The Epistemology of "Epistemology Naturalized"

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Abstract

Quine's "Epistemology Naturalized" has become part of the canon in epistemology and excited a widespread revival of interest in naturalism. Yet the status accorded the essay is ironic, since both friends and foes of philosophical naturalism deny that Quine makes a plausible case 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), or cannot legitimate is own basic presuppositions and procedures and so is essentially incomplete as an epistemology (Putnam 1982 and van Fraassen 1995), or is just armchair speculation and so not interestingly different from 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 complaints alleging confusion or vagueness regarding what naturalism is and its sufficiency for the tasks of epistemology.[1]

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[1] 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)

Section I locates the received reading of Quine within taxonomies of contemporary species of naturalism. Section II details an interpretation which challenges this reading and renders nugatory problems commonly attributed to Quine's naturalism. Seen aright, Quine's naturalism indeed radically reconceives the epistemological enterprise, but the character of that reconception differs from that which has previously been emphasized.

I

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.[2] The techniques endemic to the former are only a subset of the historically received and contingently held norms and methods of the latter. Yet even this minimalist formulation engenders 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, in the guise of epistemology, 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 – an effort to 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.[3]

Naturalists such as Quine subscribe to what I term "methodological monism."[4] What marks questions as epistemological for Quine is that they are

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

[3] The "naturalistOs dilemma" is just the epistemological analog of HumeOs 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, esp. [0F] III and IV.

[4] I discovered that Quine uses the term "methodological monism." For 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 v. non-empirical; revisable v. 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, 448-450).

Having elsewhere (e.g., 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" which 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.

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.

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.[5] On this view, philosophy makes independent contribution to the analysis of knowledge.

Three recent efforts to map systematically the peaks and divides within epistemological naturalism – (Maffie 1990), (Kitcher 1992), and (Kornblith 1994) – all situate Goldman and Quine as key points of contrast. 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, 74 ff., 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; each puts Goldman (or a particular version of Goldman) in the former.[6]

The core issue for Maffie's way of dividing the territory between Quine and Goldman is with regard to who endorses, or fails to endorse, full *methodological* continuity between epistemology and natural science. "Unlimited

[5] I discuss these issues in (Roth 1996a) and "Naturalizing Goldman." Much of this section rehearses arguments also found in selection I of "Naturalizing Goldman" (forthcoming in the *Southern Journal of Philosophy*).

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

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." (Maffie 1990, 288) Citing Goldman's work (through *Epistemology and Cognition*) as paradigmatic, a limited naturalism holds that scientific methods do *not* play a role in, e.g., explicating the meaning of epistemic terms or in the identification of the correct norms of epistemological inquiry.[7]

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. On this view psychology replaces epistemology in much the same way that chemistry has replaced alchemy." (Kornblith 1994, 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 asks 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 which distinguishes it from the special sciences.[8] Kornblith 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." (Kornblith 1994, 10-11) What puts psychology and epistemology in the same ballpark is the antiskeptical assumption that we now do know a great many things. (Kornblith 1994, 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 good-making features of processes are, while psychologists could investigate actual processes to learn whether they possess the desired qualities. (Kornblith 1994, 7)

For Kitcher, the defining features of naturalism are, first, the resuscitation of psychology (and possibly biology) as relevant to epistemological analysis

[7] 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).

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

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 Frege and the notably apsychologistic views of the early Wittgenstein. (Kitcher 1992, 59)

While Maffie provides a careful schema which 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)."[9] 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 non-relativized character of the principles which naturalized inquiry seeks to uncovers.[10]

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)

[9] (Kitcher 1992, 58). Kitcher adds, in a footnote at this point, the following observation: "The denial [emphasis mine] 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, fn. 16) Quine is later identified as someone holding exactly that radical naturalist position. (Kitcher 1992, 69-70)

[10] 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), esp. Chs. 5 and 8.

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 multi-faceted. 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." (Kitcher 1992, 68) A consequence is that a naturalistic version of the "meliorative project" might fail to yield an analysis of, e.g., 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 v. 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. (Kitcher 1992, 69 fn 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, non-naturalist 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." (Kitcher 1992, 78) Unlike Goldman, however, Kitcher acknowledges that we stipulate what our epistemic values are. Thus, the dualism at issue in Kitcher's case concerns whether or not to, e.g., 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 one defines 'science.'

Another important problem with Kitcher's proposed way of differentiating radical and traditional naturalism concerns his attribution of the meliorative project to the latter and denying it to the former. Kitcher, as 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." (Kitcher 1992, 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 appeal to an emergent consensus in historically difficult cases. (Kitcher 1992, 97-8) 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.

