide the answer to the question

of how, given that both Kuhn and Carnap hold that fundamental laws are true by convention, their conceptions of conventionality differ in such a way that Carnap's position is empiricist while Kuhn's is antiempiricist and N-K constructivist.

Pretty obviously the difference lies in whatever is expressed by Kuhn's claim that scientists who work within different and competing paradigms study "different worlds": the constructivist conception of (certain) conventions in science treats them as world-constituting or something of the sort whereas the empiricist conception does not. Of course N-K constructivists' talk about "different worlds" or the "social construction of reality" is plainly metaphorical. If such talk is without genuine metaphysical and epistemological import if it is just a vivid way of indicating some of the sociological and psychological consequences of the theory-dependent and socially organized character of scientific practice then constructivists turn out to be empiricists, or to be realists, albeit realists with an inadequate semantic theory for theoretical terms. So we need an interpretation of "different worlds" and related metaphors which gives them metaphysical and epistemological import and which distinguishes Kuhn's conception of conventionality, for example, from that of the later Carnap.

One idea might be to say that Kuhn's and Carnap's conceptions of conventionality differ in that Kuhn affirms whereas Carnap denies that conventional truths can have ontological import. For Kuhn and for Carnap the question of, for example, the existence of free electrons is to be understood within a context determined by certain fundamental laws about electrons which are themselves to be understood as constituting the conventional definition of "electron." But, it might be argued, for Kuhn the content of those conventional laws has ontological import, which the question of the existence of free electrons inherits, whereas for Carnap ontological import is absent. Something like this must be right, but the notion of ontological import does not do the right job: after all, the point of "Empiricism, Semantics, and Ontology" is precisely that it is the "internal" existential questions about theoretical entities like electrons which capture all the ontological import there really is. Still, we can certainly say that, according to Kuhn, but not according to Carnap, the theoretical conventions that fix the meanings of theoretical terms have metaphysical import. Carnap's position is empiricist rather then realist (or constructivist) in large part because his drawing the distinction between internal and external questions is designed to permit him to treat the former as nonmetaphysical components of scientific inquiry and the latter as nonmetaphysical pragmatic questions.

As the differences between empiricist and constructivist treatments of the theory-dependence of observations indicates, "different worlds" and related metaphors are supposed to have epistemological as well as metaphysical implications. One thing that seems clear about Kuhn's position is that the fundamental tenants of a paradigm are supposed to be research-guiding in an epistemically central way. Paradigm articulation consists in developing and testing problem solutions suggested by the previous achievements of the paradigm, and this pattern of reasoning defines scientific rationality.

It is important to seeing the relation between paradigm articulation and rationality that we recognize that, in exploring those problem solutions suggested by the paradigm, the scientist is to be understood as exploiting previously acquired *knowledge* of the world. Solutions to new problems are explored just in case they fit the metaphysical picture represented by the paradigm in its current stage of development, and this research strategy is rational (indeed defines rationality) precisely because that metaphysical picture represents knowledge of the world the scientist studies. A proposed problem solution that "fits" existing paradigmatic achievements is appropriate for scientific investigation precisely because it is supported by a kind of inductive inference at the theoretical level: from previously acquired theoretical knowledge the scientist infers a nontrivial likelihood that the proposed solution is correct, and that is what justifies her experimental investigation of it. It is precisely this that is the import of Kuhn's (and the realist's) claim that rational scientific investigation is guided by a metaphysical conception of the phenomena studied.

Here, I think, is the clue to the epistemological difference between the constructivist's and the empiricist's conception of conventionality in science. Although Carnap, for example, must agree that scientists know the theoretical claims that constitute the definitions of their theoretical terms, the nonmetaphysical empiricist interpretation of the relevant conventions precludes a rational research-guiding role for that knowledge. Inductive reasoning from conventionally adopted theoretical principles to (nonconventional) theoretical conclusions ("All hitherto posited charged particles have unit charge [where this is a matter of conventional definition], therefore we are inductively justified in believing that all fundamental charged particles have unit charge [where this is nonconventional]") is not acceptable on the empiricist conception. I do not mean that the empiricist need deny that such reasoning plays a pragmatic role in theory-invention, but merely that acknowledging the epistemic legitimacy of this sort of theoretical-level induction is precisely the mark of a metaphysical understanding of the relevant theoretical premises. It amounts to acknowledging them as reflections of the way in which (unobservable aspects of) the world, rather than mere convention, constrains rational scientific description at the theoretical level. There is, after all, no logical contradiction or semantic anomaly in positing a new particle with charge 1/2 even though all those previously posited have had unit charge; there is only an inductive risk, and that only if one sees the earlier posits as corresponding to a reality which scientists attempt to discover.

We have been examining two special cases of empiricism and constructivism which share a common (and nondialectical) conception of the conventions

that govern scientific investigation, but nothing in the considerations we have employed to diagnose the differences between them depends on the details of that conception. I conclude that if we are to understand the distinction between empiricist and constructivist conceptions of conventionality in science, then we should should look for conceptions of the metaphysics and epistemology of conventionality which—even when they agree about what the conceptual truths are—differ about the import of conventionality in the way suggested by the following chart:

Doctrine	Metaphysical import?	Inductive Import?
Empiricism	No	No
Constructivism	Yes (sometimes)	Yes (sometimes)

(I qualify "yes" with "sometimes" for the constructivist since presumably she will hold that not all conventions are world-constituting.)

What then of realism, whose position on the philosophical map we are trying to locate? Once we have sorted out empiricism and constructivism, there are very good reasons for holding that the realist's conception of conventionality, if it differs from the empiricist's at all, must agree with the empiricist's on these matters. Recall that the realist holds that neither the empiricist's nor the constructivist's conventionalistic treatments of theory-dependent methods in science is adequate because, according to the realist, neither approach adequately reconstructs the metaphysical import of the way in which inductive appeals to past theoretical achievements rationally regulate scientific practice (Boyd 1985a, 1989, 1990a). So the map we are looking for situates empiricism, constructivism, and realism as follows with respect to the import of conventionality:

Doctrine	Metaphysical import?	Inductive Import?
Empiricism	No	No
Constructivism	Yes (sometimes)	Yes (sometimes)
Realism	No	No

Realism and empiricism thus agree against constructivism in affirming the *metaphysical innocence of conventionality*, which they treat as entailing a corresponding *epistemic infertility*. It is to the implications for philosophical packages of these competing conceptions of conventionality that we now turn our attention.

4.5. Metaphysical Innocence and Philosophical Packages

A Quasi-naturalistic Constraint. An N-K constructivist philosophical package must reject, while a realist package must honor, the metaphysical-innocence and epistemic-infertility principles. Our understanding of the relative merits of the two sorts of packages would be enhanced by a clearer understanding of the implications of those constraints for the packages that must meet them. Fortunately developments in the history and philosophy of science which we have already explored in understanding the case for constructivism permit us to identify an additional quasi-naturalistic constraint which any plausible philosophical package must meet. If we restrict our attention to philosophical packages that meet the quasi-naturalistic constraint, I will argue, there emerges a simple, elegant even, characterization of the difference between packages that honor innocence and infertility and those that do not.

We have already seen that realist, constructivist, and sophisticated empiricist accounts of scientific knowledge represent three quite different responses to an initially surprising discovery—that the theory-dependence of scientific methods cannot be made to go away. All of the rational inductive methods of the sciences are theory-dependent in the sense that their scientific justification rests on an appeal to established background theories. Theory-dependent methods resist rational reconstruction; they cannot be portrayed as "derived rules" obtained in the first instance through the application of theory-independent methods. Nor do they honor the traditional empiricist's distinction between the scientific and the "metaphysical": the methodological dictates of the prevailing background theories depend on the theoretical structure of those theories and not just on their observational consequences. If we use positivist terminology and describe as "surplus meaning" those features of theories which go beyond their empirical content, then what has been discovered is that the methodological dictates of background theories depend on their surplus meaning.

What is important for our present purposes is that each of the quite different responses to ineliminable theory-dependence is appropriately seen as a response to the requirement discussed earlier that, prima facie, a philosophical package in the philosophy of science must accommodate the well-confirmed findings of the various special sciences. We can see this by understanding more clearly the nature of the theory-dependent rationales which background theories provide for methodological practices.

