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Feminism and Naturalism: If Asked for Theories, Just Say 'No'

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Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past. The tradition of all the dead generations weighs like a nightmare on the brain of the living. And just when they seem engaged in revolutionizing themselves and things, in creating something that has never yet existed, precisely in such periods of revolutionary crisis they anxiously conjure up the spirits of the past to their service . . . in order to present the new scene of world history in this time-honored disguise and this borrowed language.

—Karl Marx, "The Eighteenth Brumaire of Louis Bonaparte"

Ah, the 1960s. Revolution everywhere. Indeed, the publication within that decade of *The Structure of Scientific Revolutions* and "Epistemology Naturalized" toppled a philosophical Bastille, overthrowing the *ancien régime* within philosophy generally and within the philosophy of science in particular. For the Quinean and Kuhnian turn in science studies signals

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the recognition that nothing but our artifices—not the world, not logic—undergird the cognitive authority of science.

But revolution is never easy. A new order must be legitimated—no simple matter. Shortcuts beckon. The rhetoric of the old regime has the virtue of familiarity. But, as Marx warns, the easy path just leads us back to repudiated practices and institutions. Indeed, the Quinean/Kuhnian turn in philosophy of science, once so promising, stands stalled, locked in a "legitimation crisis," an internal struggle regarding how to justify its own explanatory practices. Legitimation of claims to cognitive authority, like claims in the political sphere, comes most easily when its sources are seen as suprahuman—what the facts dictate or logic compels.

The revolution falters, in other words, because despite their having made a compelling case for taking the naturalizing/historicizing turn, many feminist and naturalist philosophers of science still "anxiously conjure up the spirits of the past to their service." They summon those very spirits the revolution seemingly laid to rest—progress, rationality, objectivity, and truth. Thus, one finds feminists recoiling from charges of relativism, naturalists insisting that they too have real epistemic norms, and everyone paying homage to the constraints that, of course, 'reality' exerts.[1] The revolution cannot have it both ways. The old forms of legitimation cannot coexist consistent with the Kuhnian and Quinean reasons for their delegitimation. A feminist philosophy of science will be best served—its agenda of reform forwarded—by freeing itself from any need of or nostalgia for special philosophical justification.

No small part of the problem here rests, I suggest, with the lack of consensus regarding either the meaning of 'naturalism' or the reading of Quine's now canonical essay. For while "Epistemology Naturalized" unquestionably excited a widespread revival of interest in naturalism, the status accorded the essay is ironic. One finds both friends and foes of philosophical naturalism denying that Quine does establish that the methods of naturalism can accommodate the problems of epistemology.

Diagnoses of the problems vary. Critics insist that a Quinean naturalism either cannot provide norms and so cannot be epistemology (Kim 1988), cannot legitimate its own basic presuppositions and procedures and so is essentially incomplete as an epistemology (Putnam 1982; van Fraassen 1995), or is just armchair speculation and so not interestingly different from the epistemological projects Quine rejects (Foley 1994). Self-described friends of naturalism (Goldman 1986; Haack 1993b) are equally uneasy, for they too doubt that Quine can successfully incorporate the substance of epistemology within the limits of his naturalism.

Is naturalized epistemology epistemology enough? Skepticism here is, I maintain, symptomatic of a pervasive misreading of the main line of argument of "Epistemology Naturalized." Consequently, its moral regarding naturalism remains misunderstood. This misunderstanding, in turn, encourages confusion or vagueness regarding what naturalism is and its sufficiency for the tasks of epistemology.[2]

Section I develops a characterization of philosophical theories generally and naturalist theories in particular. In the sense of "philosophical theory" discussed, naturalism does not constitute, I argue, a philosophical theory. Section II details an interpretation that challenges readings of Quine that construe his naturalism as a type of philosophical theory. My reading renders nugatory problems commonly attributed to Quine's naturalism. Seen aright, Quine's naturalism indeed radically reconceives the epistemological enterprise, but the reconception differs from that which has previously been emphasized. Section III examines how some recent efforts in feminist and naturalist philosophy of science lose their way by failing to be naturalist enough.

I

Bertrand Russell's theory of descriptions is, as Frank Ramsey famously observed, a paradigm of philosophy. The history of analytic philosophy subsequent to the publication of "On Denoting" proves Ramsey right. To anachronistically read Ramsey's remark as a Kuhnian might, Russell's analysis of "The present King of France is bald" or "George IV wished to know whether Scott was the author of Waverley" is paradigmatic in two distinct senses. One is as a problem-solving model. It greatly simplified the assumptions needed to analyze some standard "hard cases."

Yet Russell's analysis is also paradigmatic of a type of philosophical theorizing. For Russell has a philosophical theory of meaning of which the theory of descriptions is but a part. Meaning here becomes a function of a language possessing a particular logical structure, a structure revealed by proper analysis. This theory of meaning is not itself part of any natural science, but presupposes problematic philosophical views regarding "knowledge by acquaintance" and "knowledge by description." The Russellian paradigm reminds us of how a wrong-headed philosophical theory may lurk just below the surface of elegant and seemingly metaphysically pristine formal analyses.

Another important instance of how a logical analysis may overlie a contentious philosophical theory is the verifiability theory of meaning. It merits the title of a "philosophical theory" just because it too purported to explain, without the aid of science, why science works well when it works well.

My aim here is not to offer any general criteria for designating theories as philosophical, but only to note some features that can make them such, features I will later contend characterize (some) work in feminist philosophy of science. Primary among these features is that neither the theory of descriptions nor the verifiability criterion theory of meaning could be falsified by experience. Failure to solve conceptual puzzles represents a difficulty, not disconfirmation. In addition, as is notoriously the case in the various formulations of the principle of verifiability, the demarcation criteria kept coming out wrong. Various schemas invariably excluded from the realm of meaningful statements in science some sentences even positivists wanted to keep. *But the sundry shortcomings only whetted their philosophical appetite, inasmuch as the theories were held for reasons experiments could not touch, e.g., assumptions about the possible sources of human knowledge, the deep structure of natural language, and the requirements of cognitive significance.*

It is important to remember what separates the approach of a Russell and especially a Carnap from those of their empiricist and rationalist predecessors. It is their positive proposal to actually reconstruct the link between existing scientific theories and their empirical base. Somewhat ironically, what we owe to the decades of intensive work especially by Carnap is a deep appreciation of how resistant scientific theorizing is to this specification of its inferential relation to evidence.

This explicitly constructive aspect of the logical positivist project comes finally to define what empiricism is in the twentieth century.[3] "Rational reconstruction," Carnap writes, attempts "for the first time, the actual formulation of a conceptual system of the indicated sort" (Carnap 1967, vi),[4] i.e., our system of knowledge. Reconstruction would be proof positive of long-standing empiricist claims regarding what the "deconstruction" of empirical knowledge must yield. Rational reconstruction would have established the objectivity and rationality of scientific knowledge to anyone's satisfaction.

This defining characteristic of logical positivist epistemological theories is doubly philosophical. On the one hand, it is not tested via experiment; reconstruction is just an exercise in logical imagination. On the other hand, reconstruction provides the justificatory basis, in the best understood sense of that term—a formal logical derivation—of theoretical claims. Logical reconstruction is then a paradigm philosophical claim, a "first philosophy" that is prior to scientific knowledge.

Naturalism is *not* a philosophical theory of knowledge, at least in the senses outlined above. Some, to be sure, have tried to make it so. Naturalism in epistemology can be characterized negatively by its eschewal of any notions of analytic or a priori truths. Positively, naturalism asserts a normative and methodological continuity between epistemological and scientific inquiry.[5] The techniques endemic to the former are only a subset of the historically received and contingently held norms and methods of the latter.

So characterized, naturalism is frequently the object of two complaints. The first finds the positive characterization too vague to be of any use. Bas Van Fraassen, for example, remarks that "[t]o identify what naturalism is . . . I have found nigh-impossible" (van Fraassen 1996, 172). The second suggests that naturalism's link to scientific method precludes naturalism from fulfilling epistemology's normative role. In this version of the Humean is/ought paradigm, science describes, norms prescribe. Hence the latter cannot be derived from the former. Since naturalism has only descriptive scientific methods at its disposal, this forecloses the possibility of its justifying epistemic norms.

How vague is the notion of naturalism? No more vague, I suggest, than our ability to catalog the methods of science. Naturalism, moreover, does not yoke what counts as science to some philosophical characterization. It is ironic, then, to find philosophers such as van Fraassen making continued references to "science," as if they knew exactly what that means, and yet complaining all the while about the vagueness of naturalism.

What counts as a scientific method for naturalists is not itself limited to or defined by one particular science, or driven by a prior philosophical characterization of such. For Quine, as for American naturalists historically, the methods of science include the full panoply of procedures employed in fact-driven research programs in any area of inquiry.