Radical naturalists, on this account, are pessimists regarding stability; those who would defend a version of traditional naturalism, such as Goldman, Laudan, or Kitcher, are optimists regarding the discovery of stable principles which will improve epistemic performance. As Kitcher remarks, "Naturalism 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." (Kitcher 1992, 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 which 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. (Kitcher 1992, 93; see also 100) Indeed, only the 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. . .." (Kitcher 1992, 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, "We hope, but cannot demonstrate, that the system of predicates we actually use will lead to success in the actual world." (Kitcher 1992, 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 process for all those contexts in which human subjects find themselves." [11] That is, on this view of what the "meliorative project" comes to, it remains incomplete until such a time that we know that the processes in hand are optimal for all possible experiences of which we are capable.

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. [12] *Thus Kitcher transforms traditional naturalism into a surrogate for the very type of a priori project whose rejection he invoked when initially defining "naturalism."*[13] Kitcher's nod towards, e.g., taking into account social factors, 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. [14]

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 Quine-style "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 the now common view when he maintains that Quine cannot accommodate normativ

[11] (Kitcher 1992, 76); (Kitcher 1992, 77, fn. 72) makes clear that this is KitcherOs position.

[12] (Kitcher 1992, 107). A Kantian reading of KitcherOs view is strongly suggested by the characterization of "minimal realism" and cognitive value that he develops at 104-108.

[13] 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 BernsteinOs complaints regarding McDowellOs "naturalized platonism' (with Kantian overtones). (Bernstein 1995)

[14] (Rosenberg 1996) rightly emphasizes the importance of the "Darwinian paradigm" to both KitcherOs and other recent species of naturalism. This paradigm that 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 KitcherOs enthusiasm for the Darwinian paradigm, see (Patricia Kitcher 1992), especially Ch. 7.

ity because by "integrating epistemology into science" a Quinean view "seems to leave us with no resources for making cognitively significant nor mative judgments." (Maffie 1990, 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.[15]

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. First, identify what science is and its methods are. 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 focus 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/evaluation 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 to the hows and whys of naturalizing epistemology.

On 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, EN explicates the consequences of naturalizing epistemo-*

[15] I owe the last two sentences to Jim Maffie.

logy for those prepared, for Quine's reasons or some others, to take Quine's "holistic turn."

The assumed questioned to which EN is to be the answer is: What becomes of empiricist epistemology if it is assumed that, working from within our current system of beliefs, one takes "science" as our best source of justification for beliefs about the world? What becomes of epistemology within those constraining assumptions? His 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, on 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.[16]

Both foundational projects have two aspects: the conceptual and the doctrinal. Conceptual matters are semantical, concerning definition or explication. (Quine 1969, 69) Doctrinal issues concern issues of justification and formal priority. (Quine 1969, 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. (Quine 1969, 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 the former's concern with developing a consistent and complete system for evaluating all empirical knowledge claims.

> 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

[15] I owe the last two sentences to Jim Maffie.

> 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)

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 which 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 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. (Quine 1969, 74)

The problem on the conceptual side is not quite as neat or venerable 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. (Quine 1969, 78-9) 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.[17] 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 psycho-

[17] This is explicit in any number of places in Quine. See, e.g., (Quine 1981), pp. 70-1.

logy; namely the advantage of translational reduction." (Quine 1969, 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.[18] 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.[19] This shift signals a lack of extra-scientific means both for the reconstruction of scientific truths as well as 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 that 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

[18] This is, of course, the position of epistemology as Quine portrays it in (Quine 1973), 1-4.

[19] The scalar notion is suggested by QuineOs 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. "ne of them is despair of being able to define theoretical terms generally in terms of phenomena, even by contextual definition." (Quine 1981, 72)

norms, the reason for their adoption, and the conditions under which they might be subject to change. For insofar as the notion of what science is 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 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, say, to the bases for all human beliefs? What licenses Quine's assertion that science encompasses what we can be properly said to know.[20]

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) Insofar as this sentence is read as *narrowing* the scope of epistemology, it is misread. Quine countenances as a science any form of inquiry which respects the experimental method. He does not mandate invidious distinctions between types of sciences – "hard" or "soft," natural or social.[21]

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

[20] 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.

[21] The question is explicitly raised by Quine, and answered as sketched, in (Quine 1995), 251-2. 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 to our acquisition of language (in infancy) and the development of maturer 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).

for the better."[22] "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 etc. 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.[23] 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.[24]

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

[22] (Quine 1960), 24-5. The issue of what privileges scientific evidence is an important one, but tied to considerations involving Quine's understanding of language acquisition. See below.

[23] 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.