Recall that the standard arguments for scientific realism (Putnam 1962, 1972; Boyd 1983, 1990a) are abductive: they portray realism as a component of the best explanation for the success of scientific methods. Whether or not such arguments are successful in defending realism as a philosophical thesis (for critical discussions see Fine 1984, van Fraassen 1980), they rest on important facts about the nature of the theoretical rationale for scientific methods: For any scientifically justifiable theory-dependent method M, the theoretical rationale for M will take the form of a *well-confirmed* explanation of its reliability in terms of the (typically unobservable) causal mechanisms and processes posited in the relevant background theories. The explanation for the reliability of M will characteristically invoke the prevailing theories of those mechanisms and processes to explicate the ways in which the employment of M establishes reliable epistemic contact between scientists' practices and the causal mechanisms or processes that determine the relevant properties of their subject matter. Thus an apparently *naturalistic* explanation for the reliability of M—one that presupposes the (approximate) truth of the relevant background theories—provides the scientific rationale for M. The science's "own story" of the reliability of its methods seems to presuppose knowledge of "unobservables." It is this fact, together with the impossibility of reconstructing all such methods as derived rules, which creates the challenge to empiricism and provides a case for realism or constructivism: it appears that empiricist's antimetaphysical commitments will prove incompatible with her articulation of a philosophical package that accommodates highly well-conformed naturalistic accounts of the reliability of rational scientific methods themselves.

Of course realists and constructivists must also prima facie accommodate the same apparently naturalistic theories and, of course, they do, realists by accepting the naturalistic explanations "at face value," constructivists by accepting the explanations while reconstructing their metaphysical content along Neo-Kantian lines (thereby attenuating their philosophical naturalism and preserving modest foundationalism). It will be important for our purposes to have a more abstract and metaphysical formulation of the conception of the epistemology of scientific methods which realists and constructivists thus come to have in common. Each of the particular naturalistic explanations for the reliability of a theory-dependent feature of scientific practice portrays that feature as reliable (and thereby justifies it) by indicating that the method in question is appropriate to the underlying causal structures of the relevant phenomena. For each such justified methodological feature, the role of the relevant background theories in providing its justification is to provide an (approximately) accurate account of those causal structures. Since both realists and constructivists accept this conception of the reliability and the justification of inductive methods in science generally, they should be thought of as accepting a *quasi-naturalistic* two-part *accommodation thesis*: (i) inductive methods are reliable to the extent that they are accommodated to the causal structures of the phenomena under study and of the systems (including humans) used to study them, and (ii) background theories reliably govern methodology to the extent that they provide a relevantly approximately accurate account of those structures. Good scientific method is a matter of theory-determined accommodation of practice to the actual causal structures of the relevant phenomena.

I have argued (Boyd 1990a, 1991) that the appropriate empiricist response to the challenge of theory-dependence, "sophisticated empiricism," should be thought of as accepting the conclusion that theory-dependent methods are justified by, and their reliability explained by, knowledge reflected in the "surplus meaning" in the relevant background theories while rejecting a metaphysical understanding of that knowledge. Instead of metaphysical knowledge, the relevant surplus knowledge is knowledge of inductive methods of the sort suggested in Quine's "Natural Kinds" (1969a). The theoretical structure of our background theories represents the accumulated results of second-order induction about induction. (I argue in Boyd 1990a that the consistent empiricist must portray such structures as reflecting the results of n-th order induction about induction, for all n, but that point need not concern us here.)

What we have just learned about the way in which theory-dependent methods are theoretically justified permits us to describe this sophisticated empiricist position more precisely. When background theories T justify a method M, they do so by entailing that M is reliable. Thus, in accepting well-confirmed background theories as repositories of knowledge about the reliability of inductive methods, the empiricist is simply accepting a somewhat broader conception of their empirical content: one that counts as part of the empirical content of a body of scientific theories their (conjoint) predictions about the instrumental reliability of methodological procedures. Thus, for example, theories in biochemistry would be seen as having—together with other well-confirmed scientific theories—implications not only about the observable behavior of chemical, cellular, and ecological systems but also about the reliability of methods in chemistry, cell biology, and ecology. Since the implications about the instrumental reliability of such methods represent predictions about *observable* phenomena, the traditional empiricist stricture against acknowledging metaphysical knowledge is maintained: all scientific knowledge is instrumental knowledge. The sophisticated empiricist accepts the apparently naturalistic scientific explanations for the reliability of particular methods and interprets them in just the same instrumentalist way she interprets any other scientific findings. What is untraditional about the sophisticated empiricist position is just its naturalistic and antifoundationalist treatment of scientific knowledge.

What, we may now ask, is the sophisticated empiricist assessment of the accommodation thesis? The sophisticated empiricist agrees with realists and constructivists in taking the apparently (on the empiricist's interpretation *actually*) naturalistic explanations for the reliability of scientific methods to constitute the full story of their reliability and their justification. Thus she accepts it that (i) inductive methods are reliable to the extent that they are accommodated appropriately to lawlike patterns in the relations between observable features of scientists, the objects of their study, and the equipment they employ, and that (ii) background theories reliably govern methodology to the extent that they provide a relevantly approximately accurate account of those patterns. But, of course, on the empiricist analysis causal structures *just are* lawlike structures in the relations between observables, so the sophisticated empiricist accepts precisely the (appropriate empiricist rationally reconstructed version of) the accommodation thesis. Thus, we have seen that an appropriate response to the depth of theory-dependence of scientific methods requires of empiricist as well as of realist and constructivist philosophical packages that

they incorporate an appropriate version of the accommodation thesis. Since realism, empiricism, and constructivism represent the serious contenders in the philosophy of science, we may conclude that, in the current dialectical setting, any plausible philosophical package must include a version of the accommodation thesis. This is the quasi-naturalistic constraint on philosophical packages which permits us to formulate the metaphysical-innocence thesis with greater precision.

Recall that we are looking for an understanding of the metaphysical-innocence thesis which, when we attribute it to empiricists and to realists but not to constructivists, will ratify the convictions of realists and empiricists that conventional truths lack metaphysical import and that *for that reason* they lack inductive import. If we restrict our attention to philosophical packages incorporating the accommodation thesis, then in the packages we consider, it will be held that a feature of scientists' theoretical conception of their subject matter properly has inductive import *if and only if* it represents knowledge of the causal structures of the relevant phenomena. Realist and empiricist philosophical packages satisfying the quasi-naturalistic constraint must, therefore, incorporate the claim that when (or to the extent that) such features of scientific theories are true by convention, they fail to describe causal structures, whereas constructivists must hold that *some* conventional features (those implicated in the social construction of reality) do represent knowledge of causal structures.

Here then is the insight necessary to an understanding of the metaphysical-innocence thesis: the sense in which realists and empiricists hold, while constructivists deny, the metaphysical import of conventionality in science is that constructivists affirm whereas realists and empiricists deny that in the relevant sense social conventions in science determine the causal structure of the phenomena scientists study. I add "in the relevant sense" because, of course, scientific (and other) conventions are a matter of human social practice and human social practices themselves have *causal* effects including *causal effects on the causal structures scientists study*. Since this claim is philosophically uncontroversial, we should understand realists and empiricists as affirming and constructivists as denying *the No Noncausal Contribution thesis* (2N2C): the thesis that human social practices make no noncausal contribution to the causal structures of the phenomena scientists study. If the accommodation thesis is accepted, then 2N2C exactly expresses the metaphysical-innocence doctrine whose acceptance differentiates realists and empiricists from constructivists.

A point about this interpretation of N-K social constructivism is in order here. I am of course about to go on to argue *against* constructivism in part by arguing *for* 2N2C, so it will be important to my argument that that thesis is what distinguishes plausible realist and empiricist philosophical packages from plausible constructivist ones. My experience has been that philosophers' reactions to 2N2C and the analysis of constructivism in terms of it are quite varied.

Some have thought that a demonstration that constructivists must deny 2N2C would amount to a *reductio ad absurdum* of constructivism while others have thought the interpretation of constructivism offered here entirely fair to the philosophical intentions of constructivists. I want to emphasize that I am not offering the denial of 2N2C as an analysis of the authorial intentions of defenders of N-K constructivism nor as an analysis of the meaning of any of the various claims that express N-K constructivist theses. Instead I am arguing that philosophical insights regarding theory-dependence of scientific methods, insights which constructivists helped to establish, dictate acceptance of the accommodation thesis and that it is this thesis in turn which dictates that metaphysical innocence be diagnosed in terms of 2N2C. Thus those who find 2N2C obvious should take what has been said here so far as a *reductio* rather than as an uncharitable interpretation of authorial intent or of meaning.