The second complaint above rests on what I term the "naturalist's dilemma." For a naturalist, if philosophy does not utilize the methods of science, then it has no place on the roster of legitimate forms of inquiry. So philosophy could not contribute to a (naturalized) account of knowledge and justification. If it does employ such methods, then one or both of the following problems would seem to obtain. Either epistemological inquiry will be question-begging—it will evaluate the methods and results of inquiry by using some of those very methods or results—or impossible, since one cannot derive epistemologically prescriptive conclusions from empirical (descriptive) inquiry.[6]

Naturalists such as Quine subscribe to what I term "methodological monism."[7] What marks questions as epistemological for Quine is that they are about the processes sustaining and generating scientific beliefs. But he views such questions neither as receiving some distinctively philosophical answer nor as pursued by some special philosophical method.[8]

Three recent efforts to map systematically the peaks and divides within epistemological naturalism—James Maffie (1990), Phillip Kitcher (1992), and Hilary Kornblith (1994)—all situate Quine as a key point of contrast within the spectrum of naturalisms. Maffie offers the historically most comprehensive and philosophically sophisticated classification scheme of the three. But, even within his nuanced philosophical topology, the most basic divide—and the one of interest here—is between what he terms, on the one hand, "limited" and, on the other hand, "unlimited" naturalists. (Maffie 1990, 287ff.) Kitcher signals the split that concerns me by speaking of a distinction between "traditional" and "radical" naturalists (Kitcher 1992, 74ff., but especially 75). Kornblith develops a distinction between what he terms the "weak" versus the "strong" replacement theses (the replacement in question being the replacement of traditional epistemology by some form of psychology). In each case, the first-named category marks a space for those who hold that one can be both a naturalist and retain some separate role for epistemology—methodological dualism. The second category for each, however, marks those who do not conceive of any divide between philosophical and scientific method—methodological monism. All locate Quine in the latter category.[9]

Maffie's way of dividing the territory turns on whether or not one endorses full methodological continuity between epistemology and natural science. "Unlimited naturalism" tries to "fit epistemology into science" and so naturalizes epistemology "all the way up," inclusive of meta-epistemological issues (Maffie 1990, 287). In contrast, "limited naturalism" fits "science into epistemology" (288). Citing Alvin Goldman's work (through *Epistemology and Cognition*) as paradigmatic, Maffie contends that a limited naturalism holds that scientific methods do not play a role in practices such as explicating the meaning of epistemic terms or identifying the correct epistemological norms.[10]

Kornblith speaks, in this regard, of the "replacement thesis" (Kornblith 1994, 4)—the preemption or displacement of epistemological questions by psychological ones. Replacement may be strong or weak. Strong replacement opts for a description of the causal history of belief formation. Kornblith reads Quine as arguing for strong replacement: "psychological questions hold all the content there is in epistemological questions. In this view psychology replaces epistemology in much the same way that chemistry has replaced alchemy" (7). With weak replacement, however, the fields are thought to complement rather than preempt one another's areas of inquiry. In this regard, Kornblith holds that psychology and epistemology each ask different questions and "these questions are approached with different methodologies" (Kornblith 1994, 8). Weak replacement, unlike strong, preserves the autonomy of epistemology.

Kornblith does not indicate here in what "special method" the autonomy of epistemology consists that distinguishes it from the special sciences.[11] He rejects full-blown psychologism because it does not offer the desired transition from descriptions of belief-forming processes to epistemological advice giving. He plumps instead for a more moderate view, which he terms "ballpark psychologism" (10–11). What puts psychology and epistemology in the same ballpark is the antiskeptical assumption that we now do know a great many things (10–11). This antiskepticism implies that at least some of the processes actually used to form beliefs are ones we ought to be using. Reliabilist friends of weak replacement would envision epistemologists identifying what the truth-making features of processes are, while psychologists could investigate actual processes to learn whether or not they possess the desired qualities (7).

For Kitcher, the defining features of naturalism are, first, the resuscitation of psychology (and possibly biology) as relevant to epistemological analysis and, second, the denial of the legitimacy of claims to a priori knowledge. The primary point of philosophic contrast here, i.e., the positions taken to be paradigmatically non-naturalist, are the avowedly antipsychologist view of analysis championed by Gottlob Frege and the notably apsychologistic views of the early Wittgenstein (Kitcher 1992, 59).

While Maffie provides a careful schema that sorts and categorizes the wide variation of positions labeled as forms of naturalism, Kitcher sweepingly surveys the rise of a contemporary (post-Quinean, post-Kuhnian) variant. Like Maffie, Kitcher's positive characterization of naturalism emphasizes a methodological continuity between epistemology and natural science. But for Kitcher, what separates "conservative" or "traditional" naturalists from the more "radical" sort is that the former, but not the latter, believe that there are some perduring goals and strategies of scientific inquiry, however corrigible particular formulations turn out to be. Naturalism, conservatively construed, attempts "to fulfill traditional normative functions," while radical naturalism sees "in the collapse of apriorism the demise of any possibility for normative appraisals (or, at least, the need for relativizing any such appraisals to specific, local context)" (Kitcher 1992, 58).[12] For Kitcher, what separates traditional from radical naturalism is not just, as for Maffie, an issue regarding the continuity of methods, but also a concern regarding the nonrelativized character of the principles that naturalized inquiry seeks to uncovers.[13]

With regard to the traditional normative project of epistemology, Kitcher maintains that naturalists are primarily bent on improving epistemic performance. He terms this the "meliorative project." Its primary purpose "is to identify processes that are externally ideal" (Kitcher 1992, 66). Here, Kitcher's exemplar of a meliorative project within the bounds of naturalism is (again) Goldman's reliabilism (See Kitcher 1992, esp. sections 2 and 3). The twist here is that while Kitcher considers reliabilism "appropriate for the context of methodological improvement," he suggests that it is less clear how it applies to some other traditional normative concerns, e.g., "the context of epistemic appraisal" (Kitcher 1992, 68).

The relevance of reliabilism under Kitcher's interpretation to the meliorative project is straightforward. Stable standards allow for clear-cut advice on how to enhance epistemic performance. Appraisal, however, is more psychologically complex and multifaceted. The distinction reflects the differences between getting the right answer and getting the right answer for only the right reasons. Whether one has come to an answer in the "right" way is an issue, Kitcher suggests, best left to psychologists. Epistemologists can profitably separate themselves from such questions. As Kitcher notes, "the philosophical dichotomies rational/irrational and justified/unjustified may stand in need of replacement rather than analysis. . . . [D]ebate about whether the failure to undergo the epistemically optimal process is excusable or not can profitably be sidestepped in favor of a psychologically richer explanation of what occurred" (68). A consequence is that a naturalistic version of the meliorative project might fail to yield an analysis of, for example, justification.

Kitcher, then, while praising Goldman for promoting the epistemological relevance of psychology and other sciences, also notes two persistent anti-naturalist facets to Goldman's thought—an adherence to methodological dualism (natural science versus conceptual analysis) and a related concern with accomplishing more than just the meliorative project. Like Maffie, Kitcher questions Goldman's reluctance to fully naturalize his approach (69 n. 46).

However, it is unclear from Kitcher's remarks whether or not he opposes methodological dualism in any form. For some passages suggest that Kitcher himself conceives of philosophy as offering legitimate, nonnaturalist methods. "Traditional naturalists ought to concede that there is a legitimate activity of using the arsenal of philosophical techniques (appealing to formal logic or probability theory, say) to articulate ideas about knowledge. The development of an account of epistemic value might well draw on such resources" (78). Unlike Goldman, however, Kitcher acknowledges that we stipulate what our epistemic values are. Thus, the dualism at issue in Kitcher's case concerns issues such as whether to count logic as a science, or as something else. The question here is what Kitcher, *qua* traditional naturalist, wants to say about how we come to knowledge outside the scope of science or, alternatively, how we define 'science.'

Another important problem with Kitcher's proposed way of differentiating radical and traditional naturalism concerns his attributing the meliorative project to the latter and denying it to the former. Kitcher, as do the others, views Quinean naturalization as effectively abandoning epistemology. "Radical naturalism thus abandons the meliorative venture . . . , letting epistemology fall into place as chapters of psychology, sociology, history of science" (96). Yet the "proof" that a view or method is actually reliable (and so genuinely meliorative) depends, in large part, on its historical record. Traditional naturalists, Kitcher believes, can counter the threat of "radically" relativizing epistemic standards by appealing to an emergent consensus in historically difficult cases (97–98). In other words, if a naturalist wants an epistemology that does more than relativize epistemic norms to received science, the historical record must be read so as to show that consensus emerges in the sciences due to certain stable principles guiding scientific investigation.

According to this account, radical naturalists are pessimists regarding stability; defenders of an optimistic version of traditional naturalism such as Goldman, Laudan, or Kitcher expect the discovery of stable principles that will improve epistemic performance. As Kitcher remarks, "[n]aturalism offers the optimistic picture of a particular type of organism, beginning with rudimentary representations of nature . . . and gradually replacing these with cognitively superior representations and strategies" (90).

This optimism is important, for it appears to be all that separates at any given time a Kitcher-type epistemological naturalism from the more radical varieties. Radical naturalism imagines that the best we can ever do is relativize epistemic norms to received science. However, Kitcher maintains that what would make naturalism genuinely meliorative (and so not radical) is if an account of science can be provided that shows that science allows us to possess "unambiguous possibilities of continual correction." These "unambiguous possibilities" would make the history of science something more than "a random walk" across time (93; see also 100). Indeed, only this optimism links naturalism to traditional epistemology: "whether naturalism allows any way to save the traditional meliorative project of epistemology" requires "the possibility of our sustaining the reliability of the historical process through which knowledge has emerged, given a naturalistic perspective" (113).

But is the difference between "radical" and "traditional" naturalism, as Kitcher labors to draw it, a difference that makes a difference? For what would distinguish the counsel given by a traditional as opposed to a radical naturalist? As Kitcher concedes, "[w]e hope, but cannot demonstrate, that the system of predicates we actually use will lead to success in the actual world" (88). The traditionalist possesses no epistemic wisdom that the radical lacks. The distinguishing feature would be the hope the traditionalist has that history will forever sanction the advice, for only that sanction makes it genuinely meliorative.

What traditional as opposed to radical naturalism achieves is not found anywhere in the present, but only in the future. "The ultimate goal of (traditional naturalistic) epistemology is to present a compendium of cognitively optimal processes for all those contexts in which human subjects find themselves" (76).[14] That is, in this view of what the meliorative project comes to, it remains incomplete until such time that we know that the processes in hand are optimal for all possible experiences of which we are capable.

