

THE  
ROUTLEDGE COMPANION  
TO PHILOSOPHY OF  
SCIENCE

*Edited by  
Stathis Psillos and  
Martin Curd*

2008

 **Routledge**  
Taylor & Francis Group  
LONDON AND NEW YORK

# THE EPISTEMOLOGY OF SCIENCE AFTER QUINE

*Paul A. Roth*

My present suggestion is that it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement. Taken collectively, science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one... The unit of empirical significance is the whole of science. (Quine 1961 [1953]: 42)

Few epistemological doctrines seem to fit the sciences more readily than do empiricism, taken as a philosophical doctrine about evidence, and naturalism, understood as a philosophical account of scientific method. Empiricism explains how scientific theories connect to the world; naturalism proposes optimal procedures for learning about the world. But a fundamental problem appears to attach to these doctrines. For the very type of knowledge these philosophical doctrines purport to support and clarify turns out to be implicated in supporting and clarifying empiricism and naturalism themselves. Examining this threat of circularity and its consequences leads, I suggest, to reconceptualizing the status and role of philosophical inquiry vis-à-vis scientific inquiry and empirical knowledge.

"Epistemology," Quine declares in "Epistemology Naturalized," "is concerned with the foundations of science" (1969: 69). Yet, (in)famously, Quine also maintains in the same essay that the relation between epistemology and science is one of "reciprocal containment" (*ibid.*: 83). Because Quine's writings have decisively influenced two lines of debate within epistemology generally and the relation between epistemology and science in particular – holism and naturalism, respectively – his account provides a convenient basis for surveying how these debates have evolved. My particular concern will be, in line with the Quinean perspective adopted herein, determining in what respects empiricism remains epistemologically fundamental as an account of scientific knowledge.

In what follows, I offer a sketch of a movement in twentieth-century epistemology from what I term a "bottom-up" to a "top-down" approach regarding the relation of epistemology and the sciences. This will follow lines of argument found in

"Epistemology Naturalized" by tracing the development of the arguments that systematically strip away attempts to justify science independently of science. This engenders key problems in specifying what to count as empirical, and so as evidence for and against individual scientific claims. This turns out to be the crucial step in Quine's naturalism, i.e., elimination of philosophy as a form of inquiry independent of science. Yet against those who maintain that Quine's blurring of the lines between speculative metaphysics and science represents a politically (if not philosophically) retrograde move, I indicate how Quine's holism and naturalism helped motivate and make possible a proliferation of alternative approaches to the study and understanding of science. Making explicit this connection allows a somewhat different perspective on the current disputes between philosophers of science and science studies researchers.

Towards that end, consider reference to "the whole of natural science" from "Epistemology Naturalized" (written *circa* 1968) in light of the context of an earlier use of that phrase in "Two Dogmas of Empiricism" (*circa* 1950-51). In the latter case, Quine urges a vast enlargement of the unit assessed as having (or lacking) empirical significance. In the former, he declares for naturalism, i.e., treating epistemological questions as questions within science, and so using science to account for how humans manage to acquire such knowledge. By implication, the notion of *empirical significance* must itself be subject to naturalistic scrutiny along with all other aspects of scientific method and theorizing.

By unpacking just why Quine makes use of so vague a phrase reveals just how radically Quine's critique of empiricism forces a reconception of the relation between epistemology and the philosophy of science. In particular, I suggest, terms such as "empiricism" no longer hold promise of epistemological insight regarding the basis for scientific knowledge. Empiricism simply ceases to have standing as an epistemological doctrine *apart* from science. It becomes, rather, a *consequence* of naturalism (and pragmatism), a thesis about the nature of scientific evidence maintained on the basis of scientific investigation (see Nelson and Nelson 2000).

### Empiricism, epistemology, and science in "Two Dogmas"

With regard to knowledge of the external world – empirical knowledge – Quine takes "empiricism" to name a theory of evidence – sense impressions – that provides the fundamental basis for legitimating all beliefs about what there is. In "Two Dogmas," Quine challenges a traditional empiricist view that one can discriminate by semantic criteria alone exactly which statements evidence supports (or not) and which need no evidential support because they are true "come what may." This challenges the positivist claim to be able to distinguish between statements that are meaningful and those that are not, and so, as Quine states, blurs the boundary that positivism attempted to put in place "between speculative metaphysics and natural science."