That said, it is worth remarking that the denial of 2N2C has considerable independent merit as an interpretation of the meaning or the intent of N-K constructivism. Neo-Kantian constructivism is, after all, supposed to be Neo-*Kantian,* and it is hard to think of an interpretation more in keeping with that understanding. Moreover it is by no means impossible to offer arguments in favor of the denial of 2N2C besides the general arguments for N-K constructivism. For example, Putnam (1983) argues against a realist conception of the "total cause" of an event that no such notion of cause is available *because the notion of explanation is prior to that of cause* (and presumably because there is no explanatory context in which an appeal to an event's total cause is appropriate). I do not mean to speculate here about how Putnam understands the relation between the concepts of causation and explanation nor about the relation between his pragmatism and N-K constructivism. What is important is that his claim of the conceptual priority of the notion of explanation is philosophically plausible and that it could be easily articulated along lines that would entail the denial of 2N2C.

5. DEFENDING REALISM

5.1. *Defending Innocence, Part 1: Innocence as a Scientific Hypothesis*

Let C be any statement whose truth or falsity is determined by certain causal structures and let S be any set of human social practices. If the members of S contribute to the truth or falsity of C, then we may think of their contribution as factorable into two components: the contribution which elements of S make to determining the relevant causal structures and the contribution the members of S make to establishing the semantics of the language in which C is expressed. We have seen that the dispute between realists (and empiricists) and constructivists is over the possible extent of the first component. Prima facie philosophical packages must accommodate well-confirmed scientific theories, so one approach to assessing the relative merits of realism and constructivism is to assess 2N2C as a scientific hypothesis. A number of considerations suggest that we should take it to be extremely well confirmed and to conclude, in consequence, that the plausibility of constructivism is seriously compromised.

In examining the status of 2N2C as a scientific hypothesis, we face an interesting problem. If either scientific realism or a naturalistic version of empiricism is accepted, then one should probably think of philosophy itself (or at least the philosophy of science) as a scientific discipline, whereas no similar conclusion follows from constructivism. Moreover, in any science philosophical considerations operate in determining answers to questions about confirmation. How then are we to understand the question of how well confirmed 2N2C is as a scientific hypothesis? To what extent should philosophical considerations enter into that judgment?

I have no general solution to the problem of philosophical method raised here, but I propose for present purposes to ask how well confirmed 2N2C is by scientific standards not directly affected by philosophical considerations regarding N-K constructivism and closely related issues. If 2N2C fares well by those standards, I will take that to be a prima facie problem for constructivist philosophical packages but one that could be overcome (from the points of view of both science and philosophy) if the distinctly philosophical arguments for constructivism prove sufficiently powerful.

If we approach the issue in that way, then the scientific case for 2N2C seems quite strong, if a bit hard to state. Suppose that we first ask whether anything in our current understanding of human beings or their social practices suggests that 2N2C could be false. Is such a possibility suggested by what we know of the biology, psychology, sociology, anthropology, or history of human social practices, or by what we know from linguistic theory? Different N-K constructivist packages will portray different features of the scientific picture of the world as social constructions, but, for example, do the findings of any of these disciplines provide us with any reason to suppose that there are features of human social practice which necessarily lie in any supervenience base of the causal structures that reflect the atomic composition of matter? I take it that if we exclude from consideration findings of sociologists and anthropologists whose work is quite directly influenced by—or part of—the philosophical case for N-K constructivism, the answer is plainly "no." In particular, if we examine the best available empirical theories of how social practices determine the truth or falsity of statements in natural languages, they provide every reason to accept the picture of the factorization of that determination suggested by 2N2C.

Similarly we may ask whether findings in any of the other sciences provide any reason to suppose that 2N2C is false. Do the findings of chemistry and physics, for example, give us reason to suppose that social practices of, for example, chemists and physicists are necessary components of any supervenience base of the causal structures they study? Here again of course the answer is "no." But, someone might object, the fact that none of our scientific theories give us any reason to believe that a hypothesis is false provides us with no reason to suppose that it is well confirmed, thus the failure of our background theories to endorse the denial of 2N2C is irrelevant to the issue at hand.

Complex general issues are raised here about the relation between theoretical considerations and confirmation, but three things are important in the present case. In the first place, there *is* positive evidence for 2N2C, since it underwrites our best current conceptions of how human social practices determine the truth values of statements. Moreover, the fact that violations of 2N2C are not contemplated in our best theories of human social practices itself has evidential significance, if the scientific practice that gives rise to those theories is taken to be even approximately sound. The reason is this: if people live in worlds whose causal structure is determined noncausally by their beliefs and practices in the ways contemplated by N-K constructivism, then the laws governing the relations between social practices and other conditions of human life are quite different from what they would be were 2N2C true. A research methodology that does not even countenance the possibility of failures of 2N2C would be as inadequate under such conditions as one that failed to acknowledge the important ways in which theoretical practices and concepts *causally* determine causal structures—self-fulfilling prophecies for example, or the social effects of ideologically determined theories. Thus, 2N2C may be appropriately viewed as a presupposition of methodology in social inquiry (cases directly influenced by social constructivism aside), so the philosopher who accepts the methods of social scientific inquiry as in this regard sound has reason to accept 2N2C with respect to the noncausal influences contemplated in social constructivism.

Still one might not be sufficiently confident about methods in the relevant social sciences to find the case just outlined convincing, so it is important to realize that the claim that certain practices necessarily lie in any supervenience base of certain causal structures entails that were the practices relevantly different, the causal structures would be too. Whatever the final word on the analysis of counterfactuals, they are the sorts of propositions which we can often evaluate by scientific standards. We may reasonably ask, in the light of the best available scientific theories, whether or not, for example, the general causal structures of matter would be different if chemists and physicists engaged in different social practices. The answer is "no," and the answer would be "no" for any of the alleged cases of social construction appropriate to N-K constructivist philosophical packages. *That* is evidence for 2N2C, or at least (what is enough) against those denials of 2N2C essential to the constructivist's project.

Finally it must be noted that it is in general difficult to say precisely why

loopy proposals are scientifically unacceptable. Consider for example the hypothesis that social practices in gem-cutting noncausally contribute to the determination of crop yields in Missouri. That is scientifically silly, but it is hard to say exactly why. I suggest that constructivist denials of 2N2C are, scientifically speaking, equally silly, so that the distinctly philosophical arguments for constructivism must be quite strong indeed if the constructivist's philosophical package is not to prove less plausible than the realist's.

A somewhat different sort of objection might at this point be offered against the strategy of scientifically assessing the constructivist's denial of 2N2C. It might be argued that both 2N2C and its denial are philosophical rather than scientific hypotheses and that treating them as scientific hypotheses begs the question against the philosophical arguments in their favor. In support of this contention it might be argued that the dependence of causal structures on social practices posited by constructivists is supposed to be noncausal and that, therefore, scientific considerations of supervenience relations are irrelevant to its assessment.

Against the second and more specific of these objections it must be replied that whatever the nature of the presumed determination, to say of some processes that they are necessarily part of any supervenience base for some structures entails that those structures would not obtain, or would be relevantly different, if the processes did not themselves go on. The counterfactuals of this sort which would follow from plausible N-K constructivist accounts of science do certainly seem to be the sorts of counterfactuals that are assessable scientifically, and they seem deeply disconfirmed. I conclude that there is a strong burden of proof on the constructivist to deny that her position entails such counterfactuals or to provide for them an interpretation that makes them immune from scientific criticism.

Against the more general objection it must be insisted that the special cases of the accommodation thesis relevant to any particular N-K constructivist account of actual episodes in the history of science *are* scientific hypotheses, as are the scientifically dubious counterfactuals entailed by that account in the light of those special cases. Thus it appears that the details of any particular constructivist package will be vulnerable to the charge of inconsistency with well-established science whatever the status of the most general formulations of constructivism.

