# DISCUSSION:

# SIEGEL ON NATURALIZED EPISTEMOLOGY AND NATURAL SCIENCE*

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**Introduction.** What is the relation of epistemology, understood as the study of the evaluation of knowledge claims, and empirical psychology, understood as the study of the causal generation of a person's beliefs? Quine maintains that the relation is one of "mutual containment".

> Epistemology in its new setting, conversely, is contained in natural science, as a chapter of psychology. . . . We are studying how the human subject of our study posits bodies and projects his physics from his data, and we appreciate that our position in the world is just like his. Our very epistemological enterprise, therefore, and the psychology wherein it is a component chapter, and the whole of natural science wherein psychology is a component book—all this is our own construction or projection from stimulations like those we were meting out to our epistemological subject. There is thus reciprocal containment, though containment in different senses: epistemology in natural science and natural science in epistemology (Quine 1969, p. 83).

His suggestion is to merge the evaluation of knowledge claims—the epistemological enterprise—with empirical psychology—the scientific enterprise—which studies the causal origins of man's beliefs.

However, Quine's proposal might appear to go wrong at the start. For it charges the scientific epistemologist with a traditional task, viz., the task of determining how to substantiate knowledge claims, which simply seems unfulfillable by the methods Quine allows. Specifically, Harvey Siegel contends that Quine has confused the process by which a scientific discovery is made and the method by which a scientific claim is justified. The problem is that "psychology *cannot* contribute to an account of the

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reason we have for taking a potential justification to be (or not) genuinely justifactory. The production and study of such reason is the task of the epistemologist" (Siegel 1980, p. 320, emphasis mine). Moreover, Siegel's criticism has considerable plausibility. For, as he reads Quine, Quine's proposal entails a denial of Reichenbach's distinction between the context of discovery and the context of justification. Yet this denial, Siegel charges, is based on a confusion between the explanation of the psychological origin of a claim and the evaluation of it. Quine's mistake, in short, is that his proposal to use science to study knowledge acquisition forces the epistemologist to commit the genetic fallacy, and so to be unable to do epistemology.

Quine's proposal does, indeed, represent a sharp break with the programs of his positivist predecessors. Quine chooses to use *only* science to justify what we claim to know and not, as has been traditionally the case, to seek a justification of scientific knowledge by appeal to some philosophically firmer ground. On this account, the epistemologist is free to use science to answer traditional epistemological questions in the philosophy of science, e.g., the question of what reasoning links observational data to theoretical posits. "Our liberated epistemologist ends up as an empirical psychologist, scientifically investigating man's acquisition of science" (Quine 1974, p. 3). We use the resources of natural science to study the psychological process of acquiring beliefs, including our belief in this selfsame science. For this reason, psychological inquiry should prove epistemologically illuminating. Epistemology so conceived is what Quine calls "epistemology naturalized".

In Part I, I examine the particular criticism which Siegel develops of Quine's proposal. I argue, in Part II, that Siegel's criticism is misplaced because he fails, for reasons not his alone, to appreciate just how radical Quine's proposal is. Moreover, I suggest that, although radical, this proposal is a consequence of Quine's view of the sentences of a theory as having their meaning and evidence only as an interrelated set (his Duhemian view of theories).

## I

Siegel charges that Quine is insensitive to the distinction between the use of scientific method and the evaluation of that method. One task of the epistemologist is to explain the "justificatory force" of good arguments. A philosopher of science explicates what it is about the method of science, apart from content, which makes a good scientific argument good. "An epistemologist, *qua* epistemologist, does not pass judgment on competing [scientific] accounts . . .; he/she does, however, study the general features of such scientific judgments—that is, an account of why the judgments made in the special sciences are (or are not) properly considered justificatory. And it is this sort of account, I have been arguing, that psychology cannot contribute" (Siegel 1980, p. 320). The distinction, in short, is between a study of the accepted canons of inquiry and a study of psychological associations (which are idiosyncratic).