Quine's two key lines of argument go as follows. First, he gives reasons to doubt that we can classify sentences in a way that would permit us to identify just some but not others as expressions of empirical knowledge. Second, he extends this doubt about distinguishing between what stands in need of empirical confirmation and what

e arguments that system-  
f science. This engenders  
d so as evidence for and  
e crucial step in Quine's  
y independent of science.  
lines between speculative  
ilosophically) retrograde  
ped motivate and make  
dy and understanding of  
t different perspective on  
ience studies researchers.  
of natural science" from  
the context of an earlier  
50-51). In the latter case,  
ing (or lacking) empirical  
, treating epistemological  
o account for how humans  
ion of *empirical significance*  
other aspects of scientific

a phrase reveals just how  
on of the relation between  
; I suggest, terms such as  
ight regarding the basis for  
ding as an epistemological  
*fluence* of naturalism (and  
ce maintained on the basis

### Two Dogmas"

l knowledge – Quine takes  
ssions – that provides the  
here is. In "Two Dogmas,"  
n discriminate by semantic  
s (or not) and which need  
may." This challenges the  
ments that are meaningful  
e boundary that positivism  
cs and natural science."  
, he gives reasons to doubt  
nit us to identify just some  
ond, he extends this doubt  
rical confirmation and what

does not to include finding "equivalences of meaning" between linguistic items such as sentences and non-linguistic ones such as sense impressions. For if the notion of "equivalence of meaning" cannot be cashed out in terms of the constituent elements of so-called analytic statements, the notion cannot be made to work for alleged equivalences between linguistic and non-linguistic items. In this respect, at least, the "two dogmas are, indeed, at root identical" (see Quine 1961 [1953]: 41; see also Ben-Menahem 2005).

Quine's "countersuggestion" that the measure of epistemic goodness be taken as the "whole of science," in sum, raises two key questions regarding how to construe the relationship between epistemology and philosophy of science. For his phrase "the unit of empirical significance," the term "empirical significance" should be understood as "meaningful in terms of experience." But the problematic terms – the questions invoked by the phrase – involve the terms "unit" and "empirical." For a unit to be a unit, it must be bounded. So, the first question to be answered would be: What bounds or determines the unit tested for empirical significance? The second question concerns the epistemic work to be done by an appeal to a notion of the empirical. Presumably, the job of the empirical should be to provide some evidential basis independent of the science being evaluated for the assessment of scientific claims. For otherwise the unit under test certifies as appropriate the elements used to test it. And this renders unclear the nature of any claimed epistemic advantage.

The allusion to the "whole of science" suggests that any attempted epistemic assessment of a single belief implicates *all* those beliefs comprising that theory to which a sentence belongs. For how what go by the label "beliefs" (sentences held true) and how what goes by the label of "experience" (perception) *fit* together can be logically accommodated in any number of ways. Attempts to differentiate structurally among types of linguistic items and to identify a tight logical and evidentiary fit between the linguistic and the non-linguistic ultimately reveal that there exists no such logically neat interrelationship between how the world works on us and what we think about it. In this regard, attempts to distinguish between, e.g., some type of limited holism and a more global form presuppose an ability to mark off one type of theory (e.g., those in physics) from other types (those in economics). But our beliefs do not come so neatly packaged, and their areas of possible interdependence or independence so clearly marked. The problem involves just the inability to logically specify which beliefs might be revised should experience disappoint expectations (see Nelson and Nelson 2000, esp. Ch. 5).

Reflections on the logic of science, the history of science, and the sociology of science all confirm this point, each in its own way. (Let me be clear here that what I take to be called into question involves a notion of *the empirical* or *experience* that can be made sense of as epistemically basic independently of appeal to science.) But why then believe that there exists any epistemic leverage in appeals to *the empirical*?