It might be thought that even particular cases of 2N2C are too philosophical to be well confirmed as scientific theses and that the embarrassing counterfactuals are likewise too philosophical to be evaluated by scientific standards. Even so, the prima facie requirement that philosophical packages be articulated so as to cohere with well-confirmed science is a central methodological standard in the philosophy of science, and the supervenience reduction constraint is an unproblematical special case of that requirement. What our investigation of the relation between 2N2C and well-established science indicates is that—although it is easy to see how the truth of causal claims depends in part on social practices—we have, scientifically speaking, not the foggiest idea of how causal structures themselves could depend on social practices except in mundane causal ways. Precisely because of the scientific inexplicability of the violations of 2N2C which it entails, the constructivist's account of the role of scientific conventionality in determining the truth or falsity of scientific statements fails to meet this constraint, which is clearly met by competing realist (and empiricist) accounts. Thus principles of the unity of philosophical and scientific knowledge which seem central to methodology in the philosophy of science are violated by the details of any N-K constructivist account of actual scientific episodes.

Indeed, there are a number of other considerations which suggest that N-K constructivism may cohere poorly with scientific findings. For example, we have scientific reasons grounded in evolutionary theory to suppose that our capacities are continuous with those of nonhuman animals. Do *they* socially (or otherwise) construct the causal structures of the things they know about? If not, then do we construct those structures, and how are our constructs related to their perceptual abilities? If their causal world is unconstructed, how is it that ours requires construction? . . . (You get the idea.)

Similar concerns arise about the coherence of N-K constructivism with the best-established findings of historians of science. There is a long tradition of holding that Kuhn's acknowledgment of the historical phenomenon of ineliminable *anomalies* within paradigms compromises any metaphysical understanding of his metaphorical claim that scientists who accept different paradigms study different worlds. We are now in a position to make that criticism precise and to show that it is applicable to dialectically complex versions of N-K constructivism as well as to less complex versions.

What seems evident historically is that not every effort at world construction can succeed. Certain conceptual frameworks, metaphysical conceptions, and methodological approaches will not result in the successful establishment of a tradition of inquiry because, in some sense or other, the world fails to cooperate: problem solutions of the anticipated sort are not found which are experimentally successful, anticipated success in developing predictive laws is not forthcoming, the results of efforts to articulate explanations for relevant phenomena do not result in a coherent picture of how they work, . . . Similarly, as anomalies show, apparently successful world construction can hit snags: new discoveries can pose challenges insoluble within an established paradigm.

Now, different degrees of dialectical flexibility in one's account of world-constituting conventionality will affect just which cases of world construction one would have to diagnose as failing in one or the other of these two ways, but no one thinks that scientists or others can impose just any metaphysical picture (however dialectically flexible) on the world. Feyerabend (1989) has termed the constraints which the world imposes on paradigms "resistance."

Now resistances have interesting properties. They seem to be independent of human social practices at least in this sense: that such practices seem to make no noncausal contribution to them. They appear to underwrite counterfactuals: it is not just true that some episodes of attempted world construction have met with resistance, others would meet resistance if they were attempted. Finally, successful theory construction and successful methodology require accommodation to the structure of resistances. Resistances, that is, are a lot like the theory-independent causal structures posited by realists and empiricists: the only obvious difference seems to be that N-K constructivists believe in them.

Resistances are an apparently well-confirmed feature of the history of science, and they pose a challenge to any N-K constructivist package that acknowledges them. Why, given that human social practices can construct, in broad outline, the causal relations scientists study, do they leave unaffected resistances, which look so much like causal structures? Indeed, what is the justification for denying that resistances *are* theory-independent causal structures, and for denying that, in accepting it that scientific theories and methods must be accommodated to resistances, a philosopher has already accepted a realist (or empiricist) interpretation of the accommodation thesis?

I am inclined to hold that causal structures—or at any rate the causal structures accessible in scientific investigation—*just are* the resistances which history teaches us to acknowledge; or perhaps that the causal structures scientists study are the substrate of such theory-independent resistances. Whether or not this particular analysis can be sustained, the fact remains that anomalies and other resistances represent apparent features of scientific practice which are enough like unconstructed causal structures and which play a role enough like that assigned by realists and empiricists to such structures as to pose the question of whether or not N-K constructivism coheres with the results of empirical inquiry in the history of science. It is worth remarking that one reservation which someone might have with the identification of causal structures with resistances (or their substrate) is that there would remain the question of how to distinguish between those features of established scientific theories which reflect the structure of resistances and those which are reflections of conventionality in the broad dialectical sense. N-K constructivism might be seen as gaining some support from a recognition of the difficulty of detecting such conventionality. I discuss the connection between constructivism and the problem of hidden conventionality below (see section 5.4).

I conclude that there are good reasons to hold that N-K constructivism fails to meet adequately the criterion of coherence (or perhaps even consistency) with the findings of the various special sciences and of the history of science and that the philosophical arguments in its favor would have to be very strong indeed in order to overcome the resulting philosophical implausibility. I suggested at the beginning of this paper that N-K social constructivism is often conflated with debunking constructivism. Here is an additional reason to suspect such a conflation: it seems possible to maintain, even from a realist (albeit not a *scientific* realist) perspective, the debunking conclusion that scientific "truth" is merely a social construction; it is much harder indeed to maintain, with the N-K constructivist, that scientific *truth* is a social construction. I suspect that one reason why the depth of the difficulties facing the latter position have not always been recognized has been a failure to distinguish clearly enough between the claims of debunking and N-K constructivism.

5.2. Defending Innocence, Part 2: Conventionality and the Equifertility of Methods

I have argued that constructivism fails to meet the constraint of coherence with well-established science. Turning now to the other fundamental constraint identified in section 4.3, I propose to argue that the rejection of 2N2C undermines the possibility of rationalizing a central and ubiquitously applicable methodological principle having to do with the methodological import of conventional or arbitrary features of scientific description. Recall that it is uncontroversial that there can be instances of unobvious conventionality in scientific practice and that the accommodation thesis dictates that theoretical considerations properly govern inductive practice only to the extent that they reflect knowledge of relevant causal structures. It will be useful therefore to ask what good scientific method dictates when features of well-established scientific theories are shown to be unexpectedly conventional or otherwise arbitrary.

Let us say that the choice between two theoretical conceptions is *arbitrary, or conventional in the broad sense*, just in case what would count for the appropriateness of choosing one over the other would be facts about the history and current practice of the relevant scientific community rather than anything that obtains independently of that history or practice. Simple or dialectically complex conventionality in science, whether obvious or not and whether "world-constituting" or not, will be reflected in there being a possible alternative to the actually accepted conception such that the choice between them is conventional in this sense. What is the methodology appropriate to the discovery of unexpected conventionality in a body of scientific research? I suggest that the principle that is actually central to scientific practice is the following:

The Methodological Equifertility Principle. Suppose that the choice between two conceptions is conventional in the broad sense. Then the only methodological practices which will be properly justified by the acceptance of one of these conceptions will be those practices which would also be justified by the acceptance of the other.

Corollary. Suppose that two conceptions are sufficiently different that they appear to provide competing accounts of some phenomena and to have, in consequence, different methodological import. Suppose further that the choice between them is in fact conventional in the broad sense and that this fact comes to be known. Then, the methodological import of those conceptions must be reevaluated according to the principle that the only methodological practices that will be properly justified by the acceptance of either will be those practices which they dictate in common. Practices which, prior to the discovery of the unexpected conventionality, were taken to be justified by one of the conceptions and not the other must be understood to be justified by neither.

Two examples will illustrate the application of the equifertility principle. According to Lewontin (1976), Jensen (1968) presents as evidence for the genetic determination of individual differences in intelligence the fact that the distribution of IQ scores in typical human populations is a normal distribution. Since a normal distribtion is characteristic of certain polygenically determined traits, the normality of score distributions for IQ is taken as evidence that intelligence is such a trait. A number of criticisms can be made of this line of reasoning; one is that the normality of IQ score distributions is an artifact of practice of test designers: they design batteries of test questions in order to obtain normal score distributions. Once this fact is recognized, the normality of such score distributions ceases to have evidential bearing no matter what relations normal distributions may ordinarily have to underlying genetic facts. Operative here is the equifertility principle: the standard conception of how to measure intelligence is shown to be one of several conceptions between which the choice is conventional in the broad sense, but the proposed strategy for establishing evidence about genetic determination of intelligence differences is ratified by only some of these conceptions.