A related problem is that the process of scientific discovery may be fueled by insights unrelated to scientific method. But the patterns of associations which generate such insights are not of interest to the epistemologically oriented philosopher of science. For these patterns of associations are not patterns of justification for scientific claims. "The evaluative task . . . is the only concern that can properly be called epistemological. It alone is appropriate to the context of justification. And this substantiation does not depend on the psychological origins of the proposed model" (Siegel 1980, p. 300). The distinction between the *practice* of science and the *evaluation* of science suggests, then, two specific problems for Quine. First, even if the practice of science conforms to certain methodological canons, the practice does not explain why these canons (and not others) have "justificatory force". Second, in those cases where scientific insight is achieved by psychological processes peculiar to the specific scientist, knowledge of these processes (which is what epistemology naturalized provides) is of no interest or use with regard to the justification of the asserted insight. In short, "information relevant to the *generation* of a scientific idea is irrelevant to the evaluation of that idea" (Siegel 1980, p. 302).

Siegel's claim, then, is that the standards of evaluation are what he terms "genuinely independent" (Siegel 1980, pp. 301, 303) of psychological processes. Psychology studies the chain of associations peculiar to specific individuals; epistemology studies those structures recognized by the relevant community of reasoners to have "justificatory force". Siegel supports his epistemology-psychology distinction by the following argument:

> [P]sychological information can never by itself give us good reason for accepting some particular knowledge-claim as true, and since this is our criterion of adequacy for epistemologically justificatory claims, it follows that no psychological claim can be an epistemologically justificatory claim. Which is to say, psychology cannot be relevant to epistemology (Siegel 1980, p. 315).

Siegel's claim is, at heart, that the scientist is just not in a position to explain why his or her methods are correct, i.e., have justificatory force. We want to know why a knowledge claim is true; psychology only tells us how the claim came to be made and not why we have good reason to believe it to be true; therefore, psychology is not relevant to epistemology.

Epistemology naturalized is not, then, *epistemology*, for epistemology so construed cannot distinguish good reasons from the rest.

> Since an account of the mechanisms of knowledge-acquisition cannot aid in such assessment, an account of the mechanisms of knowledge-acquisition cannot, in principle, serve as well as an account of the justification of knowledge-claims. Quine's appeal to psychology can only help in accounting for the psychological mechanisms and processes of theory development—not in the rational evaluation of theory (Siegel 1980, p. 319).

The "in principle" cast to Siegel's claim—the claim that psychological and justificatory standards are independent—is defended, I assume, by the fact that it is patently circular to justify scientific methods by appeal to those methods. Quine's proposal would seem to imply *not* that the epistemologist seek justification within science, but that justification in Siegel's sense not be sought at all.

## II

Quine chides the "unliberated" epistemologist for his or her reluctance to use the resources of science; Quine sums up his own view on this matter by stating that "Epistemology, for me, is only science self-applied" (Davidson & Hintikka 1969, p. 293). Yet for epistemologists like Siegel, schooled in the traditional distinctions of philosophy of science, Quine seems oddly insensitive to the distinctively epistemological issues. Siegel's explanation is that Quine has just overlooked the distinction between understanding psychological processes and evaluating patterns of reasoning. But Quine is clearly aware that the traditional epistemologist is concerned with evaluating scientific reasoning. It would be quite odd for Quine to simply forget this point when it came to formulating his own proposal.

In order to understand the tack that Quine takes, it is helpful to first look at one of Quine's criticisms of Carnap's attempts to justify scientific reasoning by reconstructing the link between observations and theoretical entities. For reasons briefly canvassed by Siegel, Quine condemns the "fictitious history" which rational reconstruction sought to provide (Siegel 1980, p. 318). However, Carnap's project is, in Manley Thompson's phrase, "doubly fictitious" (Thompson 1981, p. 5). It is fictitious in the first instance (the one Siegel recognizes) because it does not provide the actual history of how science developed. But it is fictitious in a second sense as well, one which is critically important for an understanding of Quine's proposal. "It is also fictitious in the sense that it presupposes an account of analytic truth given in terms of a fictitious history of language development" (Thompson 1981, p. 5). This second sense of make-believe is important if we ask after the basis of the evaluative criteria which Siegel so sharply separates from science.