The two questions – the unit of empirical significance and the content of the notion of the empirical – moreover, prove deeply interrelated. For a variety of scientific theories (broadly construed, so as to include the social sciences) serve to determine just which experiences count and under what conditions they count as

relevant for assessment purposes. Science ultimately delimits, e.g., how many senses there are, how they function, and so what even the senses properly so-called *could* provide *qua* evidence. Both questions give rise to worries about how diffuse the notion of the empirical becomes once it cannot be restricted to terms or simple statements.

One of the most philosophically unsettling consequences of epistemic assessments so conceived involves the many ways of accommodating experience to theory. Conceiving of the theory–evidence relation as interrelated and logically diffuse receives further reinforcement from significant pre-Quinean historical and philosophical work by Duhem as well as the powerful influence of work in the history and philosophy of science by Kuhn and others who came later. In addition, as Ian Hacking (1989) insists, questions of how to sort experiences into kinds remain vexed and unanswered.

### Science without foundations

If notions of sense and sensing themselves require scientific investigation in order to articulate the respects in which they support science, then the very empirical base to which science appeals becomes one best understood through science. Thus, in charting how the “unit question” and the corresponding “experience question” evolved to something like their present forms, an understanding emerges regarding how these notions in turn affect what the terms “epistemology” and “science” connote. Unlike empiricists of old, Quine does *not* look to the notion of experience to clarify those of thought or belief: all three, he maintains, stand in need of clarification. Quine links the notions of meaning, thought, belief, and experience as kindred concepts in the sense that “they are in equal measure very ill suited for use as instruments of philosophical and scientific clarification and analysis. If some one accepts these notions outright for such use, I am at a loss to imagine what he can have deemed more in need of clarification and analysis than the things he has thus accepted” (Quine 1981: 184). In particular, by conceiving of the notion of empirical knowledge as of a piece with the articulated theorizing of experience that sciences provide, the suggestion regarding the unit of empirical significance made in “Two Dogmas” turns out to imply the “reciprocal containment” of science and epistemology proposed in “Epistemology Naturalized.” (How Quine’s declaration for pragmatism in “Two Dogmas” fits with his later declared naturalism poses an interesting but, to the best of my knowledge, presently unanswered question.)

In understanding how to disentangle this relationship of reciprocal containment, it helps to appreciate the deep link between Quine’s critique of the notion of analyticity and his critique of positivist, and particularly Carnapian, conceptions of mathematics. For example, although Quine uses remarks about foundational studies in mathematics to frame the challenges to epistemology as he understands them in “Epistemology Naturalized,” this framing remains almost universally ignored in subsequent discussions of Quine’s essay and his account of naturalism (see Roth 1999).

On my telling of the tale, the epistemological program Quine advocates – and, *inter alia*, what he means by “naturalism,” “epistemology,” and “science” – involves

assessing the fate of empirical knowledge once attempts to *ground* such knowledge meet a fate that parallels attempts to ground mathematical knowledge. The primary argument of "Epistemology Naturalized" elaborates the parallel types of problems or failings that plagued both mathematical and empirical knowledge, and how those problems transform or otherwise alter what such knowledge comes to in each case.

Quine develops the parallelism in two respects, which he terms the "conceptual" and the "doctrinal." Conceptual matters are semantic, concerning definition or explication. Doctrinal issues involve justification and formal priority. 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. The intended parallel would then be to the logicist program for having a consistent, fully axiomatized, and complete set of rules adequate to all of mathematics. This approach, had it succeeded, would have provided an analysis, in the best understood sense of the term, of the entire range of truths about the world.

Yet, Quine argues, the project for providing foundation for science (i.e., for empirical knowledge) parallels the fate of the logicist project in mathematics. On the doctrinal side, the project falls because of Hume's problem – generalizations from experience outrun evidence for them. Hence, derivation of scientific laws proves impossible.

The problem on the conceptual side is not quite as neat or as venerable. For here the principal difficulty resides in the relation of the theoretical sentences and the evidence adduced in their support, i.e., holism. For holism (of the Quine-Duhem sort) forecloses the possibility of the sort of term-by-term explication that the foundational project presupposes and requires. There are, then, two irremediable failings in the case of empirical knowledge. Neither laws nor concepts can be accounted for as hoped, i.e., in terms of sensory impressions and logic alone. This dashes any hope of finding within empiricism a philosophical foundation for science. As a result, empiricism becomes itself an hypothesis within accepted science, one that helps explain why science provides the engineering success that it does. It also leaves us without a justificatory standard better than those that the sciences (broadly and collectively understood) themselves provide, since that "better standard" – deductive justification from a specified base – is not to be had. The incompleteness results for empirical knowledge, in short, redefine what can be hoped for or expected by way of justification of empirical knowledge.