Consider now the case of alleged unobvious conventionality mentioned earlier in this paper. According to cladists, there is a deep level of conventionality in the definitions of higher taxa of which traditional systematists were unaware. Some cladists put this claim in an especially strong way by maintaining that the only nonarbitrary constraint on the erection of higher taxa is that the taxa themselves be monophyletic. Consider now research strategies in the study of macroevolution. Researchers interested in how the pace of evolutionary change has varied between different intervals in geological time have often proposed to assess such variation by estimating, for such intervals, the number of higher taxa at various levels which have either emerged or have become extinct during them.

Suppose now for the sake of argument that the strong cladist claim about the arbitrariness of higher taxa is true. In that case, of course, calculations of the rates of emergence and extinction would produce entirely arbitrary results and would thus be irrelevant to the study of evolutionary forces. Again the operative methodological principle is equifertility: different classificatory conceptions between which the choice is conventional in the broad sense would dictate entirely different numerical measures of the rates of evolutionary change.

Equifertility seems to be a fundamental methodological principle regarding conventionality or arbitrariness in scientific descriptions. Indeed, we can use it to provide a kind of methodologically relevant "measure" of the extent to which features of such descriptions are arbitrary. By the *methodological spectrum* of a theory let us mean the class of methodological judgments which (given prevailing background theories) it properly underwrites. The equifertility doctrine entails that two theories between which the choice is conventional in the broad sense will have the same methodological spectrum. In consequence, the claim that a theory is unexpectedly arbitrary in particular respects entails that its methodological spectrum is narrower than prevailing methods would suggest; competing claims regarding respects of arbitrariness will thus entail different conceptions of a theory's methodological spectrum, and these differences provide a measure of sorts of the methodological import of the differing estimates of arbitrariness (see Boyd 1990b).

Moreover, there do not seem to be any limitations to the applicability of the equifertility principle: good scientific method seems to dictate that we reject methods that are artifacts of social convention or other idiosyncratic features of our community's history. Nevertheless, if the accommodation thesis is accepted, then it follows that the acceptability of any instance of equifertility is equivalent to the acceptability of the corresponding special case of 2N2C. Thus the constructivist appears to be in the position of being unable to provide an account of scientific knowledge which ratifies a central principle of scientific methodology. She must acknowledge exceptions to 2N2C and thus corresponding exceptions to equifertility.

On no plausible account can all social conventions in science be world-constituting, and thus the constructivist will have to distinguish between cases in which 2N2C holds and cases in which it fails. Given the scientific inexplicability of any such failures, the prospects are dim that she will be able to offer a satisfactory account of the difference between the two sorts of cases. The fact that the constructivist must also rationalize a corresponding distinction between applications of equifertility makes the prospects for her success even fainter.

I conclude, therefore, that N-K constructivism fails pretty spectacularly to satisfy the requirement of coherence with the findings and methods of the best science. One additional concern about authorial intent is raised by the arguments I have offered for this conclusion. Some philosophers have objected to those arguments on the grounds that the authors of Neo-Kantian conceptions of social construction clearly *intended* to appeal to a kind of social construction that is prior to scientific theorizing about causation or about method in a way that would make scientific critiques inappropriate.

I agree that authorial intent has been correctly assessed here, but the question we have been addressing is whether or not there is a sort of social construction with the features N-K constructivists require. After all, phenomenalists intended to appeal to a conception of the reducibility of physical objects to sense data which would not compromise our ordinary conception of the causal relations involved in perception nor compromise methodological commitments that rest on a notion of independent observation of the same object by several researchers. Recognition of that intent does not, by itself, give us any reason to reject the arguments that suggest that no such reduction exists. A similar situation exists with respect to N-K constructivism. Constructivists make claims about the metaphysical import of human practices that when taken together with other claims about science with which they agree appear to contradict 2N2C. That gives us a good reason to doubt that the sort of social construction they posit happens. The burden of proof lies with the constructivist either to indicate a flaw in the arguments about 2N2C or to provide other philosophical (or scientific) reasons why we should find its rejection acceptable.

5.3. Assessing N-K Constructivism as Epistemology: Philosophical Integration and Species Chauvinism

Pretty plainly the denials of 2N2C entailed by N-K constructivism deeply compromise its capacity to meet well-established requirements of unification with the findings of the sciences; so serious is the shortfall, in fact, that the N-K constructivist's position has much in common with debunking constructivism. Still, coherence with established science and its methods is not the only standard by which philosophical packages are properly assessed, and there is a nontrivial epistemological argument *for* constructivism: that it permits the preservation of a plausible version of inference-rule foundationalism. We need to know whether or not this advantage outweighs the apparent epistemological failings of constructivism, so that it would be appropriate to rethink our understanding of the epistemology of science so as somehow to accommodate (nondebunkingly) the oddities of constructivism.

That the answer is "no" is suggested by three considerations. In the first place, of course, the depth of the failure of N-K constructivism to reconstruct actual science is profound, and this strongly suggests that it is on the wrong track epistemologically.

Moreover, the failures of foundationalism implied by the rejection of inference-rule foundationalism are independently suggested by other naturalistic developments in epistemology. The whole thrust of reliabilist accounts of more commonplace cases of knowledge is that what is decisive in distinguishing cases of knowledge from other cases of true belief is not the operation of some privileged principles of justification but the reliability of the operative mechanisms of belief regulation. While such an account of, for example, perceptual knowledge does not entail the falsity of modest inference-rule foundationalism, it does enhance the plausibility of its rejection, especially since the naturalistic account of inductive reasoning in the sciences which is apparently provided by the sciences themselves assigns to theory-dependent justificatory methods and procedures a crucial causal role in ensuring that reliability, thus corroborating the traditional intuition that justification is somehow essential in most cases of inductive knowledge. I conclude that a philosophical package that includes a realist and naturalistic account of scientific knowledge has the virtue that its rejection of inference-rule foundationalism coheres well with the results of independently developed naturalistic research in epistemology and the further advantage that it affords us a naturalistic account of the important role of justificatory arguments in induction.

These advantages are supplemented by another that is suggested by our earlier consideration of the peculiar relation of constructivism to evolutionary theory. It has proved very fruitful in contemporary epistemology and philosophy of mind to consider the ways in which psychological and epistemic descriptions can be appropriately applied, either literally or metaphorically, to nonhuman animals or to nonliving information-processing systems. Two things seem clear. First, there is almost no doubt that we should literally attribute knowledge to a variety of different nonhuman animals, not all of them intelligent primates. Second, when we attribute knowledge metaphorically to much simpler animals and simple nonanimal information-processing systems, our extension of epistemic concepts is well motivated: there is much in common between the "knowledge" of such systems and knowledge in humans and more complex animals. Now for none of these nonhuman systems is it plausible to suppose that their knowledge (or "knowledge") rests on their being able to deploy the resources of a priori justifiable inductive methods or anything of the sort. We thus have philosophical as well as evolutionary reasons to be concerned about a kind of species chauvinism in our epistemological thinking: what reason have we to think that for us alone knowledge is to be understood in terms of epistemically privileged principles of induction? I suggest that the answer is "none."

I do not mean to suggest that if apparently adequate inductive rules of this sort were discovered—or if their existence were strongly suggested by examinations of scientific practice—then we should reject the proposal that they should set epistemic standards for creatures like us capable of understanding them. Nor do I suggest that we should leave unexplored the hypothesis that approximate adherence to those rules explains the special inductive successes of the sciences. What I do suggest is that, in the absence of any evidence that such rules exist, we should favor philosophical packages that incorporate a scientifically grounded naturalistic and anti-(inference-rule)-foundationalist treatment of scientific knowledge over packages that salvage foundationalism at the expense of scientific plausibility. I conclude that when we weigh the case for N-K constructivism provided by the basic epistemological argument against the contrary case arising from considerations of the quasi-naturalistic constraint and the plausibility of 2N2C, the case against constructivism is quite strong. I suggested in section 3.1 that there were three and a half arguments for constructivism of which the fundamental epistemological argument was the first. It is time to turn our attention to the other two.