What Quine appreciates but Siegel does not, I claim, is that the criteria of evaluation relevant to science are, in fact, part of what needs to be justified. We can view scientific practice as a type of object language and then suggest that what the epistemologist does is to state the various rules, definitions, etc., to be applied in the object language. But what legitimates our choice of meta-linguistic rules? If any justificatory account is fictitious in the sense that it is just a speculative reconstruction of justificatory practices, then, I claim, Siegel does not establish the distinction he seeks between accepted practice and epistemology. *For far from being independent, epistemology recapitulates practices we deem efficacious for justification in other contexts.*

Specifically, Siegel notes at least two distinct senses of justification. First, a scientific claim may be said to be justified just in case its proof conforms to the accepted standards of the discipline. This he terms the "context of decision" (Siegel 1980, p. 311). However, such an account leaves unexplained the "justificatory force" of the reasons. "Still, once we have a thorough account of scientist $S$'s decision to adopt theory $T_1$ instead of $T_2$, we may well want to ask the further question: was $S$'s decision justified? . . . Factors accounting for the scientist's decision to adopt $T_1$ need offer no such justificatory account—though they may well settle the question of why the scientist in fact made the decision he/she did" (Siegel 1980, p. 310). The second sense of justification concerns what Siegel calls "epistemologically justificatory" claims (Siegel 1980, p. 314ff.). Justification in this sense "must pass the following test: it must purport to give us good reason for taking the knowledge claim to be true" (Siegel 1980, pp. 314–315). The specifics of this test are not as yet known. All Siegel wants to claim is that the canons which establish that good reasons have been given are irrelevant (indeed, he claims, logically independent) to the psychological factors (including in some cases conforming to accepted standards in a discipline) which explain why a person believes a claim to be true.

How will we know good reasons when we encounter them? Siegel suggests the following "shopping list" account of epistemological problems to be approached:

> [I]t is the philosopher's task to make sense of, on a 'meta-' level, the fundamental issues concerning inquiry—such as: is there a sense in which a given scientific theory accurately describes the world, and the 'hidden mechanisms' of the world, and if so, in what sense; if not, just how is such a theory to be interpreted; is there any reason to take such accurate descriptions of natural phenomena as *true* of

> the world; does this latter query even make sense; is there a general account of evidence to be given such that the evidence for a particular theory can be assessed according to that account; and so on—unencumbered by their sociological, psychological, and historical trappings (Siegel 1980, p. 307).

One might add that the account is also to be unencumbered by appeal to the standards of the best science one currently has, unless, of course, one has some *independent* reasons for thinking those standards relevant to epistemologically justificatory claims. For on Siegel's account, it is just such scientific standards which demand examination. They provide a context of decision, perhaps, but this is not part of epistemology as Siegel conceives it.

Now it might appear that the issue between Siegel and Quine can be sharply drawn at this point. For Quine insists that we cannot stand apart from our general scheme of the world in order to evaluate it. The denial that there are standards of evaluation which are genuinely independent of and better warranted than those current science endorses is what is meant when Quine claims that there is no "first philosophy".

But then how does Quine propose that we account for our methods of evaluating patterns of reasoning (both in science and without)? Quine's empirical psychologist *qua* epistemologist studies how human beings acquire the beliefs that they do. The study of these psychological processes will reveal, ideally, how most of us come to a belief in the enterprise. We come to understand, in other words, why certain beliefs command acceptance within the relevant community. And we settle for this genetic account of the context of decision because we simply cannot justify our evaluative criteria by appeal to some firmer, extra-scientific basis.[1] *Yet Siegel claims to agree with Quine that there is no such "first philosophy", no body of truths which are prior to and firmer than those which science has to offer* (Siegel 1980, p. 320). But then Siegel is mistaken to suggest that we can make sense of evaluative criteria independently of psychology, for there is no extra-scientific basis, by his own admission, for doing so. However, if this is the case, then Siegel, too, denies that there is a "science of justification".

