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NATURALIZED EPISTEMOLOGY AND THE LAW OF EVIDENCE: REPLY TO REDMAYNE

Ronald J. Allen* & Brian Leiter**

2003 MICH. ST. L. REV. 885

In our article, *Naturalized Epistemology and the Law of Evidence*,¹ we sought to provide a brief but philosophically sophisticated introduction “to important recent developments in epistemology” and “to show . . . how these developments provide a conceptual foundation for some familiar approaches to problems from the law of evidence.”² Our motivations for doing so were complex, but included both highlighting and explaining for the legal academy the rejection of post-modern epistemology by most contemporary philosophers, and the implications of that rejection for the study of evidence. We had, thus, both general philosophical and highly practical goals in mind. Michael Redmayne appears to take us to task for the inadequacies of our philosophical presentation, and further suggests that the philosophy is of no import in any event.

With respect, we think that it is not our original presentation but Redmayne’s understanding of modern epistemology that is in error, and we think further that his error may be instructively elaborated and explained to advance our first goal of introducing certain philosophical concepts to the legal audience. Whether an improved understanding of the relevant philosophical concepts will in any fashion improve the field of evidence, or any other field, we leave to the reader’s imagination and future developments. Indeed, as we said in our original article, “[f]or the great bulk of evidentiary scholars . . . this paper merely solidifies the ground beneath their feet.”³ Nonetheless, we remain hopeful that the solidification is of some significance, even if only in assuring evidence scholars that the pursuit of truth is not some silly quixotic activity that only the uninformed would undertake with a straight face. Surprisingly, when one gets to Redmayne’s discussions of specific issues, the methodology he employs could be taken straight out of our

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1. See Ronald J. Allen & Brian Leiter, *Naturalized Epistemology and the Law of Evidence*, 87 VA. L. REV. 1491 (2001).

2. *Id.* at 1492.

3. *Id.* at 1493.

recommendations, although not for the reason he advances; a critical point that we return to below.⁴

We briefly elaborate these two points here. We first correct Redmayne's philosophical mistakes and then discuss his apparent methodological approach to evidence problems, showing it to be continuous with the lessons we tried to impart in our original paper. Before doing so, however, we wish to express our appreciation for his having spent considerable time on our original article. All of us agree that the issues under consideration are deserving of careful and sustained study, and that is precisely why we will use his critique as the vehicle for further correcting some possible misunderstandings about the relevant philosophical concepts and their implications.

Redmayne's argument gets misdirected right from the start. Although he professes to disown the "tendentious" method of "proceed[ing] by way of definitional fiat,"⁵ plainly his argument about the relevance of Alvin Goldman's program in naturalized epistemology to evidence scholarship amounts to nothing more than stipulative definitions backed by some selective citations of philosophers who (more-or-less) endorse his definitions.⁶ Redmayne talks about "the core concerns of epistemology,"⁷ "the central epistemological questions revolving round the definitions of knowledge,"⁸ "the sort of abstract and general analysis characteristic of philosophy,"⁹ and the "questions . . . traditionally addressed by epistemology;"¹⁰ but how he decided what was "core," "central", "characteristic" and "traditional," except by stipulation and citation, is left to the imagination. Redmayne's conclusion that "references to naturalized epistemology in Allen and Leiter's article are not doing any important work" is premised on just these unelaborated and "tendentious" claims.¹¹

Fortunately, this is clearest in Redmayne's central challenge that Goldman is not really "any sort of naturalist" since "[h]is naturalism only emerges when we move beyond the central questions of epistemology . . . to

4. See Mike Redmayne, *Rationality, Naturalism, and Evidence Law*, 2003 MICH. ST. L. REV. 849, 853.

5. *Id.* at 853.

6. See, e.g., ALVIN I. GOLDMAN, *EPISTEMOLOGY AND COGNITION* (1986); ALVIN I. GOLDMAN, *KNOWLEDGE IN A SOCIAL WORLD* (1999) [hereinafter GOLDMAN, *KNOWLEDGE*].

7. Redmayne, *supra* note 4, at 850.

8. *Id.* at 853.

9. *Id.* The naturalist challenge, of course, is precisely to whether such analysis can proceed except in tandem with empirical science.

10. *Id.* Redmayne may be closest to the mark on what is "traditional," but that invocation is obviously question-begging against the naturalist challenge to the epistemological tradition.

11. *Id.*

ask: What methods of inquiry are likely to result in our acquiring knowledge."¹² Redmayne thinks the "central" questions "revolv[e] round the definitions of knowledge" and not around "[h]ow we should go about acquiring knowledge."¹³ The main argument he offers bears quoting:

Most epistemology does not focus on inquiry. There may be a good reason for this, in that questions about inquiry are not obviously amenable to the sort of abstract and general analysis characteristic of philosophy. Now it would be tendentious to proceed by way of definitional fiat, asserting that questions about inquiry are not really epistemological at all. But it is important to understand that the Goldman/Allen/Leiter brand of naturalized epistemology is expansionist. Its claim to be naturalistic only comes into play where questions not traditionally addressed by epistemology are concerned.¹⁴

Redmayne's claims about epistemology are at best highly misleading and at worst simply wrong. Fundamentally, no epistemologists are interested in a "definition" of knowledge; none take themselves to be in competition with lexicographers. Philosophers are interested in an analysis of the concept of knowledge, not in the regulation of linguistic practice,¹⁵ and many are interested in inquiry. In a footnote, Redmayne acknowledges Richard Swinburne as an epistemologist interested in inquiry, as though he were the exception that proved the contrary rule.¹⁶ However, he neglects that epistemology since Descartes in the seventeenth century (who wrote on optics, physiology and geometry, among other "applied" subjects) through Carnap in the twentieth century has been concerned with vindicating the methods of inquiry characteristic of the modern physical sciences. So, to assert that "questions about inquiry are not really epistemological at all" is not only tendentious but flatly wrong.¹⁷ Epistemology is about inquiry, about the ways we acquire what we take to be "knowledge," about which ways are sound and reliable and which are not.¹⁸

12. *Id.* at 852-53.