5.4. *Hidden Conventionality and the Case for Constructivism*

It is unproblematic that there could be — and all but unproblematic that there are — features of our current scientific conception of the world that are conventional in the broad sense but that appear to us to represent discoveries about causal structures. We lack altogether certain methods for ferreting out such hidden conventionalities, and this fact seems to underwrite N-K constructivist convictions for at least some students of the philosophy and social studies of science. In a way this might seem strange since fallibilism regarding questions of social construction hardly justifies social constructivism, especially of the Neo-Kantian variety. Still, there is a point to the concern: scientific realism is, characteristically, a position of those who are inclined to accept the findings of the various sciences "at face value," and the arguments for it turn on accepting for the most part the naturalistic accounts of the reliability of scientific methods which are confirmed by the application of those very methods. A serious enough skepticism about our ability to uncover hidden conventionality would cast doubt on the realist's case. We need more than mere fallibilism, however — all the more so because realist approaches provide some resources for distinguishing mere conventions from real "maps" of causal structures (for example, count as probably nonconventional those features of received background theories which clearly seem implicated in reliable methodology: see Boyd 1990b). We need some special reason to suppose that philosophers generally, or at any rate realist philosophers, will tend to make significant mistakes about what is conventional or merely historical and what is not. I believe that those who worry about hidden conventionality typically have one or both of two different special concerns of this sort in mind. One is a matter of assessing the prospects for *experimental metaphysics*, the other a matter of concern over *hidden politics*.

Experimental metaphysics first. Positivists called "metaphysics" any theorizing about the unobservable, and they held that experimental knowledge of "metaphysics" is impossible. If realism is true, then scientists routinely do experimental "metaphysics," and they often do it successfully. What about experimental metaphysics (without the quotation marks)? Plainly it has been an influential view among realists that scientists do successful experimental metaphysics as well: witness the widespread view among realist philosophers of science that materialism has been confirmed as a scientific hypothesis. One plausible concern with this enthusiasm for experimental (no quotes) metaphysics might plausibly be that we run the risk of treating as metaphysically informative features of scientific theories which are in fact merely artifacts of the conceptual history of the relevant scientific communities. If we hold, with the realist, that *physical* scientists—biochemists, molecular geneticists, and pharmacologists, let us say—have discovered something(s) unobservable and important about the biological and even the mental world, and if we agree that they have done so by employing a materialist research strategy, one that could be and is defended by claiming that all phenomena—mental as well as biological—are physical, *still* need we conclude that it is the materialist theoretical formulation of their perspective which captures their insights about the relevant causal structures? Could not the materialistic thesis that these scientists, or our rational reconstructions of them, affirm be conventional? Could there not be a rationalization of the same methods for studying (admittedly partly unobservable) causal structures which had no materialist philosophical implications? Might a sort of *scientism* not blind *scientific* realists regarding this question?

I think that questions such as these pose interesting problems for the defender of experimental metaphysics but that N-K constructivism is the inappropriate position for the philosopher who has the concerns in question. The worry, after all, is that we may not be able to determine reliably just which elements of our best-confirmed scientific theories are really conventional in the broad sense. But the proposed solution is to adopt a general solution to that difficulty: to hold that it is *always* the features of our theories which define the basic metaphysical picture which are conventional (that is, after all, what N-K constructivists hold). Moreover, this solution seems to have the opposite of the desired methodological import with respect to experimental metaphysics. If we are always justified in taking the basic metaphysical picture presented by the sciences as reflecting socially constructed *reality* (which is supposed to be, of course, as real as things get), then we *are* justified in, for example, taking materialism to be a well-established scientific finding. What the critic of experimental metaphysics raises is the possibility that the metaphysical-looking doctrines reflected in scientific theorizing are *merely* conventional, where that status deprives them of real metaphysical import. Since the defining feature of N-K constructivism is that it attributes metaphysical import to just the sorts of conventions at issue, we have again a case in which N-K constructivist doctrine is invoked where a limited sort of debunking—of just the sort precluded by N-K constructivism—is needed instead. As we shall see, this pattern continues.

On to politics. A central concern of many scholars (not just professional philosophers) who are attracted to N-K constructivism is to elucidate the often hidden role of ideology in science. When scientific ideology is effective, it is invisible: a hidden political element determining the content of scientific theorizing. It is effective, that is to say, because there are features of social practice whose influence on the content of scientific theories is unobvious. Struck by the overwhelming evidence that such hidden politics is a standard

feature of much of scientific life, many scholars have been led to adopt an N-K constructivist conception of scientific knowledge. Once again, the oddity of the position is evident when it is recognized that their aim is a critical one.

Consider a case of ideological factors in science, say the "social construction of gender." It was an all but uniform feature of nineteenth-century biological thinking to affirm the intellectual inferiority of women; that is ideology in science. How will adopting an N-K constructivist view of nineteenth-century biology help us criticize this ideology? Well, first, it is clear that the principal explanation for the uniformity with which this doctrine was accepted involves the operation in science of historically determined social practices toward which the critic has an unfavorable attitude. The influence of these practices is hard to detect—just like the influence of world-constituting conventions in science. Are the social practices that determined the doctrine of the inferiority of women themselves to be thought of as world-constituting? If not, then it is hard to see why an N-K constructivist conception should be especially important to their criticism, since the standards for the epistemic and political criticism of non-world-constituting social practices are presumably the same for the realist and the N-K constructivist.

Suppose, then, instead that the social practices are to be understood as world-constituting. In that case, the critic will be obliged to hold that it was true (by social construction—but that is as true as things can be) that nineteenth-century women were intellectually inferior in the way indicated by the relevant biological theories. Now, this is a conclusion which someone independently committed to N-K constructivism might be obliged to accept, but it could hardly be taken to indicate that N-K constructivism facilitates the criticism of ideology. Here again, thinkers who have adopted an N-K constructivist conception seem to have been looking instead for a conception of the relevant conventions which denies them metaphysical import. It is a debunking constructivist treatment, if not of nineteenth-century biology in general, then of nineteenth-century biology of sex differences, which is recommended here, not N-K constructivism.

One remaining political application of N-K constructivism needs to be discussed here. In some cases of the ideological role of science—the social construction of gender is an example—the subject matter of the relevant sciences is *us*, and it is important to understand the extent to which scientific practices in such areas may determine what we are like. Theories of sex differences frame social and educational expectations, self-images, legal and economic possibilities, and so on, so that the nature of men and women is in a deep sense *socially constructed*. Some thinkers, struck by this fact, and concerned to emphasize its importance, understand the social construction of gender, for example, on the N-K model of the social construction of reality. Two considerations suggest that this is a mistake.

In the first place, of course, the social construction of gender roles facilitated by, among other things, sexist ideology in science, is *causal*: social practices in science are among the factors that cause other social patterns that cause men and women to exhibit certain psychological dispositions more often than others which they would exhibit under different circumstances. In the absence of an entirely independent argument, there is no reason to assimilate these causal relations to the model of noncausal determination of causal structure by theoretical practices.

Moreover, noncausal social construction—even of the dialectically complex sort—*cannot fail*: the whole idea is that certain social practices *impose*, in something like a logical or conceptual way, a certain general causal structure on the world. But social constructions of the causal sort often fail spectacularly at particular historical junctures. The social construction of the inferiority of colonial subjects (which was, of course, accomplished more with troops, guns, whips, and courts than with scientific theories) eventually produced rebels, not persons genetically suited to be ruled. Although no one doubts this, thinking of *causal* social construction on the model of Neo-Kantian *noncausal* construction focuses attention on its successes rather than on the conditions of resistance. It is hard to see how that would enhance the prospects for a critique of ideology.

I conclude that general considerations of the unobviousness of the influence of (some) social practices in science, although important, do not tend to support N-K constructivism.

5.5. Scientific Pluralism and Nonreductionist Materialism

Two quite specific forms of the social determination of the structure of scientific theories are often cited as providing reasons for N-K constructivism. In the first place, it seems certainly true that for any given scientific discipline, there will be more than one conceptual scheme that could be employed to capture adequately the knowledge reflected in its theories. There is thus a significant measure of conventionality in the broad sense involved in the acceptance of whatever conceptual framework scientists in a given discipline employ.

Moreover, between scientific disciplines there are variations in the schemes of classification and description which are appropriate even when—in some sense—the same phenomena are under study: economists and sociologists must employ different explanatory categories even if they are both studying consumers. The naturalness of concepts and the appropriateness of methods seem to be interest-dependent—to depend on the interests of the investigators.