In this regard, Quine's use of the term "naturalism" must be treated circumspectly, since a definition of "naturalism" typically makes reference to the "methods of science," yet what to count as science cannot be readily taken for granted in Quine. There is no small irony in the complaint that Quine's notion of naturalism is vague. For it typically emanates from those who assume that they know exactly what science is or what epistemology is, and this despite lacking a demarcation criterion for the former or settled explications of belief, justification, and truth for the latter.

### Science fully naturalized

Ironically, this relocation of empiricism within science breaks down whatever divides may be thought to remain between philosophy of science and science studies. Philosophy of science and science studies were distinguished primarily by the elements that were cited in the *explanans* for a given *explanandum* event (e.g., theory change, theoretical commitment, confirmation). Typically, philosophers downplay and science studies' practitioners emphasize how the practice of science stands implicated in the customs and mores of those societies in which the science takes place.

I suggest that those problems that led, in the first place, to the expansion of the unit of empirical significance and the *theorizing* of the empirical make moot those disputes. What counts as experiences and how to assess their effect (e.g., social psychology v. neurology) will depend in part on the science at issue. For while socially mediated experiences cannot, in principle, be excluded from epistemological consideration, attempts to map those experiences to individual beliefs remain subject to all the usual indeterminacies. In this respect, the key problems inherent in the epistemological project on the philosophical side – bounding the unit of experience and theorizing the empirical – emerge, like the return of the repressed, in science studies' efforts to provide a "social epistemology."

Indeed, many debates regarding the epistemology of science – the rationality of theory choice, accounting for theory change, hypothesis acceptance – that divide philosophical and sociological accounts of scientific claims actually split on the question of which experiences prove relevant to explaining scientific claims. Sociologists claim to favor causal explanations of beliefs and philosophers prefer reason-based justifications. Put another way, one means by which to understand disputes in the area of science studies, at least with regard to the explanation or assessment of scientific knowledge claims, would be to take them as disagreements regarding what tests test, and even which aspects within the experience of individuals bear on the assessments of epistemic claims.

Consider, for example, how accounts offered by Galison (1987) differ from what one finds in Pickering (1986). Both of these accounts, moreover, appear to be relatively *internal* histories – they do not look much beyond the scientific communities. But Galison emphasizes how debate in a scientific community becomes settled by citing the reasons which prevailed, while Pickering emphasizes unacknowledged concerns – for instance, the need to be able to *recycle expertise* and yet have a more viable theory – as leading scientists to favor one view over another. These approaches can be contrasted in turn with, for example, Shapin and Schaffer (1985), who take a yet wider view of the factors determining one's theoretical preferences. Background beliefs regarding social status or religious affiliation might influence which individual beliefs count or how they count. In addition, which beliefs might be open to revision will be determined by perceptions regarding how those beliefs connect to religious or political views deemed important. Consideration such as these makes the "unit of empirical significance" culture sized.

In saying this, I acknowledge some discomfort in moving from theories conceived of as linguistic entities to cultures so conceived. As I indicate in what follows, the

question of the relevant "unit" being assessed has become increasingly diffuse and problematic. I find no general answer to the question of how to bound or otherwise specify the unit in which to embed the epistemic evaluation of a specific scientific claim. Debates need to proceed on a case-by-case basis in this regard. From this perspective, the label "naturalism" only obscures uncertainties regarding the scope and content of the present notions of science and experience.

Questions concerning the unit assessed and which experiences serve to assess also affect questions of how to distinguish between epistemic norms and the methods of epistemology and scientific norms and the methods of science. For first philosophy regards (as it must) epistemic norms and methods as *independent* of natural science. Scientific knowledge, properly so-called, would then be a consequence of the units (typically, sentence sized) certified by the right epistemic processes, whatever those may be taken to be. This leads to a *bottom-up* strategy. The sort of esoteric and non-observational claims to know made within particular natural sciences count as knowledge provided that they can be legitimized by iteration of those methods and norms – whatever they are – for certifying, for example, basic perceptual statements or clear and distinct ideas.