But this might appear too quick. For even if there is no "first philosophy", one might argue, this does not establish that the evaluative criteria of epistemology could not be both independent of psychology and not part of psychology. An example which Siegel suggests of "epistemological argumentation" (Siegel 1980, p. 315) which purports to distinguish

[1] I examine the interplay between our use of science and the study of how we *acquire* science in Roth 1982.

epistemology from psychology is the following: "because we can give general arguments in favor of inductive inferences, and because we can make inductive inferences concerning certain perceptual claims, it therefore follows that establishing a person's belief in a claim, by making it subject to inductive inference, thus establishes that belief as evidence for the truth of that claim" (Siegel 1980, p. 316). That is, psychology tells us why person $X$ believes a claim to be true; epistemological argumentation establishes whether or not this belief is reliable.

Further, our judgments about reliability might be seen as independent of scientific considerations as well. For one might arrive at evaluative criteria in epistemology by generalization upon "intuitive judgments" about the merits of particular cases of scientific practice. Such criteria would not be better warranted than scientific criteria, but neither would such criteria be part of any natural scientific corpus since they are, *ex hypothesi,* based on non-scientific practice.[2]

However, this suggestion will not work. Siegel's claim, after all, is that any epistemologically justificatory claim must give good reasons, and these good reasons are logically independent of some practice which the community just happens to accept (whether or not the community in question here is a scientific one is not important). Generalizations upon intuitive judgments, whatever those might be, replicate the practice of making inductive generalizations in science. Since they are just another case of the practice for which justification is being sought, Siegel can hardly appeal to the non-scientific inductive generalizations for the purpose of providing "good reasons" for explaining why claims supported by this practice are true.

But Siegel's problem is worse than I have suggested so far. For, on the one hand, Siegel recognizes that if there is no first philosophy, then the epistemologist cannot decide which of two competing scientific claims is true. What the epistemologist can say is which, if any, claim is supported by good reasons. "An epistemologist, *qua* epistemologist, does not pass judgment on competing accounts of the structure of space; he/she does, however, study the general features of such scientific judgments, and tries to develop an account of the justification of such scientific judgments" (Siegel 1980, p. 320). Yet on the other hand, Siegel sometimes suggests that the epistemologist is to contribute to the decision about which claim is actually true. "For example, an account of how we come to acquire views about the structure of space, since it will account for how we come to acquire the view that space is Euclidean as well as the view that space is non-Euclidean—is a more justifiable construal of space. Such further distinction . . . requires our going beyond an account

[2]This argument was suggested by a referee for *Philosophy of Science*.

of the mechanisms of knowledge-acquisition" (Siegel 1980, p. 319). Siegel's suggestion is that it is by practicing epistemology rather than empirical science that we will decide between claims that space is Euclidean and that it is non-Euclidean. However, this contradicts his earlier noted assertion that epistemology cannot decide between such claims. Moreover, the suggestion that epistemology can contribute to the decision concerning which of the arguments is better in a way in which the scientist cannot just is first philosophy, i.e., ceding the epistemologist a vantage point from which to judge truth which the scientist (and everyone else) lacks.

Siegel wants to be a thoroughly modern philosopher of science, which seems to require taking a Quinean stance towards first philosophy. Yet Siegel also wants to fault Quine for not taking traditional epistemology seriously enough. But given the Quinean stance Siegel accepts, his complaint loses its point. For there then ceases to be justification of knowledge *except* in the first sense of "justification", i.e., the sense in which justification is identified with conformity to the canons of scientific inquiry, and so occurs in a recognized context of decision. If there is no justification, e.g., via rational reconstruction or via derivation from extra-scientific certainties, then there seems to be no "science of justification" which the epistemologist practices but the scientist does not.