So-called naturalist positions such as Kitcher's that promise more by way of normative edification than does Quine invariably turn out to fail to justify such normative claims naturalistically (or at all). As Miriam Solomon quite properly notes with regard to Kitcher's pseudo-naturalism—a position she dubs "Legend Naturalism"—his "naturalism does no work—no data or theories from psychology or sociology shape the epistemic account—the naturalism is just window-dressing for a previously and independently developed account of scientific rationality" (Solomon 1995, 207).[15]

A further problem is that what Kitcher advocates is, for all intents and purposes, a type of Kantianism naturalized. He hopes that the historical long run reveals what transcendental philosophy did not, viz., the perduring structure and operation of human cognitive capacities and the limits of inferences to be drawn about the world from experience. "The goal of pure inquiry is to produce a structured account of nature insofar as that is possible for limited beings like ourselves" (Kitcher 1992, 107).[16] Thus, Kitcher transforms traditional naturalism into a surrogate for the very type of a priori project the rejection of which he invoked when initially defining 'naturalism.'[17] Kitcher's nod toward taking into account social factors, for example, obscures the fact that all that separates "radical" from "traditional" naturalists is the latter's belief that it is our cognitive manifest destiny to get things right. But why naturalism, in order to be meliorative, must insist that history provides what transcendental philosophy did not, Kitcher nowhere explains.[18]

All three of the overviews just considered distinguish, in any case, radical naturalists such as Quine from moderates such as Goldman by the contention that a Quinean "radical naturalism" is somehow incompatible with normative concerns. In order to provide normative judgments, all agree that epistemology must at least have the resources to evaluate "the fitness of cognitive behavior" (Maffie 1990, 286) relative to truth. Maffie articulates a now common view when he maintains that Quine cannot accommodate normativity because by "integrating epistemology into science" a Quinean view "seems to leave us with no resources for making cognitively significant normative judgments" (285). Psychology provides only a list of processes we do, in fact, possess. But no description of processes or other natural properties tell us what norms to value. The received wisdom, in short, has Quine advocating the supersession of epistemology by a descriptive subject, psychology. There no longer is normative epistemology as we once thought we knew it.[19]

In sum, then, Quinean naturalism is not a philosophical theory in the previously specified pejorative sense of that term. It is empirical through and through, from its conception of logic to its conception of methods to what even to count as science. Quinean naturalism is untainted by prior philosophical commitments to reduction or to a hierarchy of sciences. No area of belief stands aloof from alteration or emendation in light of experience. Even the preference for naturalism itself is evidence-driven. Should some approaches other than those the sciences offer prove more efficacious in furthering our goals, the commitment to naturalism itself would then be jettisoned. There is no more vagueness to the notion of what naturalism is than there is to what the methods of the sciences themselves are. There is no more an obstacle to examining, emending, or excluding norms within a naturalistic approach than there is in any self-critical scientific approach. Which is to say, there is none at all.[20]

II

The preceding suggests that disputes about naturalized epistemology focus less on what it is for an epistemology to be naturalized than on what qualifies a naturalistic approach as epistemology. The substance of Quine's naturalism derives from his account of the "methods of science." Understood in this way, any case for Quine's naturalism must answer three critical challenges. It must first identify what science is and its methods are, and second provide good reasons for preferring these to other modes of inquiry. Finally, the argument must establish that these methods actually suffice for the prescriptive purposes of epistemology.

Criticism to date of Quine's naturalism focuses primarily on the last mentioned issue, with occasional allusion to the second. This fosters the illusion that the details of how Quine conceives of science are unproblematic or consistent with the answers imputed to Quine regarding the latter two challenges. In addition, a pervasive misreading of Quine's response to the second challenge results in the charge that Quine rejects the "doxastic assumption," i.e., that he wrongly tries to replace epistemology's normative/evaluative concerns with a causal/genetic enterprise. I propose a way to construe his responses to all three challenges so as to provide a cogent and coherent account of the hows and whys of naturalizing epistemology.

In my reading of "Epistemology Naturalized," that essay offers neither an argument for the naturalization of epistemology based upon some particular argument against foundationalism nor does it plump for some particular conception of science. Rather, Quine takes for granted that the history of post-Humean empiricism leads to holism. Consequently, questions about the justification of claims to *empirical* knowledge must be adjudicated intra-theoretically. *In other words, "Epistemology Naturalized" explicates the consequences of naturalizing epistemology for those prepared, for Quine's reasons or others, to take Quine's "holistic turn."*

The assumed question to which "Epistemology Naturalized" is to be

the answer is: What becomes of empiricist epistemology if it is assumed that, working from within our current system of beliefs, we take 'science' as our best source of justification for beliefs about the world? What becomes of epistemology within those constraining assumptions? Quine's answer, I contend, is that the fate of empirical knowledge is no worse off in this (epistemological) regard than that of mathematical knowledge given the incompleteness theorems. The primary argument of the essay, in my reading, is to establish and defend this parallel. The consequence is the same for each as well: the epistemological limitations influence which problems to pursue and how best to pursue them.

Quine explicitly parallels foundational questions in epistemology with foundational studies in mathematics. "But still the success achieved in the foundations of mathematics remains exemplary by comparative standards, and we can illuminate the rest of epistemology somewhat by drawing parallels to this department" (Quine 1969, 69). The parallels, then, between the two forms of inquiry are to "illuminate" what Quine wants to say about epistemology. Yet it is precisely the nature of these parallels, and the critical role it plays in the argument given in "Epistemology Naturalized," that has been wholly ignored in efforts to ascertain and evaluate Quine's position. This distorts the focus of his argument in general, and of his position regarding naturalism in particular.[21]

Both foundational projects have two aspects: the conceptual and the doctrinal. Conceptual matters are semantic, concerning definition or explication (69). Doctrinal issues concern issues of justification and formal priority (69–70). Ideally, the definitions would generate all the concepts from clear and distinct ideas, and the proofs would generate all the theorems from these self-evident truths (70).

This suggests that the intended parallel to the foundational project in science is the logicist program for having a consistent, fully axiomatized, and complete set of rules adequate to all of mathematics. The parallel implies a concern with developing a consistent and complete system for evaluating all empirical knowledge claims (71).[22] Take sense impressions, and then either explicate or derive all statements about the external world. This formula, if successful, would have provided an analysis, in the best understood sense of the term, of the entire range of truths about the world.

Notice that rational reconstruction is just a method. *Qua* method, rational reconstruction has two aspects: one procedural, one normative. Procedurally, the question is what methods and evidence suffice to reconstruct all that the sciences teach us is true. Normatively, the question is what makes any such reconstruction rational, in a philosophically relevant sense. Philosophical foundationalists look for answers to both questions that hold the sciences to stricter standards than the sciences as we now find them hold themselves.

Yet the project of providing foundations for science discovers that it too is subject to forms of incompleteness, paralleling the fate of the logicist project in mathematics. Indeed, distinctive forms of incompleteness attend the doctrinal and the conceptual aspects of this model as an epistemological program in the foundations of science. On the doctrinal side, the project stumbles on Hume's problem—the simplest generalizations from experience outrun our evidence for them. Hence derivation of laws of science proves impossible (74).

The problem on the conceptual side is not quite as neat or accepted as that on the doctrinal. The principle difficulty on the conceptual side—the incompleteness of any explicatory project—turns on the fact that the relation of theoretical sentences and supporting evidence is never as it needs to be in order to make the desired translation possible (78–79). In short, holistic considerations regarding the language-evidence relation foreclose the possibility of an explication of the term-by-term sort that the foundational project requires.[23] Thus, there are two irremediable forms of incompleteness. Neither laws nor concepts can be justified as hoped. This forecloses the possibility of providing within empiricism a philosophical foundation for science.

But why is this a reason to prefer the methods of science? Quine's response is to suggest that the next best set of methodological norms are those that science itself offers. "To relax the demand for definition, and settle for a kind of reduction that does not eliminate, is to renounce the last remaining advantage that we supposed rational reconstruction to have over straight psychology; namely the advantage of translational reduction" (70). Yet Quine's response may appear both to assert dogmatically a preference for science and to shift illicitly the nature of the epistemological project to causal/genetic concerns.

The "suppressed" premise in the argument from the failure of foundationalism to the conclusion that we can do no better by way of justification than science self-applied is what I shall refer to as Quine's epistemic "scalar hypothesis." The "scale" here is one of degrees of strength of justification, with formal derivation constituting the strongest end of the scale. The hypothesis is that no standards lie between formal deduction and the more assorted methods of various sciences, i.e., that we can do no better than scientists do with regard to validating our beliefs.[24] Although a hypothesis about norms, it itself is based on observation and open to refutation by new facts, and so naturalistic.

Quine starts, in other words, with what are generally taken to be instances of knowledge—mathematics and natural science. He then asks, following the logicist lead, for a consistent and complete system for explicating and deriving these putative truths. Incompleteness does not motivate Quine to abandon the view that these are "best case" instances of what to take as knowledge. Rather, incompleteness forces Quine to settle for the next step down on the epistemic scale of justification.[25] This shift signals a lack of extrascientific means both for the reconstruction of scientific truths and for certifying what reconstructions count as rational.

Emphasis on the parallel between foundational studies in mathematics and those in science makes explicit how incompleteness in both cases shifts the norms for justification to the next best set of practices available. In logic this involves, for example, assessing the properties of different possible axiomatizations. In the case of empirical knowledge, the norms shift from those represented by explication and derivation to those embedded in the practice of science. *Foregoing foundationalism changes the understanding of the "best available epistemological standard."*

Making explicit the role of the "scalar hypothesis" in the argument for naturalizing epistemology permits identification of the source of epistemic norms, of the reason for their adoption, and of the conditions under which they might be subject to change. For insofar as the notion of what comprises science is not static but dynamic, so too will be the standards deemed appropriate or scientific. Quine is, as Kitcher charged, a "radical" naturalist. The third challenge too is met since, *ex hypothesi*, the methods of science circumscribe the bounds of knowledge.