By contrast, if epistemology can claim no norms or methods certified by procedures that stand aloof from all other modes of inquiry, then epistemology proceeds from within science. First philosophy requires an account of epistemic assessment that can be independent of science. But Quine argues that we cannot successfully isolate the preferred empiricist standards – analyticity, experience – and that this failure turns on irremediable problems concerning the character of word-world connection. He offers as an alternative account one in which science should be understood as ranging over just the panoply of norms and methods deemed legitimate for purposes of inquiry. Epistemology, so conceived, becomes a *top-down* investigation, at least in the following sense. Evaluation assumes a certain theoretical stance, and from within that stance proceeds to make what sense it can of our putative sense-making procedures and claims.

Thus, I take there to be a type of affinity between, on the one hand, the alleged independence of epistemology and a bottom-up strategy as opposed to, on the other hand, conceiving of epistemology as pursued from within a scientific account of the world. Epistemology-within-science proceeds top-down, that is, by asking how, given an explanatory theory and its justificatory norms provisionally accepted, to encompass within it a justificatory account of their acquisition and justification. Naturalizing epistemology by making it part of science exemplifies this top-down strategy. Bottom-up strategies take an ultimately dogmatic stance (knowledge begins *here*), while top-down strategies allow for a pragmatic approach to judging a theory's merit.

From the standpoint of examining the relationship between philosophy of science and epistemology, those strategies yield very different results. Viewed bottom-up, justification consists only of inferential links. Traditional puzzles here concern justifying generalizations – typically, laws; related epistemic problems involve articulating the logic that connects evidence to experimental tests, experimental tests to theories, and the logical connections that exist among those statements comprising a scientific theory. Epistemic evaluation involves justified inference and nothing else.



Hans Reichenbach offers a straightforward and representative formulation of this view: "The essence of knowledge," he declares, "is *generalization*." Moreover: "Generalization ... is the origin of science" (Reichenbach 1951: 5). Although the strategy for assessing laws must be bottom-up – from evidence of experiment to laws – once the laws are in hand, epistemology becomes top-down.

Reichenbach's core philosophical question asks how knowledge of the world manages to transcend what observation alone provides. His answer echoes themes repeated frequently during the first half of the twentieth century, viz., that the philosophical study of science can clarify the inferential processes that lead from experience to theory. For science trumps claims of common sense because of its superiority in explaining how things, in general, hang together. Science can explain what passes for common sense; common sense cannot account for scientific understanding. The study of inference, moreover, marks the special, albeit limited, place for philosophy.

Quine takes science to be about trying to construct a "systematization of our sensory intake" (Quine 1995). The initial systematization comes with learning the language one first learns to speak, and of the objects and events about which we communicate with others. The "reciprocal containment" of epistemology and natural science takes epistemology to be a part of an attempt to systematize experience. But, though only an aspect of the scientific enterprise, epistemology so conceived contains the scientific enterprise, since all of it results in the end from shared stimulations. Quine's reconceptualization of knowledge still takes knowledge to be the best systematic account for beliefs held, but takes science to constitute this.

Quine's very liberal view of what to count as science can be adopted here without epistemological loss. For by taking science to be just the extensional equivalent of those empirically oriented disciplines and their collective methods, one does not assume the burden of discerning deep relations between, for example, physics and history, on the one hand, while, on the other hand, one can criticize freely those forms of inquiry, for instance, astrology, that might assume some of the techniques of science (measurement, prediction) but without the desired results. The appeal to the empirical remains one that the sciences themselves endorse, but it may be jettisoned if results warrant that conclusion. As Quine somewhere remarks, should a ouija board prove a better predictor than physics, it would be pragmatically rational to abandon physics and go with the ouija board.