Each of these instances of pluralism in science has been taken to provide evidence for N-K constructivism or related positions. In the first case, the conventionality involved in choices of conceptual schemes is assimilated to world-constituting conventionality on the N-K constructivist model; in the second, the interest-dependence of kinds and methods is taken to indicate the

sort of mind-dependence of reality congenial to constructivists but not to realists.

I have discussed these cases at some length elsewhere (Boyd 1980, 1985a, 1989). What is important here is that the plurality of conceptual schemes exemplified in the two sorts of cases, far from representing a challenge to realism, is predicted and fully explained by a realist conception of scientific knowledge. Consider first the plurality of conceptual schemes within a single discipline. It is a truism that when we employ a relatively small finite vocabulary to formulate descriptions of complex systems, the respects of similarity and difference which ground the definitions of the primitive terms we use will not exhaust the explanatorily or predictively important respects of similarity and difference. The remaining explanatorily important distinctions must be captured by more complex descriptions generated from the basic vocabulary. Thus there will always be some arbitrariness—some conventionality in the broad sense—in the choice of conceptual frameworks in any complex inquiry.

This truism is uncontroversial and it certainly poses no problem for the realist who holds that the respects of similarity and difference involved are reflections of socially unconstructed causal structures. (Perhaps it poses a problem for the constructivist—Why don't we just socially construct a simpler world?—but that's not the issue here.) Thus the conventionality of choice of conceptual schemes is apparently something which the realist can cheerfully acknowledge. It is true, of course, that such conventionality raises methodological problems for realist friends of experimental metaphysics: one must somehow be sure that one's metaphysical lessons are not drawn from features of scientific theories which are conventional in this way. But that is a problem for realists in their experimental-metaphysician moods, not a problem for defenders of 2N2C.

Consider now the interest-dependence of conceptual schemes. In a causally complex world the respects of similarity and difference in causal powers which are predictive or explanatory of one sort of phenomenon (or of certain aspects of a given sort of phenomenon) will not typically be those which are important for phenomena of different sorts or for different aspects of the same phenomena. Thus it is unsurprising that the vocabulary and conceptual schemes suited to one sort of inquiry will usually be unsuited to inquiry with different explanatory or predictive aims. Here again there is nothing to trouble the realist. The appropriateness of a scheme of classification depends on the purposes or interests in the service of which it is to be used, but there is nothing here to indicate that the causal structures which the various conceptual schemes map out depend noncausally on human interests and desires or on social practices. That conceptual schemes are "mind-dependent" in the way indicated suggests nothing Kantian or Neo-Kantian. There is no threat to 2N2C.

There remains one additional route to N-K constructivism along similar lines. Scientific realism does open up the possibility of scientific metaphysics, and most scientific realists are materialists—either materialists generally, or at least materialists about the subject matters of the various special sciences including psychology. It may be reasonably argued that in the present dialectical situation plausible realist philosophical packages will embody a commitment to materialism. If this is conceded, then it follows that the realist will be obliged to offer a materialist interpretation of each of the plurality of conceptual schemes appropriate to scientific inquiry. This requirement, it might be argued, fatally compromises the realist's endorsement of conceptual pluralism—a materialist interpretation of a theory or conceptual scheme must be reductive, so the realist must hold that the conceptual resources of any scientific discourse are ultimately reducible to those of some standard version of physical theory.

The objection is cogent just in case it is impossible for the realist to defend a nonreductionist understanding of materialism. There is a certain irony here. A nonreductionist understanding of materialism is available to the realist *but not to the empiricist or to the constructivist*. Here is why: Materialism asserts that all phenomena (or all phenomena in the relevant domain) are composed of physical phenomena. In particular it asserts that all causal powers and mechanisms are composite from physical causal powers and mechanisms. For the empiricist such causal talk must reduce to talk about the deductive subsumption of the relevant laws and lawlike generalizations under the laws of physics, and that in turn requires (in consequence of Craig's theorem) that the vocabulary of those laws and generalizations be reducible to that of the laws of physics.

Similarly, for the constructivist, physical (biological, psychological, historical . . .) causation is socially constructed in the practices of physicists (biologists, psychologists, historians . . .), so to say that the causal powers or mechanisms operating in some other discipline are composite from physical powers or mechanism is to say that there is a reductive relation of some sort between the concepts and practices of the other discipline and those of physics.

On a realist understanding, by contrast, causal powers, mechanisms, and the like are phenomena conceptually and metaphysically independent of our conceptual schemes, and the ways in which powers, mechanisms, particles, and so on aggregate to form composite phenomena is not a conceptual matter but a matter of the theory-independent causal structures of the relevant phenomena. Thus on a realist analysis materialism is not in need of, and does not possess, a reductionist analysis of the sort at issue (I develop this and related themes in Boyd 1985b, 1989).

Thus if realists should be materialists (despite the methodological difficulties with experimental metaphysics discussed earlier, I think they should), they are entitled to formulate and defend philosophical packages that provide a nonreductionist understanding of materialism, one compatible with a plurality of mutually irreducible scientific conceptual schemes. Incidentally, since both materialism and the mutual irreducibility of theoretical conceptions in science are independently attractive positions, the capacity of realism to accommodate them both when empiricism and constructivism cannot is an additional point in its favor.

5.6. *Cultural Pluralism: Alternative Conceptions of Tolerance*

Sophisticated constructivism reflecting a dialectically complex conception of conventionality will mirror realism in its treatment of semantic and methodological commensurability for standard cases in the history of science, but the sophisticated constructivist has an option not open to the realist. Whenever two traditions of inquiry are sufficiently different that there are no compelling arguments for methodological or semantic commensurability, the constructivist is free to diagnose a particularly deep form of methodological and semantic incommensurability: *that which obtains between traditions involved in different episodes of world making*. The availability of this option has often been taken as providing a justification for constructivism on the grounds that its exercise, in some or all cases of the sort in question, provides the appropriate remedy to cultural chauvinism. Where the "Western scientific outlook," say, conflicts with that reflected in the tradition of some preindustrial tribal culture, an analysis according to which the two traditions represent different episodes of world making precludes on our part any sort of condescension based on the conviction that participants in the other tradition are irrational or fundamentally wrong. Both rationality and truth are differently constructed in our two traditions.

It is important to see what is *not* at issue here. In the first place, it is not at issue that sometimes, when there is a translation scheme that appears to establish semantic commensurability between two traditions of inquiry, there will be a better semantic conception that diminishes the apparent disagreement between the traditions perhaps at the expense of semantic commensurability. It is fully compatible with realism to hold for example, about an apparent disagreement between a Western physician and a tribal medical practitioner, that the tribal terms initially translated as "disease" and "cure" really have different meanings *and different extensions* than the English terms offered as their translation, that their meanings and extensions are not expressible in English, and that when properly understood the tribal practitioner's views are more accurate than they appear to be on the initial translation.

Where the realist's and the constructivist's options differ here is that their accounts of the semantics of the relevant languages and of the accuracy of the different theories are subject to different constraints. The constructivist may cheerfully hold that some tribal term "d" means "conditions caused by demons," has as its extension the set of conditions that are so caused, and has a non-null extension—all of this in the world socially constructed by the relevant tribal practice. The realist could say the same things only if she could defend a philosophical package in which the existence of demons is somehow reconciled with the apparent scientific evidence against their existence—all this, of course, in the single world which she and both practitioners study. Thus while the strategy of attenuating apparent disagreements between traditions of inquiry by diagnosing appropriate failures of semantic commensurability is available to both realists and constructivists, its applications are considerably more constrained for the realist.

More importantly, there is no issue about the *cultural relativity of rational justification nor any issue about the extent of its applicability*. Here is why: Both realists and constructivists accept the accommodation thesis and the associated critique of the hope for theory-independent methods of empirical investigation. They must agree that, insofar as rationality is a matter of epistemic responsibility, rationality is exhibited by the conscientious application of culturally transmitted standards of reasoning and of epistemic practice. At least for a person with significant exposure to only one cultural tradition, there are no other possible standards for the assessment of her epistemic responsibility. Moreover, and this too is dictated by any rejection of the existence of theory-independent methods, even cosmopolitan agents with experience of more than one culture are obliged to assess conflict in cultural standards from a perspective somehow derived from their primary theoretical and practical commitments. There just are no other rational standards to apply.