But the criticism can be made that to claim that epistemology is concerned with the foundations of science appears to arbitrarily circumscribe the concerns of epistemology. Why just the foundations of science as opposed to, say, the bases for all human beliefs? What licenses Quine's assertion that science encompasses what we can be properly said to know?[26] Yet these criticisms presuppose that whatever "science" is for Quine would preclude the study of some area or other of human belief. But Quine does not start with some prior conception of what science is, and then insist that all legitimate empirical inquiry fit this procrustean bed. Rather, Quine's conception of science is quite liberal.

The opening sentence of "Epistemology Naturalized" is: "Epistemology is concerned with the foundations of science" (Quine 1969, 69). Reading this sentence as *narrowing* the scope of epistemology misreads it. Quine countenances as a science any form of inquiry that respects the experimental method. He does not mandate invidious distinctions between types of sciences—"hard" or "soft," natural or social.[27]

The charge that Quine illicitly infers epistemic virtues from the pragmatic and instrumental ones science offers simply misses the implications of conjoining the failure of foundationalism with the consequences of holism. For a holist *cum* naturalist, there is no alternative to beginning with all methods of empirical inquiry—from physics to history—as we find them. "Unlike Descartes, we own and use our beliefs of the moment, even in the midst of philosophizing, until by what is called scientific method we change them here and there for the better" (Quine 1960, 24–25).[28] "Best" and "better" are clearly provisional characterizations for Quine; there is no point of cosmic exile. Epistemology starts with what we now have, and goes from there. Quine does not need to defend adverting to scientific standards in this case. Rather, for this time and culture, the burden of proof is on those who would claim extra-scientific bases for knowledge.

Quine's version of naturalized epistemology naturalizes epistemology in two related but distinct ways. One level is explicitly normative, in which the naturalized epistemologist uses and refines whatever the standards of proofs, techniques of inquiry, or other methods are among those that are already in use in the sciences. This follows directly from endorsing these standards as the ones best available to us for justifying our beliefs about the world. The second level is explanatory, and involves the construction of scientific—causal—explanations.

In a completed scientific world picture, the first (normative) level would be the proverbial ladder that is kicked away after it is ascended. Short of that point, however, naturalized epistemologists proceed in both of the aforementioned ways that scientists do. The naturalistic/pragmatic turn embraces not only the descriptive results of scientific inquiry, but also the prescribed practices for conducting such inquiry. For doing science involves both.[29] In this regard, Quine's writings advocate a paradigm shift in epistemology—a change in the methods, problems, and standards previously invoked in the subject.[30]

Thus, when Jaegwon Kim complains that "Quine is asking us to set aside what is 'rational' in rational reconstruction," he misses the point (Kim 1988, 389). For what Quine has understood is the need to alter

what counts as an epistemologically satisfactory *reconstruction* of science (Quine 1969, 76). For a foundationalist, a rational *reconstruction* was to reconstruct science by derivation from sensory evidence. Such reconstruction is now understood to be impossible due to both forms of "epistemological incompleteness." This shifts not only what counts as a possible reconstruction from a derivation or explication to a causal analysis, but also changes what makes the reconstruction *rational*. It is rational if done in a scientific way.

Kim reacts incredulously to the suggestion that any causal account could be of epistemological interest. He asks "in what sense is the study of causal relationships between physical stimulation of sensory receptors and the resulting cognitive output a way of 'seeing how evidence relates to theory' in an epistemologically relevant sense?" (Kim 1988, 390). But a question of how evidence relates to theory is either a question of what *methods* a science employs, or it is a request for a causal story, to the extent there is one. In either case, the answer must employ those inferential procedures belonging to the repertoire of methods in the sciences.

On the one hand, a question about scientific justification might be taken as a request for a scrutiny of the type of confirmation theory and other inferential practices employed by some portion of the scientific community. Are their conclusions licensed by acceptable inferential procedures? The question here is normative, i.e., whether or not they are acting in accord with the best recognized standards given what else is known.

On the other hand, one might interpret the question causally, as one of how we human beings might ever have come up with scientific theories such as those we possess, given our information about the world—our stimulus input. This way of interpreting the question accords with the second level previously noted, i.e., a demand for a causal/developmental story of belief acquisition. Each of these two readings raises questions about the theory-evidence relation. Epistemologists might conceivably take an interest in either question. But one asks what makes such reconstructions rational, and the other asks by what methods we reconstruct truths. Each is a question asked about science from within science.

Here, then, is the full philosophical import of Quine's initial parallel between the foundational aspirations in mathematics and those in epistemology. The causal story is what rational reconstruction becomes once demands for stricter reconstructions are seen as futile. Any other interpretation either confuses both the first question and the second, or is a demand, *per impossible*, for a better reconstruction than science itself provides.

This confusion of justificatory practices and rational reconstruction is also manifest in the charge that Quine violates the "doxastic assumption," i.e., the view that only beliefs can justify other beliefs (See especially Koppelberg 1990; also Gibson 1995). On the one hand, philosophers understand justification in terms of inferential relations. But, on the other hand, scientists characteristically explore causal relations. Naturalism, once again, appears to confusedly substitute a causal story for a rational/inferential one.

Koppelberg notes the resistance to accepting this switch and gives voice as well to his own suspicions, however inchoate his reasons, of this resistance (Koppelberg 208–9).[31] But, we can now say, what Quine provides is precisely this: a *theory* that tells us what justification consists in. The theory unfolds by appreciating the paralleling of mathematics and science as paradigm cases of knowledge, by identifying the foundational projects associated with each, and by apprehending what the failure of each such project implies, particularly with regard to the changes effected on the notion of rational reconstruction. In Quine's naturalism, the causal and the normative are just opposite sides of the same scientific coin.[32]

Richard Foley rightly rejects criticisms such as Kim's that impute to Quine's procedure an abandonment of a concern with the normative project of epistemology (Foley 1994, 246–48). Foley has a different question: just how does Quine's approach differ from the non-naturalist epistemological tradition? (245). He suspects that the differences are more illusory than real (256).

He discerns two possibilities for distinguishing Quine's way of doing epistemology from that of, for example, Descartes or Roderick Chisholm. The first, albeit "uninteresting," differentiating factor is that "the canons of rational belief just are the canons of science, broadly conceived" (258). This move is uninteresting, Foley suggests, inasmuch as it simply marks out the substance of Quine's epistemic advice; by itself, this does not differentiate Quine's way of determining what advice to give from that of his non-naturalistic predecessors (255).

Foley's second way of distinguishing Quine from non-naturalists emphasizes Quine's rejection of the analytic/synthetic distinction, a move that leads to the conclusion that "the fundamental epistemic norms in his system cannot be known a priori, nor are they necessary. Rather, they are continuous with science" (258). If these norms were themselves shown to be *part* of science, Foley acknowledges, then a genuinely important and interesting difference between Quine and the tradition would have been identified. For then the normative issues become one with other scientific questions.[33]

The problem here, Foley maintains, is that Quine's commitment to the revisability of norms is hardly more than a *façon de parler*, inasmuch as Foley believes that Quine's commitment to empiricism is such as to make it effectively unrevisable. Foley's evidence that Quine is a closet traditionalist is that Quine offers no scientific defense of the very norm—empiricism—that Foley takes to be central to Quine's conception of the scientific project.[34] "The specific norms Quine favors are ones that he recommends from his philosophical armchair, with little or no concern for an empirical defense of them" (258–59).[35] Quine's procedures are, Foley concludes, only "tub-thumping" (as Kitcher puts it) for his favorite epistemic values.

Yet Foley concedes Quine's philosophical point, viz., that it is our most successful scientific practices that delimit the acceptable epistemic norms for a naturalized epistemologist.[36] As Foley acknowledges, there exist tests for ascertaining the correctness of the tenets Quine favors. More generally, as Foley notes, the "interesting conception [of Quine's] epistemology is one that makes his epistemology part of science, but this requires that even his most fundamental norms . . . be products of science" (255). Can this be done in a manner consistent with the approach Quine advocates? Foley allows that it can.[37] In the end, then, Foley acknowledges that Quine can be read as "doing epistemology" in an interestingly and importantly different fashion, one in which the norms of science are taken as findings of science. *But all that one needs to defend the consistency and normativity of naturalized epistemology is this sense*. For Quine, axiological claims about ends are to be made a posteriori through experimental practice, rather than a priori through conceptual analysis or appeal to intuition, and this is what distinguishes Quine from traditional epistemology.[38] His is the practice of epistemology *within* and *as* science.

Peter Hylton argues for an interpretation related to the one I offer above (Hylton 1994), but draws some different conclusions from those I defend. In line with the view developed here, Hylton maintains that for Quine, "[n]aturalism can be equated with the failure of foundationalism" (Hylton 1994, 268). As a consequence, there is for a Quinean naturalist no distinction to be drawn between philosophy and science. "What is crucial to Quine's naturalism is the negative point, that there is no theoretical perspective other than the general perspective of natural science—and, in particular, no distinctively philosophical perspective" (267). Hylton emphasizes, rightly in my view, that Quine's conception of science is primarily methodological (278).[39]

Where I take exception to Hylton's otherwise thoughtful and penetrating analysis is the account he offers of what follows from the failure of the foundational program in epistemology. Hylton finds in "Epistemology Naturalized" no reason to accept science self-applied as the obvious surrogate project with which to replace foundationalism (269). Hylton's complaint underscores the significance of the epistemic scalar hypothesis for Quine's argument, as well as the fact that it is commonly overlooked. For this shifts the burden of proof to those who would resist or deny the naturalistic turn.