Those favoring philosophy of science as epistemology most characteristically insist on the virtues of systematicity and explanatory power. Those favoring the ordinary (i.e., those who take as the work of epistemology an analysis of the great many truths already known prior to science) most typically appeal to truths known as truths *prior* to any investigation and which any plausible theory of knowledge must yield as a result. In this regard, circular reasoning might be thought to undercut the above characterization. For Reichenbach's assertion that science "requires a reinterpretation of everyday life" already decides what for many epistemologists remains the fundamental question at issue: What does a theory of knowledge need to be a theory of?

### Naturalism and normativity/politics and epistemology

Indeed, debates regarding the role of natural science in and as epistemology proceed under the rubric, in the current philosophical climate, as debates about the role of naturalism. I take these debates, that is, to be just disputes as to whether and how an empirical theory can play a role as an epistemological theory. The nub of this debate centers on the claim that epistemology provides a normative theory and that no scientific theory can provide an account of norms since such theories simply account for (describe) the world and so cannot determine what the standards of knowledge ought to be.

Scientific theories presumably might employ such standards, but it falls to philosophy to discover and account for the norms determinative of knowledge. In this regard, disputes about naturalized epistemology focus less on what it is for epistemology to be naturalized than on what qualifies a naturalistic/scientific approach as epistemology.

Some recent work illustrates problems connected to the "unit question" and the "theorizing of the empirical" by exploring debate about these issues within American philosophy of science and pragmatism, and various European imports (from logical empiricism to Marxism). In their Introduction, Hardcastle and Richardson (2003) correctly acknowledge that "the best current tool for understanding 'analytic philosophy' must surely be sociology of knowledge, especially the notion of 'boundary work'." Alan Richardson stresses an intellectual evolution within naturalism and pragmatism from Dewey and Morris, on the one hand, to Quine, on the other. On Richardson's account, Morris and Dewey view science as a tool for progressive politics, while Quine decouples naturalism from any progressive view of science. For Quine, science neither progresses (if "progress" means "comes closer to the truth"), nor does it provide a basis either for enlightened politics (which would be another form of progress).

Richardson, in particular, emphasizes that in the debate between Quine and Carnap, the "semantic, pragmatic, logical, epistemological, scientific, 'natural,' formal, and metaphysical are at stake all at once". On Richardson's account, the Morris and Carnap conception of scientific philosophy was structured so as to exclude traditional metaphysics or epistemology. Precisely by limiting the scope of the intelligible, philosophy of science was to clarify *philosophical* disputes. Since inferential relations (including inductive inference) could be explicated without appeal to values, the clarificatory role of logic allowed real progress (both intellectual and political) in debate to be achieved. Critiques of, for instance, Heidegger by positivists attempted to show just how this was to go. By making precise the notion of inference, philosophy could be of social utility by debunking efforts to rationalize certain types of claim.

Yet the political utility of philosophy of science and logic to debunk requires – in the view Richardson finds in Morris and Carnap – the separation of the logic of inquiry from explicitly normative concerns. But in order to maintain their concept of a neutral "logic of science," Morris and Carnap needed the notion of analyticity. Hence, Quine's critique reverberates across a very broad intellectual and cultural front.

On this reading, it comes as no surprise that Richardson situates Quine's naturalism as "conservative." For Quine famously declares at the close of "Two Dogmas" that the rejection of the analytic-synthetic distinction blurs the "supposed boundary between speculative metaphysics and natural science." Yet it was the drawing of this boundary that underwrote the political utility which Carnap and others conceived the philosophy of science to have. Quine's skepticism also muddies ethical waters. For Carnap stresses the element of choice in the selection of frameworks in order to indicate that our way of understanding the world involves an element of free choice, and so an action for which one bears responsibility. Blurring the boundaries between theory and experience blurs questions of responsibility because how beliefs map on to experience, and so rationalizations of what one believes, loses just the sharpness and clarity that gave it some political purchase. No one adaptation of experience to belief necessarily counts as more rational than some other.

Richardson thus terms Quine's pragmatism "thin" because Quine does not address questions of policy or action. For Quine, pragmatic considerations enter in with respect to how the web of belief gets warped in order to incorporate recalcitrant experiences. No sentence stands aloof from revision, including the putatively analytic ones. Choice of frameworks, in this regard, does not insulate from revision statements assumed to constitute the framework of inquiry. Construing all statements as potentially revisable, however, scotches the hope that philosophy of science could serve the cause of political demystification by appealing to the independence of logic and purely inferential connections between evidence and beliefs. What counts as "pragmatic" turns out only to be how from within the framework one adjudicates questions of confirmation and so the *adjustment* of beliefs. Likewise, without a purely logical criterion for which beliefs *ought* to be revised in light of the experience, philosophy provides no objective guide to action. In one important sense of "pragmatic," philosophy loses its pragmatic value.