It might still be urged, on Siegel's behalf, that actual debates in philosophy of science with respect to the appropriateness of certain methodological restraints are not themselves part of or a result of actual scientific inquiry.[3] That is, practicing scientists are conceived, in this view, as being concerned with substantive issues, e.g., testing specific hypotheses and formulating laws. If we identify science with, perhaps, a statement of accepted method plus such substantive inquiry, then methodological controversies appear to help shape scientific inquiry without being, in the Quinean sense, a part of it. Yet arguing for the appropriateness of certain norms of inquiry counts, surely, as epistemology. So a Quinean is faced with the following dilemma: either explain what the philosopher of science is doing as something other than epistemology or give up the claim that epistemology is just a "chapter" of scientific inquiry.

However, for the dilemma to stick, one must justify a view of natural science which excludes methodological disputes as part of the practice of science. Thus, on the view scouted above, disputes, e.g., between those holding different views about the standard by which to evaluate scientific progress or the appropriate sample size in clinical tests, are not engaged in disputes which are part of science. Selection of a model takes place

[3]I thank Teddy Seidenfeld for making clear to me this objection and its force.

in some nether world of inquiry which we are to call epistemology.

Certainly, it is not analytic that methodological disputes and concerns over the proper constraints on scientific procedure are *not* to be counted as part of science proper. Moreover, it is clear that, for Quine, science is not just the search for truth, e.g., substantive laws, but also includes debate with regard to how best to proceed about the business of formulating such laws. "For we can fully grant the truth of natural science and still raise the question, *within natural science,* how it is that man works up his command of that science from the limited impingements that are available to his sensory surfaces. This is a question of empirical psychology, but it may be pursued at one or more removes from the laboratory, one or another level of speculativity" (Quine 1974, p. 3, emphasis mine). This stance, in turn, is perceived by Quine as a direct consequence of rejecting first philosophy. (Quine also distinguishes, in Davidson and Hintikka 1969, p. 294, between the "methodological facet" of science and those special sciences which deal in substantive issues, e.g., biology or zoology. Smart, whose characterization of Quine's view is strongly endorsed by Quine, ibid. 292, reads Quine's position here as I do. See especially ibid. pp. 3–4).

In asserting that there is no distinction in kind between, on the one hand, competing hypotheses or models, and, on the other hand, competing methodological norms, am I foisting upon Quine's account of scientific method some unwanted and unnecessary normative component? If Quinean science is not objective in this respect, i.e., with regard to the determination of norms, does this not vitiate the ideal of an empirical and (relatively) value-free discipline?

This challenge, however, ignores Quine's claim that the only way to evaluate the norms of scientific inquiry is just by the practice of science. Given that Quine denies that there are extra-scientific constraints on scientific method, a scientist must modify the scientific ship while afloat in the sea of enquiry. "Our speculations about the world remain subject to norms and caveats, but these issue from science itself as we acquire it. . . . The norms can change somewhat as science progresses" (Quine 1981, p. 181). A parallel might be drawn here with the fact that there is no clear way to mark off what is "purely" observational from what is not; the parallel is that we cannot clearly mark off what is purely normative from what is not. Concerns regarding the justification of theoretical entities or norms of inquiry are subject to a general holistic constraint; we go with what seems required or what works best now, given current purposes.

Does this mean that it is possible that other norms of inquiry, radically different from (perhaps antithetical to) our own, might prove as efficacious in scientific practice? Would we, faced with the success of some alien science which proceeded by different principles, have to admit that their results were *justified?* "Yes, I think that we must admit this as a possibility in principle; that we must admit it even from the point of view of our own science. . . . I should be surprised to see this possibility realized, but I cannot picture a disproof" (Quine 1981, p. 181). What is relevant to the pursuit of scientific truth is whatever promotes this pursuit. As argued above, Siegel's proposed alternatives either beg the question (for they presume that we have some clear, non-scientific account of validation and justification, which is just what Siegel needs to prove) or they conflict with Siegel's own avowed rejection of first philosophy. Siegel offers us no way to advance the limits on epistemology imposed by a naturalized epistemology which are also consistent with other constraints he accepts.