Against the shared concerns of both Hylton and Foley that any attempt to justify basic norms must be, to a greater or lesser extent, circular (Foley 1994, 256; Hylton 1994, 269–70), my reconstrual eliminates the need to simply assume the truth of naturalism. In my account, naturalism follows from two assumptions: first, the fact that there are apparently sound arguments to the conclusion that foundationalism is impossible, and second, the normative scalar hypothesis, i.e., the claim that we possess at present no better standards of validation for empirical knowledge claims lying between what foundationalism promised and what science itself offers.[40] Given the normative scalar hypothesis, Quine's naturalism subsumes the is/ought gap.

Quine starts, not with appeal to any a priori truths or incorrigible beliefs, but with just the best explored systems of inference and evidence he has available. The focal point, the crux of the whole matter, is *not* a contrast between the normative and the descriptive. The epistemologically significant contrast with which Quine is working, rather, is *extrascientific* versus *intrascientific*. This, of course, is the point of his inveighing against "first philosophy"—the presumption that we have access to methods and evidence better than those that science itself underwrites. The issue throughout "Epistemology Naturalized" is not whether to be normative, but how.

Quine views science as self-correcting, and so as incorporating a concern for norms within its ongoing practice. Even the bias in favor of

empiricism as a theory of evidence is one that Quine believes science to underwrite.[41] Can a naturalized epistemology be normative? Quine, for one, would not expect it to be otherwise. But this means only that the rules for engineering the success of science are not themselves discovered by some special nonscientific form of inquiry. *Contra* Kitcher, "radical naturalism" does engage in a meliorative project.

The issue for Quine with regard to naturalizing epistemology is which set of norms to settle for. Having tried to improve or clarify the intuitively most plausible set of truths and practices by a standard stricter than current science supplies, Quine concludes that there are no normative precepts available superior to those that scientists employ. The description of what it is to do science includes, *inter alia*, the norms relevant to that practice.[42] *The justification of this set of norms, in turn, is given by the success of the practice (relative to other options) in attaining a desired end.*[43]

Following science, much may be let go, for example, belief in physical bodies as basic.[44] Quine suggests that even if, for some reason or other, we gave up empiricism as our theory of what counts as scientifically acceptable evidence (and so granted legitimacy to clairvoyance, for example), the test of a science of clairvoyance would still be successful prediction. Could even this benchmark of science be altered? "In that extremity [of countenancing clairvoyance] it might indeed be well to modify the game itself, and take on as further checkpoints the predicting of telepathic and divine input as well as of sensory input. It is idle to bulwark definitions against implausible contingencies" (Quine 1990, 20). The parameters of the scientific language game, defeasible though they may be, are for Quine animated by purposes tied to technology and understanding, and defined by prediction. If inquiry yields no predictive test, then it is not a Quinean science.

But, now, if worthwhile purposes may be so diverse as to include interest in understanding, then what counts as a scientific explanation, and so a scientific method, is any inquiry and practice that satisfies understanding and has empirical checks. For naturalism is *not* a theory of how to decide among competing or incompatible accounts of science. An irony here is that the "naturalists return" is coincident with the departure of faith that the term 'science' marks out fields of inquiry by methodological kind (See Roth 1996b). The ghost of the demarcation problem haunts naturalized epistemology insofar as opting for a naturalized epistemology does not settle which type naturalistic theory to prefer.

III

Feminism as I see it is concerned with the oppression of women. Feminist philosophers of science examine both how and why the sciences have been utilized, wittingly or not, as institutions and instruments of this oppression. But the theoretical alternatives for legitimating a feminist challenge to scientific practice offers them, or so it might appear, only a Hobson's choice. On the one hand, the sciences have no lack of defenders even among feminists regarding the objectivity, rationality, and truth production of their theories and methods. Defenders of the scientific faith account for any aberrant instances—be they Nazi medicine or primatology research—as just cases of bad science. But then they are stuck with the old vocabulary of legitimation, one that has not served them at all well. Thus, tradition "weighs like a nightmare on the brain of the living."

On the other hand, as has been by now compellingly documented (and not just in the case of women), scientific theories have been used to legitimate various oppressive social policies, to embody prior commitments to social values, and to influence in their most basic formulations by prejudicial forms of thought at play in the culture. So some critics, both feminists and others, respond to claims of scientific objectivity with "*Écrasez l'infâme*." But this appears to be a case of tossing out both the baby and the bath water.

Charting a *via media* between these unattractive alternatives motivates a melding of feminism and naturalism into a social empiricism. Such views are naturalistic and empirical by virtue of their conception of what the possible evidence is and what is the source of methods for assessing evidence. They are social insofar as they maintain, paraphrasing Quine, that science is a social art and that in acquiring it, one depends entirely on intersubjectively available cues as to what to say and when. Terming science a "social art" implies, *inter alia*, that scientific inquiry and scientific communities do not operate in splendid (epistemological) isolation from the world around them. Finally, a specifically *feminist* social empiricism concerns how gender-nuanced patterns of reasoning and theorizing result in scientific inquiry abetting forms of oppression.

But even here anxiety about the legitimacy of the naturalist project is evident. For feminists subvert their achievement—the unmasking of science so as to expose social agendas at work beneath the veneer of dispassionate inquiry—by propounding philosophical theories that only

function to help perpetuate old illusions. Indeed, they do this using philosophies of a thoroughly discredited sort—a "conjuring up of the dead" to the new scene in "time honored disguise and . . . borrowed language."

Recall the hallmark of logical empiricism, viz., its claim to offer a reconstruction of knowledge claims in empiricists terms. Feminist critics then emphasized how this specific pretension about the relation of evidence to theory only served to hide a social agenda behind the mask of methodology. But with the Kuhnian turn, the notion of social construction replaces that of rational reconstruction. Encouraging this replacement was pioneering work in the sociology of science by Barry Barnes and David Bloor—the self-named Strong Programme in the sociology of scientific knowledge. I think their work and what follows can stay, but it needs a turn to Quinean themes afterwards.

Barnes staked out early on a conception of what he termed "natural rationality" as central to the underlying intellectual presuppositions of this sociological approach. As he conceived it, a study of "natural rationality" examines "how people actually reason rather than how ideally they should reason" (Barnes 1976, 115), a deliberate contrast to idealized accounts of reasoning he finds in the philosophy of science.

Two features of natural rationality are particularly salient for subsequent critiques of conventional philosophy of science. First, Barnes maintains, for "inductive inference to be possible, observations have to be taken as instances of something or other. . . . Hence a *fundamental* distinction between 'theoretical' and 'observational' discourse cannot be sustained in any culture" (122). Second, "[n]atural rationality operates in the context of a pre-existing set of concepts and beliefs. . . . No unique set of beliefs is thrown up spontaneously by the interaction of an unsocialized naturally rational actor with the world" (122).[45] Barnes' insight is that "no distinctiveness can be attributed to our existing natural science. All institutionalized systems of belief and action appear to embody natural rationality alike—science no more or less than any other institution" (122). He derides, in this context, "virgin birth" views of scientific rationality.

But their well-motivated studies of natural rationality at work in laboratory settings notwithstanding, the claim by social scientists to have privileged access to the factors explanatory of what natural scientists do is ironic. For the social sciences are where positivism had its strongest and most enduring impact on disciplinary practices and self-image. Texts in these areas still serve up potted versions of Carl Hempel's *Philosophy of*

Natural Science as received philosophical truth. Moreover, hand-wringing about their status as "real sciences" remains endemic in the social sciences. A positivist conception of science, in turn, is still taken as their legitimation. Follow the methodological recipe, and "real" science results.

Indeed, a basically positivist view of what it takes to be a science is as much a part of the Strong Programme as it is of other social sciences. The red flag is their claim to provide causal explanations of the cases they study, from turn-of-the-century phrenologists to Weimar physicists, and from contemporary high-energy physicists to cancer researchers. Claims for the causal adequacy of their explanations also signal where these sociologists go off the naturalistic track. But it is sheer pretense on the part of these sociologists to maintain that any of their case studies yield causal explanations. There are no causal explanations, because they can cite neither laws nor regularities; there are not even passable facsimiles of ones. Nor do the sociologists have some special account of "causal explanation" to which to appeal. They write as if they could provide *Der Logische Aufbau der Welt* done up in sociological dress. The sociological claim to have uncovered the secret engine of scientific cognition reveals only that social constructionists are themselves true believers in scientific rationality. But there are no such reconstructions. An inability to detail how the social is in fact "constructed" comes as no philosophical surprise.[46]

My endorsement of Barnes' notion of natural rationality still stands. The pervasive presence and influence of social factors on what is termed 'scientific reasoning' goes without saying. But if the question is one of determining the relation of values to practices, taking sociological construction as the replacement program for rational reconstruction is simply a nonstarter. If experience teaches anything, it is the virtual impenetrability of whatever processes there are that lead people to associate beliefs in the way they do. There is no saying exactly how, given knowledge of someone's interests, education, gender, and so on, these factors then determine what other sets of beliefs, especially scientific beliefs, a person is going to hold.

A recent essay by political scientist Joyce Mushaben, "Collective Memory Divided and Reunited," provides an illustration of how varied is the way that individuals understand (Mushaben 1999). Mushaben, an expert in post–World-War-II German politics, focuses on the impact that reunification has had on the self-understanding of German feminists as they examine women's roles in Germany's troubled history since the 1930s.