But Richardson surely takes a misstep when he then goes on to claim that "Quine's naturalism is intellectually conservative" inasmuch as it "opens up a way back into metaphysics and epistemology and changes the revolutionary, forward-looking rhetoric of logical empiricism and American pragmatism into a story of continuity going back all the way to Locke and Hume" (Hardcastle and Richardson 2003). For here Richardson seems strangely blind to the radical upheavals that did in fact follow from the changes rung by Quine on "the unit of empirical significance." While Quine's critique does not allow the critique of metaphysics to serve the political purposes some positivists had hoped, it does serve (unwittingly, I suspect) to broaden (and so, in one sense, liberalize) discussion of the factors that play into scientific decision-making.

Richardson bemoans Quine's version of pragmatism since it does not dictate how to revise beliefs in the face of experience. Yet that very feature of Quine's thought becomes a license for insisting on the relevance of the sociology of science. Relatedly, Richardson's linking of Quine's project to the empiricist tradition of Locke and Hume misses precisely what makes Quine a philosophical radical because of his thorough and substantive reconceptualization of empiricism, science, and epistemology.

Keep in mind that for Quine the social aspect of the story remains key: "Language is a social art. In acquiring it we have to depend entirely on intersubjectively available

cues as to what to say and when" (Quine 1960). Additionally, and even more importantly, this change shifts accounting for beliefs from inference alone (as Reichenbach thought) to inference or explanation. This *mixing* of inferential and causal accounts leaves some beliefs *unrationalized*. But which? For reasons alluded to above and now well known, inferential considerations alone do not mandate how to adjust beliefs in the face of recalcitrant experience. While this frustrates those who would like to see each individual belief assessed by its rational merits, it also allows seeing change in belief as a function of change in circumstance. As noted below, each conception of belief change carries with it its form of political critique.

Richardson worries that Quine's turn away from the analytic-synthetic distinction was a turn toward "conservative" thought, at least insofar as the *failure* of scientific prediction did not necessarily direct one to which associated belief to revise. Hence his concern that Quine's "thin" pragmatism is no pragmatism worthy of the name, since it fails to direct action. But this only brings into a focus a part of the epistemological story, and ignores much of what has actually transpired in the wake of Quine's work. For the practical upshot of those reflections has not been the sort of intellectual paralysis or ennui about which Richardson appears to worry, but a proliferation of non-philosophic accounts of what scientific theories just are theories about. The effect has been the creation of an unruly but not-to-be-denied *social* approach to epistemology. The hallmark of this approach, or at least the aspect of greatest interest to those concerned with the relation of science and epistemology, involves the inclusion of various factors – race, gender, class – said to influence the imputed "logic" shaping theories and the criteria for judging them adequate.

While the sociology of science has flourished in the wake of philosophic work criticizing the supposed epistemic foundations of science, too much of this sociological work simply seeks to redo by means of a social logic what could not be done by more austere formalisms. The results prove correspondingly (and unsurprisingly) unsatisfying. The obsession with theoretical formulation brings out the worst in both philosophical and social-cultural analyses of science. More interesting than the now well-rehearsed shortcomings of understanding science in purely inferentialist or theory-centric terms are laboratory-centered studies of how science succeeds when it does. For the account of *knowledge production* that emerges in these contexts provides a much better sense of how theory connects to the world, and what it takes to make this connection succeed.