[24] This point is also emphasized in (Rosenberg 1996).

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 human beings, given their information about the world – their stimulus input – might ever have come up with scientific theories such as we possess. 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, the other asks by what methods one reconstructs 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 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 the 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.

> 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 208-9)

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, the foundational projects associated with each *qua* form of knowledge, and what the failure of each such project implies, particularly with regard to the changes rung on the notion of rational reconstruction. In Quine's naturalism, the causal and the normative are just opposite sides of the same scientific coin.[25]

Richard Foley rightly rejects criticisms, such as Kim's, which impute to Quine's procedure an abandonment of a concern with the normative project of epistemology. (Foley 1994, 246-8) Foley has a different question: just how does Quine's approach differ from the non-naturalist epistemological tradition? (Foley 1994, 245) He suspects that the differences are more apparent than real. (Foley 1994, 256)

Foley discerns two possibilities for distinguishing Quine's way of doing epistemology from that of, e.g., Descartes or Chisholm. The first, albeit "uninteresting," differentiating factor is that "the canons of rational belief just are the canons of science, broadly conceived." (Foley 1994, 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 his non-naturalistic predecessors. (Foley 1994, 255)

Foley's second way of distinguishing Quine from non-naturalists emphasizes Quine's rejection of the analytic-synthetic distinction, a move which 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." (Foley 1994, 258) If these norms were themselves shown to be *part* of science, Foley acknowledges, then a genuinely important and interesting

[25] Can reasons be causes for Quine? I find nothing in Quine to suggest that they cannot.

difference between Quine and the tradition would have been identified. For then the normative issues become one with other scientific questions.[26]

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, e.g., 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 – Foley takes to be central to Quine's conception of the scientific project.[27] "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." (Foley 1994, 258-9)[28] 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 which delimit the acceptable epistemic norms for a naturalized epistemologist.[29] 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." (Foley 1994, 255) Can this be done consistent with the approach Quine advocates? Foley allows that it can.[30] 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 aposteriori

[26] 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)

[27] (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 accomodate 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)

[28] See also (Foley 1994), 257. For a similar complaint, see (Solomon 1995), 207 fn 2.

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

[30] (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.

through experimental practice rather than a priori through conceptual analysis or appeal to intuition, and this is what distinguishes Quine from traditional epistemology.[31] 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, "Naturalism 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." (Hylton 1994, 267) Hylton emphasizes, rightly in my view, that Quine's conception of science is primarily methodological. (Hylton 1994, 278)[32]

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. (Hylton 1994, 269)Hylton's complaint underscores the significance of the epistemic scalar hypothesis for Quine's argument as well as the fact it is commonly overlooked. For this shifts the burden of proof to those who would resist or deny the naturalistic turn.

Against, then, 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. On my account, naturalism follows from two assumptions: first, the fact that there are apparently sound arguments to the conclusion the foundationalism is impossible, and second, the normative "scalar hypothesis," i.e., the claim that we possess at present of no better standards of validation for empirical knowledge claims lying between what foundationalism promised and what science itself offers.[33] Given the normative scalar hypothesis, Quine's naturalism subsumes the is/ought gap.

[31] I owe this formulation to Jim Maffie.

[32] 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), Chs. 1 and 2.

[33] For related remarks, see (Quine 1981), 71-2.

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 normative v. descriptive contrast. The epistemologically significant contrast with which Quine is working, rather, is *extra-scientific* v. *intra-scientific*. 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 which 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 which Quine believes science to underwrite. 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) 664-5.

Can a naturalized epistemology be normative? Quine, for one, would not expect it to be otherwise. But this says only that the rules for engineering the success of science are not themselves discovered by some special non-scientific 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 scientists employ. The description of what it is to do science in-

cludes, *inter alia*, the norms relevant to that practice.[34] *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.*[35]

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 fn. 18) "Genuinely factual" and "science" cannot be defined without reference to one another.

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.[36] (See Quine 1995, 252) Nor he does require that a science be extensional. (See Quine 1990, 72. Note 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)

Prediction "defines" science, that is, only in the sense of bounding it, as chalk lines may "define" a playing field. The nature of the play into which prediction integrates is left unspecified. It is not a formulation meant to pith the essence of what to count as science.

[34] 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 Ch. 6, (Haack 1993a). However, as I argued in Ch. 2 of (Roth 1987), Quine's reductionist and scientistic proclivities about which Haack complains can be separated from his naturalism.

[35] There is no gain here to insist that the end is "truth." See references in fn. 5.

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

Following science, much may be let go, e.g., belief in physical bodies as basic. 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, e.g., clairvoyance), 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 which 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.

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 him 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.

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