The particular problem that Mushaben ponders involves the fact that reunification brought together two groups of feminists—East Germans and West Germans—each raised in a different understanding of both Germany's and women's responsibility for German actions during World War II. Unification accelerated the challenges of later feminists to the avoidance strategies of their sisters-in-arms. For when the Wall came down, those living east of the old divide found themselves faced with what appeared to be unmodified and unrepentant German nationalism, while those in West Germany could see only vestiges of a militaristic and authoritarian state that they had repudiated. In broad strokes, what Mushaben examines is how feminists in both the GDR and the FRG early on took refuge in the view of women as among the victims and the oppressed during the Nazi regime. Yet, she notes, the feminist slogan "the personal is the political" worked against this view as an ultimate refuge from participation in and responsibility for the events of that era. Feminist scholars and writers from both East and West Germany increasingly produced works challenging the sanguine assumptions of the "women as victims" view.

Mushaben highlights a controversy that has developed over the appropriation of feminist doctrines by women committed to Germany's New Right, i.e., neo-Nazis and ultra-nationalists. She observes that "many women of the New Right see their own behavior as the logical extension of feminism's self-empowering legacy" (Mushaben 1999, 31). This puts contemporary feminists in a special "double bind": first, in deepening their already problematic and largely unexplored relationship to their mothers and the behavior of German women of that generation, and second, in assessing their own doctrines and how they are open to neo-Nazi appropriation (31–32).

Once again, the historiographical moral cautions against an assumption of "obvious" connections within the web of beliefs. Mushaben nicely summarizes the difficult lesson her study suggests: "This treatment of mothers, daughters and the fascist experience confirms the existence of 'competing generations of collective memories'—fractured further still by the prism of gender—'which are not linear and progressive but circular, repetitious and profoundly unpredictable'" (33). Her analysis nicely illustrates ways in which feminist doctrine underdetermines beliefs about history, agency, and appropriate forms of government.

Theorizing as if social construction could do the job that rational reconstruction could gets us nowhere, I am suggesting, because there is no discernable structure at work. Only adherence to some prior philosophical theory could foster the belief that a structure must be present. Such prior philosophical commitments frustrate the efforts of some feminist and naturalist philosophers of science to pursue a naturalized and socialized study of scientific reasoning.[47]

Helen Longino's effort to sketch a type of social epistemology, I contend, runs aground on the shoals of philosophical theory as well. In Longino's case, she evinces a felt need to steer between the fear of relativism and the charge of what one might call standpoint elitism. "The problem with recognizing the social locatedness . . . of individual epistemic subjects is that it seems to force us into choosing between relativism and demonstrating the epistemic superiority of one among the various social locations. I wish to reject this choice" (Longino, in Schmitt 1994, 139). Her alternative is to conceive of scientific inquiry on the model of a Habermasian idealized speech community (56).

Longino goes so far as to formulate her account in the great Justified True Belief tradition. Her analogue to the justification condition, where S is the member of an epistemic community, W a real-world system, M a model of that system, and C a variable ranging over communities, is as follows (153):[48]

> $S_1 \ldots S_n$'s representing W as M is the result of warranting practices adopted by C in circumstances characterized by
> a. public forums for critical interaction,
> b. uptake of criticism,
> c. public standards, and
> d. equality of intellectual authority among diverse perspectives.

This avoids, Longino maintains, the relativism endorsed by sociologists of science.

In particular, she opposes Karin Knorr-Cetina's sociological version of the epistemological question "How is that which we come to call knowledge constituted and accepted?" (137) and seeks instead "an account of knowledge that incorporates the social dimensions of science into the epistemology of inquiry," i.e., one that demonstrates "that good science does not express the biases decried by feminist critics" (138). Longino opts for a less social epistemology than Knorr-Cetina certainly because she believes than an account of how science works must put a positive philosophical face on that epistemic community's justificatory practices.

Her schematic accounting of warranting procedures, Longino believes, gives us all we can have. At the end of the day, scientists can only argue in terms of practices they know and use. "The point is that there is nothing further—that appeal to standards or methodological norms beyond those ratified by the discursive interactions of an inquiring community is an appeal to transcendent principles that inevitably turn out to be local". Once a community has responded to "free and open" criticism, "then it is not clear that there are any further grounds (beyond simple disagreement) for criticism" (155). Longino's account assumes that ongoing dialogue is the best (because the only) corrective possible for distortions in the process of inquiry.[49]

Longino wants a difference (in warranting practices) that actually differentiates her proposal from Knorr-Cetina's forthright relativism, which settles for "answers invoking the social processes and interactions in the laboratory and between the laboratory and its worldly environment". But has Longino shown, as she intends, "[w]hat distinguishes knowledge from other doxastic states" (137)?

Note first that her model of warranting practices is, at best, a model of normal science, in the straightforward Kuhnian sense. Almost any case of the scientific outsider—from Copernicus to Barbara McClintock—cannot be a knower by her account. This is by definition—by failing to use warranting practices adopted by their community. This is an unavoidable result of any positive explication of scientific knowledge that puts the weight on public, and not impersonal, standards. The other clauses are idle, pending further explication of 'public' and 'equal intellectual authority.'

Individualistic models of scientific reasoning, the very ones that are rejected by Longino and others because they present an inappropriately abstract and abstracted view of how deliberation can or even should proceed, were in fact tailored for "rebels." It is precisely the cases in which outsiders are later "vindicated" that those who oppose social epistemology such as Larry Laudan want to rescue using nonsocial accounts of what good scientific reasoning is. Longino's exclusion as knowers those whose cases motivate, in the first place, a revised account of scientific knowledge suggests that the account has gone terribly wrong.

It goes wrong not because individualism in epistemology needs to be brought back in, but because Longino lacks the courage of her own philosophical convictions. If the best sense to be made of good reasoning is what the community of practitioners, in their collective wisdom, take it to be, then no matter how openly or freely they arrive at that view, there are no transcendent principles, only local ones. But although she says that there are no transcendent principles to which to appeal to bail us out, she just cannot stop herself from still looking. So her account instead simply designates that a particular institutional arrangement instantiates the desired doxastic state. But this is only a relabeling of Knorr-Cetina's relativism, not a proof that she has transcended it.

Naturalism in the Quinean and Kuhnian senses is the realization of what philosophy of science comes to once we acknowledge that there is no special formula, were we but to know it, that would redeem us from our epistemic sins. From this perspective, "feminism" is best taken as marking a particular hermeneutics of suspicion that investigators may bring to bear on scientific practices as they find them. But a "feminist perspective" is not a corrective of distortions in research. There is no methodological template that will do this for us. Rather, the goal must be to find ways to prevent or deflect whatever we term 'science' from becoming complicit in oppression.

Nothing of interest follows, I believe, from calling knowledge 'social.' For it is either tautological—redundantly emphasizing what one means, in part, by knowledge—or unhelpful—serving only to summon the ghosts of dead philosophies. Naturalism, however, adds to knowledge that the sciences provide our best institutional approaches for enhancing the project of epistemology. Taking naturalism seriously requires, however, renouncing the old modes of legitimation and settling for the limits of the new. Doing this leaves us still very much at the beginning of the project of understanding science.

Revolution remains unfinished unless and until we learn to forego the old vocabulary of legitimation and instead become fluent in the new. As Marx observes in concluding the passage I quoted at the outset: "In like manner a beginner who has learned a new language always translates it back into his mother tongue, but he has assimilated the spirit of the new language and can freely express himself in it only when he finds his way in it without recalling the old and forgets his native tongue in the use of the new."

Bertrand Russell observed early in this century that "every advance in knowledge robs philosophy of some problems which formerly it had, and . . . it will follow that a number of problems which had belonged to philosophy will have ceased to belong to philosophy and will belong to science" (Russell 1918, 34). So while some, such as Kim, read Quine's naturalized epistemology as surrendering to the skeptic, I read it as indicating the limits of reason in light of the science of the late twentieth century. We are now in a position to forego intuition-mongering and simply settle for science. The so-called epistemology of empirical knowledge has ceased to be a *philosophical* problem.

Notes

1. See, for example, the following expressions of such epistemological anxiety: Evelyn Fox Keller (in Kourany 1998, 397–98); Elisabeth Lloyd (in Nelson and Nelson 1996, 242); and Helen Longino (in Schmitt 1994, 138).

2. For lamentations regarding the presumed vagueness of naturalism, see Conee (1996) and Plantinga (1996). Bas van Fraassen complains: "To identify what naturalism is . . . I have found nigh-impossible" (van Fraassen 1996, 172). Yet van Fraassen writes throughout his essay about "science," as if he knew exactly what that is. As I argue below, the terms 'naturalism' and 'science' should be seen as equally clear or equally problematic since the latter is central to the definition of the former. See Roth (1996b).

3. "I had realized, on the one hand, the fundamental importance of mathematics for the formation of a system of knowledge and, on the other hand, its purely logical, formal character to which it owes its independence from the contingencies of the real world. These insights formed the basis of my book. . . . This orientation is sometimes called 'logical empiricism' (or 'logical positivism'), in order to indicate the two components" (Carnap 1967, vi).

4. Ibid.

5. See Maffie (1990) and Kitcher (1992). For an especially insightful overview of contemporary variants of philosophical naturalism, see Rosenberg (1996).

6. The "naturalist's dilemma" is just the epistemological analog of Hume's is/ought problem. The charge against naturalism in either case—the ethical or the epistemological—is that one cannot read off what it is best to do from descriptions of what is the case. See Alston (1989) for related discussion, especially sections III and IV.