Quine's conceptualization of the relation of epistemology and science proves deeply ironic. Empiricism requires science to explicate that notion – experience – on which, in turn, to base confidence in science. A further irony involves the fact that the proposed unit of empirical significance – "the whole of science" – cannot itself be tested *qua* unit. So confidence in the whole of science cannot be licensed in this way – the way in which science supposedly issues such license. What then guides changes rung on scientific theories? Quine appears to endorse a "pragmatic" basis for such change (Quine 1961 [1953]). And while Richardson protests that Quine's blurring of boundaries fails to be pragmatic because it provides no neutral guide to change, that blurring helps underwrite Quine's view that there exists no point of cosmic exile (Quine 1969), and so makes adjustment a pragmatic rather than purely logical

matter. A final, albeit surely unintended, irony then situates Quine with Heidegger and against Carnap in seeing humans as having a choice at every level of their understanding of the world (see Stone forthcoming).

### Acknowledgements

I thank David Hoy, Kaija Mortensen, Abraham Stone, and Stephen Turner for helpful comments on an earlier draft of this essay.

See also Empiricism; The historical turn in the philosophy of science; Naturalism; Scientific method; Social studies of science; Underdetermination.

### References

- Ben-Menahem, Yemima (2005) "Black, White, and Gray: Quine on Convention," *Synthese* 146: 245–82.
- Galison, Peter (1987) *How Experiments End*, Chicago: University of Chicago Press.
- Hacking, Ian (1989) "Quine's Natural Kinds," in R. Gibson (ed.) *Perspectives on Quine*, Oxford: Blackwell, pp. 129–42.
- Hardcastle, Gary L. and Richardson, Alan (eds) (2003) "Introduction," *Logical Empiricism in North America*, Minneapolis: University of Minnesota Press.
- Nelson, Lynn Hankinson and Nelson, Jack (2000) *On Quine*, Belmont, CA: Wadsworth.
- Pickering, Andrew (1986) *Constructing Quarks*, Chicago: University of Chicago Press.
- Quine, W. V. (1960) *Word and Object*, Cambridge, MA: MIT Press.
- (1961 [1953]) *From a Logical Point of View*, 2nd edn, rev., Cambridge, MA: Harvard University Press.
- (1969) *Ontological Relativity and Other Essays*, New York: Columbia University Press.
- (1981) *Theories and Things*, Cambridge, MA: Harvard University Press.
- (1995) *From Stimulus to Science*, Cambridge, MA: Harvard University Press.
- Reichenbach, Hans (1951) *The Rise of Scientific Philosophy*, Berkeley: University of California Press.
- Roth, Paul A. (1999) "The Epistemology of 'Epistemology Naturalized'," *Dialectica* 53: 87–109.
- Shapin, Steve and Schaffer, Simon (1985) *Leviathan and the Air Pump*, Princeton, NJ: Princeton University Press.
- Stone, Abraham (forthcoming) "Heidegger and Carnap on the Overcoming of Metaphysics," in Stephen Mulhall (ed.) *Martin Heidegger*, Burlington, VT: Ashgate Publishing.

### Further reading

An excellent overview of the consequence of Quine's "liberalization" of the notion of empirical significance is John Zammito, *A Nice Derangement of Epistemes* (Chicago: University of Chicago Press, 2004). Some sense of the diversity of issues here and the numerous perspectives from which these are explored can be found in Paul A. Roth "Will the Real Scientists Please Stand Up? Dead Ends and Live Issues in the Explanation of Scientific Knowledge," *Studies in the History and Philosophy of Science* 27 (1996): 813–38. A philosophically conservative account is Larry Laudan, *Beyond Positivism and Relativism* (Boulder, CO: Westview Press, 1996); a more radical and polemical introduction is Steve Fuller's *The Philosophy of Science and Technology Studies* (New York: Routledge, 2006). Interesting and challenging approaches to the set of issues scouted in this essay include: Joseph Rouse's "What Are Cultural Studies of Scientific Knowledge?" *Configurations* 1 (1993): 57–94; Donna Haraway's "A Game of Cat's Cradle: Science Studies, Feminist Theory, Culture Studies," *Configurations* 2 (1994): 59–71; and Steve Fuller's *Thomas Kuhn: A Philosophical History for Our Times* (Chicago: University of Chicago Press, 2000). For a range of critical reactions to Fuller's book (including my own), see *Social Epistemology* 17 (2003): 2–3. The *locus classicus* for the science studies approach remains B. Latour and S. Woolgar, *Laboratory Life* (Beverly Hills, CA: Sage, 1979).