A reason supporting Quine's position is the following: the methodological constraints are selected *not* by reference to what satisfies some *a priori* standard of justification or some extra-scientific ideal of inquiry, but by what promotes, as a matter of fact, the ends of science. By the "ends of science" I mean the goals (at least) of explaining the observed evidence and the making of successful predictions. Epistemology, understood for the moment as a concern with constraints on scientific method, is then part of the general scientific enterprise *since it shapes the constraints of sciences only in light of the acknowledged ends and purposes of natural science*. The touchstone of whether we have done epistemology correctly, in other words, is if the changes rung on scientific method allow us to better achieve scientific goals. *The traditional epistemologist looked for guidance—for standards of justification—elsewhere than in the practice of science. The Quinean epistemologist looks to science, even when seeking to improve it, in order to know what justifies changes in practice*. And just so long as we hold science (and epistemology) liable to no other standard that what smooths the way for ongoing research, we sustain the Quinean thesis that epistemology is best viewed as a part of natural science.

The foregoing discussion is, of course, *not* a justification of Quine's claims about epistemology naturalized. What I have argued is that Siegel's attempt to refute Quine is both unconvincing and inconsistent in its own characterization of epistemology. In my final remarks, I offer a sketch of the line of argument one actually does find in Quine and a suggestion regarding why confusions arise about Quine's project.

If we accept, following Quine, that analyticity and related notions do not illuminate the notion of logical truth, and if we accept Quine's claim that the sentences of a theory face the tribunal of experience together and not individually, then science self-applied yields the deepest understanding of our beliefs. For what better justification can we hope to have for our beliefs, jointly shared or individually held? There is no *evaluating* our theory from without, so we must try to account for it from within, using the resources it offers. In asking epistemological questions from within science, we attempt to explain our belief in the science of which that epistemological enterprise is a part. In this sense, epistemology contains all of science while being contained within it. Siegel is surely correct to note how different this is from what has heretofore counted as epistemology. What he misses is the force of Quine's reasons for reconceiving the task of epistemology. The force of the position lies in pointing out that in the absence of a first philosophy, natural science is our best means of evaluation.

Siegel's misunderstanding of Quine is based, I suggest, on a failure to distinguish Quine's *reasons for* advocating epistemology naturalized and the *consequences of* doing epistemology in this way. His confusion is understandable since Quine is not often clear when he is speaking as advocate of the project and when as practitioner.[4] Quine's *reasons for* advocating this project are those noted in the previous paragraph. A *consequence of* doing epistemology naturalized is to conflate the epistemological and psychological concerns which Siegel desires to hold separate.

The deeply radical flavor of Quine's proposal is just that it leaves unclear what parts of traditional philosophy of science and of traditional epistemology can be retained. Siegel's puzzlement is how to fit one part of the old tradition within Quine's scheme. Quine is not insensitive to such distinctions; the problem is whether the traditional concepts have any place in the epistemological enterprise as Quine conceives it. If epistemology cannot be the "science of justification", what is it? This reconception is understood by Quine as a consequence of two of his widely accepted positions—his critique on analyticity and his Duhemian view of theories (for these are his reasons for denying first philosophy). In order to locate the role of epistemology, one needs to examine the soundness of that argument. To attempt to measure the adequacy of Quine's proposal merely by whether or not it accords with traditional epistemology is to miss the point of the proposal.

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[4]I have detailed elsewhere Quine's insensitivity to this distinction in his writings and the confusion this causes in interpreting his epistemology. See Roth (1980).

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