7. I discovered that Quine uses the term 'methodological monism.' Fot Quine, methodological monism follows from his rejection of the analytic/synthetic distinction and his consequent acceptance of holism. The 'monism' signals that he recognizes no principled distinction in kind (e.g., empirical versus non-empirical; revisable versus non-revisable) among sentences in a language. The monism is methodological inasmuch as the means of evaluating statements is scientific (see Quine 1981, 70–71). Arthur Danto also identifies methodological monism as the defining feature of naturalism. See Danto (1967, 448–50).

Having elsewhere (Roth 1987) argued for "methodological pluralism," does the present account of naturalism represent a change of view on my part? My position is now somewhat more radical. In Roth (1987), the "pluralism" that I defend urges *broadening* the notion of what counts as science. I no longer believe that there is any point to arguing about what is or is not a science (see Roth 1996b). There are only different ways of doing empirical inquiry.

8. Self-described naturalists such as Alvin Goldman attempt to escape such criticisms by construing the analysis of knowledge as tolerating "methodological dualism." Goldman would like to have matters both ways, i.e., to separate himself from those who insist on pursuing a purely a priori analysis of epistemic notions but yet still maintain that there are techniques—specifically philosophical ones—distinct from what we now classify as science. Goldman, *pace* Quine, construes naturalized epistemology as a "liaison" of two distinct forms of inquiry. In this view, philosophy makes

an independent contribution to the analysis of knowledge. I discuss these issues in Roth (1996a) and Roth (1999).

9. Rosenberg agrees in general with this diagnosis, but goes on to suggest, correctly in my view, that Quine is the *bête noire* of other erstwhile naturalists because he (Quine) decouples naturalism from realism, progressivity, and other "philosophical" theses.

10. Maffie argues that limited naturalism preserves a type of fact-value bifurcation, and so "undermines the integrity of naturalism as a comprehensive methodological and epistemological program" (Maffie 1990, 289). For a related discussion of Goldman's work, see Markie (1996).

11. But see Kornblith (1993) for one account of how this might be accomplished.

12. Kitcher adds, in a footnote at this point, the following observation: "The *denial* [my emphasis] of normative appraisal flows from the relativization of such appraisals, if one also accepts the idea that there are always available changes of context that would reverse any piece of normative advice" (Kitcher 1992, 58 n. 16). Quine is later identified as someone holding exactly that radical naturalist position (Kitcher 1992, 69–70).

13. This project is clearly reminiscent of Larry Laudan's views. See, e.g., Laudan (1986) and Laudan (1987). Kitcher here distinguishes himself from Laudan by looking to psychological processes for stable principles, and not just the history of science. Historical stability for Kitcher, I take it, is itself to be explained by pointing to the stability of underlying cognitive structures. Kitcher, unlike Laudan, also believes that social processes can enter into epistemic analysis in positive ways. See discussions by Kitcher (1993), especially chapters 5 and 8.

14. Kitcher (1992, 77 n. 72) makes clear that this is Kitcher's position.

15. Solomon thinks this is the case for Quine as well, and here I disagree.

16. A Kantian reading of Kitcher's view is strongly suggested by the characterization of "minimal realism" and cognitive value that he develops in pages 104–8.

17. For a related criticism, see Solomon (1995), but especially her observation that for Kitcher "naturalism is just window-dressing for a previously and independently developed account of scientific rationality" (Solomon 1995, 207). Nor is Kitcher alone in doing this in the name of naturalism. See Richard Bernstein's complaints regarding McDowell's "naturalized platonism" (with Kantian overtones) (Bernstein 1995).

18. Rosenberg (1996) rightly emphasizes the importance of the "Darwinian paradigm" to both Kitcher's project and other recent species of naturalism. This paradigm fuels the hopes of Kitcher et al. that some traditional philosophical theses, e.g., that science progresses or that realism is correct, can piggy-back on a naturalist project. For a corrective to Philip Kitcher's enthusiasm for the Darwinian paradigm, see Patricia Kitcher (1992), especially chapter 7.

19. I owe the last two sentences to Jim Maffie.

20. Over fifty years ago, Abraham Edel mounted a defense of naturalism in ethics germane to this discussion of naturalism as a source of normative insight for the sciences. He there nicely articulates just why naturalism is reflexive regarding its normative commitments. In the quote that follows, imaginatively replace each use of "ethics" or cognate terms with the appropriate form of the term "science."

> The whole articulation of a morality within a society under given conditions, the problems of change and adjustment within it, require constant valuational activity. We find our commitments as what we are committed to in the specific lines of choice and directions of striving in which we are engaged. Even the major permanent ends we may thus elicit on analysis . . . do not become the objects of isolated independent selection. Their evaluation rests on the whole network of choices and the kind and quality of life to which they commit us.
>
> . . . Mr. Murphy seems to me to pose the question almost as if an ethical theory must somehow equip a hypothetical man who holds no values to choose between conflicting values. If he means to eliminate all reference to an existent value-pattern of the self as already settling the moral problem, then he poses an impossible task. The question "What values should I choose if I had no values?" is meaningful only if it asks what other who had values would recommend for a person in my position. All justification is in a matrix of existent values. Scientific method is applicable to values in so far as it provides a way of identifying one's existent values, testing them, and refining or revising them in choice. (Edel, "Is Naturalism Arbitrary?" *Journal of Philosophy* 43: 146–47)

We have ends important to us, and we have systems that, we hope, will abet us in achieving those ends. If the ends seem to require rules we find overly restrictive, we can alter or drop the goal; if a rule does not function well relative to the end in view, we can change the rule. This is as true for science as for ethics. Nelson Goodman offers a similarly naturalistic justification of deduction: "Principles of deductive inference are justified by their conformity with accepted deductive practices. Their validity depends upon accordance with the particular deductive inference we actually make and sanction. If a rule yields unacceptable inferences, we drop it as invalid. Justification of general rules thus derives from judgments rejecting or accepting particular deductive inferences. . . . A rule is amended if it yields an inference we are unwilling to accept; an inference is rejected if it violates a rule we are unwilling to amend. This process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either" (Goodman 1979, 63–64).

21. Roger Gibson, in otherwise very careful exegetical accounts of Quine's naturalism, completely misses the importance of the parallelism (Gibson 1987, 1995).

22. "Just as mathematics is to be reduced to logic, or logic and set theory, so natural knowledge is to be based somehow on sense experience. This means explaining the notion of body in sensory terms; here is the conceptual side. And it means justifying our knowledge of truths of nature in sensory terms; here is the doctrinal side of the bifurcation" (Quine 1969, 71).

23. This is explicit in any number of places in Quine. See, e.g., Quine (1981), 70–71.

24. This is, of course, the position of epistemology as Quine portrays it in Quine (1973), 1–4.

25. The scalar notion is suggested by Quine's remarks such as the following: "The fifth move, finally, brings naturalism: abandonment of the goal of a first philosophy. It sees natural science as inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. Naturalism has two sources, both negative. One of them is despair of being able to define theoretical terms generally in terms of phenomena, even by contextual definition" (Quine 1981, 72).

26. The points raised here were emphasized to me by both Peter Markie and Jim Maffie, and I owe the formulation of the problems to their remarks.

27. The question is explicitly raised by Quine, and answered as sketched, in Quine (1995), 251–52. Quine expresses the basic epistemological question in the following way: "Given only the evidence of our senses, how do we arrive at our theory of the world?" (Quine 1973, 1). The "theory of the world" in question embraces both our acquisition of language (in infancy) and the development of mature science. Our mother tongue is our first theory of the world, and natural science is its refinement and extension. The epistemological relation of ordinary language to natural science is fundamental for Quine, and he never substantively alters from the account he gives of it in his 1954 essay, "The Scope and Language of Science" (Quine 1976).

28. The issue of what privileges scientific evidence is an important one, but is tied to considerations involving Quine's understanding of language acquisition. See n. 40.

29. Quine has recently put the matter this way: "Is this sort of thing still philosophy? Naturalism brings a salutary blurring of such boundaries. Naturalistic philosophy is continuous with natural science. It undertakes to clarify, organize, and simplify the broadest and most basic concepts, and to analyze scientific method and evidence within the framework of science itself. The boundary

between naturalistic philosophy and the rest of science is just a vague matter of degree" (Quine 1995, 256).

30. This point is also emphasized in Rosenberg (1996).

31. "Davidson, Rorty and Stroud agree that evidential and causal questions have to be kept strictly apart. *I am not sure Quine would agree.... I think that a thorough-going naturalism should try to combine them systematically....*

"A naturalist in epistemology has the task of showing how any interest in factual and empirical matters can help to explicate and to explain questions of justification.... [M]any traditional epistemologists do nothing more than rely upon their intuitions about epistemic justification.... What we really need is a theory that tells us what justification consists in" (Koppelberg 1990, 208).

32. Can reasons be causes for Quine? I find nothing in Quine's work to suggest that they cannot.

33. Quine, of course, does take it that, e.g., empiricism is a norm which results from the *findings* of science. "The crucial insight of empiricism is that any evidence for science has its end points in the senses. This insight remains valid, but it is an insight which comes after physics, physiology, and psychology, not before." (Quine 1976, 225)

34. The work of van Fraassen (1995) contains a similar charge, viz., that Quine's empiricism is an inadequate account of how science proceeds. However, van Fraassen's account is not Quine's. For one, van Fraassen's empiricist identifies experience as the "one and only source of information" (van Fraassen 1995, 69). But Quine's "unassailable tenets of empiricism" only claim that sensory stimulation is our sole source of *evidence*. And for Quine, evidence is not the same as what van Fraassen takes as information. For example, van Fraassen claims that a Quinean naturalist could not accommodate taking instinct to be the explanation of an infant's breast-feeding behavior (van Fraassen 1995, 76). But this clearly is not correct since Quine allows, e.g., innate quality spaces as an explanation of color perception (Quine 1969, 126; also elaborated in Quine 1973).