Thus neither the realist nor the constructivist lacks the resources for explaining, in any case of conflicting cultural standards of rationality, why it would be inappropriate to take such a conflict as indicative of a failure of rationality—or of intelligence, or of any other cognitive or moral virtue—on the part of participants in the other culture. Only the empiricist who believes in a priori justifiable theory-neutral standards of rationality lacks such resources—and perhaps only a caricature of an empiricist, since any philosopher who believes that such standards exist will surely hold that their discovery would require developments in statistical theory of sufficient complexity that it is to no one's discredit as a rational agent not to have lived in a culture in which they have been achieved. Almost certainly the main antidotes to chauvinist diagnoses of the irrationality of other cultures are political rather than philosophical, but insofar as philosophical remedies are sought, they are as readily available to the realist as to the constructivist.

What the realist cannot do—as the constructivist can—is to offer an account of the relation between traditions of inquiry which guarantees that neither *could be* better than the other at mapping causal structures or metaphysical reality because they represent independent instances of world construction. Thus a certain sort of guarantee of tolerance is available only to the constructivist. Should this count in favor of the constructivist perspective?

Of course, if the availability of this sort of ontological tolerance is seen as advantageous, its advantages will have to be weighed against the numerous

philosophical disadvantages of N-K constructivism already diagnosed. But it is not in any event obvious that there is an advantage at all. If being a constructivist is never having to say they're wrong, it is never having to say we're wrong either. If the basic metaphysical presuppositions of any framework of inquiry are taken to be basically correct by convention, then this is true of one's own framework, and a certain conception of open-mindedness — being willing to consider the possibility that others' conceptions are in some ways superior to one's own — is compromised.

The latter claim may be made precise. The following principle — call it the *insight thesis* — is a consequence of the accommodation thesis:

> Suppose that a body of research practice within a research tradition has proved systematically successful in achieving some sort of knowledge. Then its success provides good evidence that the theoretical principles and methodological practices that have governed that research reflect an insight into *the causal structures of the phenomena under study.*

This thesis is common to constructivists, realists, and sophisticated empiricists, but its interpretation depends crucially on the philosophical perspective from which it is advanced. For either a realist or a sophisticated empiricist, the causal structures referred to are features of the unique actual world, whereas for the constructivist the reference to causal structures in the formulation of the insight thesis is reference to causal structures in the world socially constructed by the research tradition within which the successes in question occurred. In the light of these differences consider the following:

> *An Antichauvinist Principle for Projectibility Judgments within a Research Tradition T.* Suppose that it is discovered about a tradition T′ other than T that (a) T and T′ share to some extent a common subject matter and (b) inquirers (or practitioners) in T′ possess skill or sophistication about some theoretical or practical issues concerning that common subject matter roughly comparable to that of inquirers and practitioners in T. Then, prima facie, the doctrine that the theories employed by workers in T′ in their successful endeavors embody an approximation to the truth about the causal structures of the phenomena that make up that common subject matter must be counted as projectible in T.

> *Corollary.* The discovery of the relevant sort of commonality of subject matter with a sophisticated tradition makes relevant features of that tradition *internal* to the tradition within which the discovery of commonality takes place. The recognition of relevantly sophisticated traditions alternative to T sharing a common subject matter dictates a corresponding "open-mindedness" within T *even when the two traditions are such that methodological commensurability between them fails to hold.*

Each of these principles is entailed by the insight principle, but for constructivists—and not for realists or sophisticated empiricists—their application is restricted to those cases in which the traditions T and T′ are part of a common episode of the social construction of reality. In precisely those cases in which constructivism is supposed to provide an antidote to chauvinism — those in which the constructivist portrays the apparently competing traditions as embodying different episodes of world construction—the force of the open-mindedness principle is lost. The cost of metaphysical insurance against treating other traditions as mistaken is immunity from the requirement that one take them seriously. Even if it were not for the deep technical difficulties with N-K constructivism, it is not clear that this would be the version of cultural tolerance to endorse.

5.7. Realism and Unity of Knowledge: Concluding Scientific Postscript

One line of argument in metaphilosophy has it that a generally naturalistic conception of the subject matter and methods of philosophy is appropriate. Naturalistic conceptions are correct in epistemology, semantic theory, metaphysics, and ethics, and the reason they are correct is that philosophy is one of, or at any rate is continuous with, the empirical sciences. (It usually goes with this position to remind the reader that the empirical sciences are not what empiricists thought they were.) I am inclined to think that *something* like this is right, but the interesting task is to say just what it is. The results of our inquiry into the relative merits of realism and N-K constructivism provide some indications of an answer.

In the first place, if inference-rule foundationalism is seriously mistaken, as it appears to be, then the accommodation thesis or its analogue will hold about all or almost all branches of knowledge. Whether this entails naturalism in epistemology or not, it certainly entails that the epistemology of inquiry in any field must be grounded to a significant extent either in the findings of that field or in a substantive critique of its findings and methods. (The epistemology of morals must be grounded to a significant extent in moral theory, or in a critique of moral theory and its methods, and similarly for social sciences, theology, aesthetics, etc.) Insofar as the various areas of human inquiry are interconnected, epistemological theories must satisfy a requirement of integration with the best-substantiated results of all of the various areas of inquiry.

Similarly, once the possibility of experimental metaphysics is acknowledged, any sort of human inquiry must be seen as potentially relevant to metaphysics, and thus metaphysical theories too must face the requirement of integration with the rest of our knowledge. What seems dictated is that philosophy—along with all other disciplines—is properly governed by a principle of unity of inquiry analogous to the principle of unity of science proposed by empiricist philosophers of science: the results of inquiry in any area are potentially relevant to the assessment of the results in any other.

This principle of unity of inquiry seems philosophically attractive; indeed, it seems to capture much of the motivation for philosophical activity. It has, however, the consequence that—even when the relations between disciplines are understood nonreductively—there is some limit to disciplinary autonomy. This fact has provided for some a motivation for a particular kind of N-K constructivism which portrays various contemporary disciplines as reflecting independent episodes of world construction. Often the aim is to save, for example, the social sciences, the arts, literature, history, morals, or religion from the threat of scientific criticism ("the imperialism of physics"). Reflection on the nonreductionist character of (realist) materialism will indicate, I believe, that neither the social sciences, nor the arts, nor literature, nor history, nor morals are in any way challenged by the sciences. (For the crucial case of morals see Sturgeon 1984a, 1984b; Miller 1984; Boyd 1988; Railton 1986.) In the case of most orthodox religion, by contrast, there does appear to be a conflict with apparently well-confirmed materialism.

Should N-K constructivism be accepted in order to save religion from scientific critique? The myriad metaphysical and epistemological difficulties facing the articulation of constructivist philosophical packages suggest that the answer must be "no." So too does the fact that constructivism seems in general ill-suited for the defense of open-mindedness. Finally, the denial of the full applicability of the principle of unity of inquiry seems especially inappropriate for the defense of traditions of inquiry with aims as synoptic as those of traditional theology. I conclude that a respect for the integrity of the aims of theology as well as other deep philosophical considerations precludes such a move. The scientific challenge to religion cannot be made to go away.

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EIGHT

Do We Need a Hierarchical Model of Science?

Diderik Batens

According to hierarchical models of science, our scientific knowledge in the broadest sense, including descriptive as well as methodological and evaluative statements, forms a knowledge system or is embedded in a larger knowledge system that has two properties: (i) it is stratified, and (ii) the items of some layer are or should be justified in terms of items of a higher layer. Hierarchical models are deeply rooted in Western culture in general. They are both viewed as describing the natural order in a variety of domains and as outstanding problem-solving environments.[1] Most past philosophers explicitly or implicitly favored hierarchical models. The vast majority of those who view science as a rational enterprise will, if pressed, opt for a hierarchical model. Even those who reject hierarchical models often retain many of their aspects.

I hope to show, first, that hierarchical models are affected by a number of difficulties—I shall be brief on this well-known point—and, next, that we need not try to repair them because there is a much more attractive alternative which I shall try to spell out and argue for. The alternative is the "contextual" approach to meaning and knowledge, embedded in a relative-rationality view. I deal with only a few aspects of this approach here and refer to other publications where necessary, but I have tried to make the present text as self-contained as possible.[2]

I begin with a historical remark. In section 2, I indicate some major difficulties of the hierarchical (and the holistic) model with respect to justification. The third and fourth sections are devoted to two central features of the contextual model. For the sake of expository clarity, I postpone the discussion of some more fundamental problems to the final section.