35. See also Foley (1994), 257. For a similar complaint, see Solomon (1995, 207 n. 2).

36. Even Quine's views on empiricism are subject to revision. See, e.g., Quine (1995), 257.

37. Foley (1994, 255). For reasons given below, it is not quite correct to say, as Foley here does, that empiricism defines what science is for Quine.

38. I owe this formulation to Jim Maffie.

39. The link Hylton stresses between, on the one hand, ordinary language as embodying our proto-scientific theory and, on the other hand, the role of language learning in guiding our understanding of theory acquisition and development is of the very first importance in understanding Quine's epistemology (Hylton 1994, 270–77). It is a point I have attempted to highlight in my own writings on Quine. See, in particular, Roth (1978) and Roth (1987), chapters 1 and 2.

40. For related remarks, see Quine (1981, 71–72).

41. Quine, in writings of recent vintage, nicely summarizes these aspects of his views.

> Insofar as theoretical epistemology gets naturalized into a chapter of theoretical science, so normative epistemology gets naturalized into a chapter of engineering: the technology of anticipating sensory stimulation.
>
> The most notable norm of naturalized epistemology actually coincides with that of traditional epistemology. It is simply the watch-word of empiricism: nihil in mente quod non prius in sensu. This is a prime specimen of naturalized epistemology, for it is a finding of natural science itself, however fallible, that our information about the world comes only through impacts on our sensory receptors. And still the point is normative, warning us against telepaths and soothsayers. Moreover, naturalized epistemology on its normative side is occupied with heuristics generally—with the whole strategy of rational conjecture in the framing of scientific hypotheses." (Quine 1990, 19–20)

See also Quine (1986, 664–65).

42. Susan Haack is clearly correct to complain about ambiguities in Quine's notions of science, and so his attendant account of naturalism. See, e.g., Haack (1993b), and especially chapter 6 of Haack (1993a). However, as I argued in chapter 2 of Roth (1987), Quine's reductionist and scientistic proclivities about which Haack complains can be separated from his naturalism.

43. There is an additional problem here. As Peter Hylton notes, when Quine asserts that all "genuinely factual questions" are scientific ones, the "phrase is of course the location of a problem: exactly what constitutes a genuinely factual question? Quine's answer here is far from straightforward" (Hylton 1994, 280 n. 18). The terms "genuinely factual" and "science" cannot be defined without reference to one another.

44. Examining what Quine says with regard to a science/non-science partitioning is helpful, if only as a starting point of investigation. To begin, he denies that specifying a demarcation criterion is any part of his project (see Quine 1995, 252). Nor he does require that a science be extensional (see Quine 1990, 72, noting the liberal implication of his "ecumenical" conception of truth in §42). Further, he maintains that prediction is not a norm of science, but that prediction is probative of claims (for individual sentences or groups of them) to scientific status.

> But when I cite predictions as the checkpoints of science, I do not see that as normative. I see it as defining a particular language game, in Wittgenstein's phrase: the game of science, in contrast to other good language games such as fiction and poetry. A sentence's claim to scientific status rests on what it contributes to a theory whose checkpoints are in prediction.
>
> ... [P]rediction is not the main purpose of the science game. It is what decides the game, like runs and outs in baseball. It is occasionally the purpose.... But nowadays the overwhelming purposes of the science game are technology and understanding. (Quine 1990, 20)

As Laudan persuasively argues, considerations drawn from the history of science might well yield normative considerations without abetting demarcation criteria (Laudan 1990).

45. Indeed, in work of later vintage, Barry Barnes cites Quine favorably as an advocate of the study of "natural rationality" in Barnes' sense of the term. In its heart, the sociological project aims, then, to be a naturalistic one (Barnes 1992, 334).

46. There is a disanalogy here I need to acknowledge. Carnap's concern in rational reconstruction was explicitly non-psychological, at least in the sense that he had no interest in the actual genesis of any (scientific) concept. He concern was, for any given theoretical notion, not in showing how it actually might have been acquired, but in revealing a line of reasoning that would show why the concept was rationally justified, i.e., inferable from the evidence in a logically appropriate manner. Social constructionists, however, characteristically write as if they are detailing the genesis of a view, i.e., tracing how it actually was inferred on the basis of prior beliefs. In this sense, the social constructionist view owes more to the phenomenological tradition than the analytic, e.g., to Peter Berger and Thomas Luckmann or Alfred Schutz, than to the Vienna Circle. Nonetheless, my point of analogy rests on the idea of a traceable logical structure from base beliefs to theoretical ones, and on this point the analogy remain valid and is all, I believe, I need.

47. Such prior philosophical commitments frustrate the efforts of some feminist and naturalist philosophers of science to pursue a naturalized and socialized study of scientific reasoning. On the naturalizing side, Miriam Solomon, for example, defends a "social empiricism" (Solomon, in Schmitt 1994). What makes her empiricism social is her view that the attainment of scientific goals is best understood as following a path that leads not through individually selected cognitive processes but, rather, through ones which "aggregate individual and social processes (epistemic, cognitive, motivational, sociopolitical) which are, as a whole, adequately responsive to empirical successes" (Solomon, in Schmitt 1994, 219–20). She suggests that explanations of theory change that screen out everything but scientific success will invariably prove unsatisfactory.

For example, she takes the empirical success of the theory of plate tectonics as insufficient to explain a shift in the scientific community to that view. All fuller explanatorily correct stories involve, she believes, "mechanisms that are traditionally viewed as 'biasing': cognitive salience and availability; motivational biases such as self-interest . . . ; and social factors such as the influence of institutional training and institutional structure" (221). Solomon's social empiricism, then, seeks to demonstrate that the various factors she cites are not necessarily in opposition to one another; biasing factors are not a distortion of the process but part of its natural history. Whatever influences (real world) reasoning of scientists she incorporates as part of a social theory of scientific reasoning.

The key distinction with which Solomon works is the one between "biasing factors" and "empirical success," with the latter defined as "engagements with the world which include experimental, observational, predictive, and some technological successes" (226). But what marks "real" empirical success? For, I would have thought, it is the very lack of a philosophically reputable notion of empirical success that is the reason, in the first place, to look to other sorts of explanatory factors.

Indeed, while her account relies on "empirical success" and "biasing factors" as independent elements of explanation, her case studies suggest a murkier picture in which "success" may not be distinguishable from so-called biasing factors. For example, Solomon contests the view that the plate tectonics revolution is best explained by examining how individual geologists each assessed the data. What then is Solomon's story?

> Consensus took place when plate tectonics had universal (in the scientific community) empirical successes, so that even biased evaluations of the importance of some successes led to the same choice of plate tectonics. That consensus was helped along by salient advertising of new results . . . and bandwagon phenomena. I contend that the successes of plate tectonics were necessary for consensus on the theoty, but not sufficient. It makes sense to say that the scientific community as a whole selected plate tectonics because of its universal successes, while no individual did so. Furthermore, the scientific community did so only via mechanism that are traditionally viewed as "biasing." (221)

The picture she wants to promote is that, as it were, no one person's vote is determinative. It is only when different groups, deciding more or less autonomously of one another, arrive at the same decision that the theory in question "wins." Universal success is success within each subgroup that has a vote on such matters. Individuals acts only derivatively affect change, through membership in relevant groups with a vote in the outcome. In this respect, theory change is a social achievement, not an individual one.

But even if the decision is social in the above sense, how does this make it a story at odds with the one she sets herself against? In the end, the evaluators are individuals, and she says explicitly that empirical success created the consensus. Within each subgroup, either the decision-making process is driven by the empirical successes or by them in some combination with biasing factors. If the former, the biasing factors drop out as a necessary element of the story. But if the latter, we are left again with the problem of how to distinguish what counts as an "empirical success" independent of alleged "biasing factors." So either empirical successes are sufficient to tell the story of change, or they remain obscurely bound up with "biasing factors."

Either so-called biasing factors are constitutive of what counts as empirical success or they are not. If not, as Solomon would prefer, I do not see how empirical success could fail to be sufficient on the story as she tells it. For she says, "Consensus took place when plate tectonics had universal . . . empirical successes, so that even biased evaluations of the importance of some success led to the same choice of plate tectonics" (220). Universal empirical success—successes acknowledged within each subgroup—trumps bias on her own account. Perhaps the other factors she mentions help explain the quickness with which this theory gained acceptance. But what she says appears disconcertingly similar to what she purports to oppose, viz., that "geologists were each persuaded of continental drift by objective assessment of the implications of new geophysical and paleomagnetic data" (220). What her account suggests is only that a theory needs empirical successes within each subgroup, not that non-epistemic factors are necessary to explain success.

Any effort to "split the difference" among the factors would require a reconstruction of how each factor must weigh in judgment. But the historical tapestries are not so woven that we perceive the threads , some white with theory, others black with fact, that we can then neatly unravel and determine how exactly each contributed to the larger picture under study. For, given the concerns that drive one in the first place to doubt efforts at line-drawing between theory and observation or the social and the scientific, Solomon's two factor theory—"biasing" and "empirical success"—is bound, absent a robust philosophical theory of "empirical success," to reduce to one for purposes of explanation. Such reconstructionist gambits are the poisoned pawns of philosophy.

48. Obviously worrisome is d—the "equality of intellectual authority among diverse perspectives." Longino's concern, in formulating d, is an attempt to ensure "free and open criticism referencing some goals" of the community in question (155). I am not aware of actual intellectual communities that satisfy this, and with good reason. Some perspectives do not merit intellectual authority.

49. This account, as I read it, is an elaboration of the "social account of objectivity" broached in chapter 10 of *Science as Social Knowledge*.

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