13. Redmayne, *supra* note 4, at 853. Redmayne dismissively calls Allen and Leiter's process an "inquiry." *See id.*

14. *Id.*

15. There are reasons, to be sure, to think they fail. *See generally* Jonathan M. Weinberg et al., *Normativity and Epistemic Intuitions*, 29 PHIL. TOPICS 429 (2001). But this conclusion leaves lexicography untouched. *See id.*

16. *See* Redmayne, *supra* note 4, at 853 n.17.

17. *Id.* at 853.

18. Philip Kitcher describes as the "traditional" questions of epistemology the following: "What is knowledge? What kinds of knowledge (if any) are possible? What methods should we use for attaining knowledge, or, at least, for improving the epistemic qualities of our beliefs?" Philip Kitcher, *The Naturalists Return*, 101 PHIL. REV. 53, 56 (1992). Notice that what Redmayne calls "inquiry" is on Kitcher's list of the three main "traditional" questions of epistemology. *See id.*

Perhaps all Redmayne means is that, while most epistemology is deeply interested in the way we acquire knowledge (in “inquiry”), much modern epistemology has been concerned only to vindicate—to lay the foundation for—pre-existing methods of inquiry (e.g., those of the sciences), rather than to develop *new* methods of inquiry. Even this more muted point is difficult to support. The seminal—or at least, most famous—text of late twentieth century Anglo-American epistemology prior to the naturalistic revolution¹⁹ shows that even this reformulated claim is too strong. Roderick Chisholm was deeply interested in formulating the methods of inquiry for individual knowers, and thus would be banished from the epistemological canon according to Redmayne. But Chisholm and his many progeny to one side, it is true that there are other major figures in epistemology in the modern period who have had only tangential interests, if that, in the development of methods of inquiry.

What does this show? Plainly nothing of any philosophical, as opposed to “classificatory,” interest. For thousands of years, philosophers thought, for a variety of reasons, that the empirical facts did not bear on the question of what counts as knowledge or justification. Naturalists think the empirical facts matter, in one way or another. Why Redmayne wants to deny that this is (a) naturalism, or (b) relevant to the evaluation of evidentiary rules, is, in the end, utterly mysterious. Evidence scholarship would be better served by asking the questions that we argued deserve asking than engaging in disputations about terminology. In doing so, it would be in harmony with much of modern epistemological thought.

Are we now being “tendentious”? We think not. In the centenary issue of the *Philosophical Review*, the premier journal of Anglophone philosophy, Philip Kitcher, contributed a now-classic essay on the naturalistic revolution in philosophy, especially epistemology.²⁰ As Kitcher noted, Frege is the arch anti-naturalist in philosophy precisely because of his “opposition to what he perceived as intrusions from psychology or biology.”²¹ Kitcher also notes the “several kinds of naturalism” that have overtaken contemporary philosophy “all share an opposition to the Frege-Wittgenstein conception of a pure philosophy above (or below?) the special disciplines,” i.e., the sciences.²² Indeed, the two distinguishing features of the anti-naturalist approach, according to Kitcher, are the pursuit of “epistemological questions in an apsychologistic way—logic, not psychology, is the proper idiom for epistemological discussion” and the view of “philosophical reflection as a

19. See RODERICK M. CHISHOLM, *THEORY OF KNOWLEDGE* (1966).

20. See Kitcher, *supra* note 18, at 53.

21. *Id.* at 54.

22. *Id.* at 55.

priori,” i.e., prior to experience or empirical evidence.²³ Naturalists like Goldman repudiate both claims, and thus are usefully, and quite intelligibly, marked as naturalists—and that, in a nutshell, is why (as we argued) naturalized epistemology is relevant to evidence scholarship.

Yet, Redmayne also has “another reason for thinking that the references to naturalized epistemology . . . are not doing any important work.”²⁴ Namely, that Goldman’s social epistemology—which Redmayne appears to concede is quite relevant²⁵—is not really part of naturalized epistemology. Once again, we enter the realm of pointless terminological quibbles. Would we be able to avoid this dispute simply by retitling our essay?²⁶

It is of course true that Goldman’s program in *Knowledge in a Social World* is concerned only with what Goldman himself calls the “weak” sense of knowledge, that is, mere true belief.²⁷ (Redmayne fails to note this qualification—“weak”—which is introduced by Goldman.) Yet even Goldman takes this to be a radical departure from “the Cartesian image of inquiry as an activity of isolated thinkers,” since it emphasizes instead the need to attend to “the interpersonal and institutional contexts in which most knowledge endeavors are actually undertaken.”²⁸ Goldman is also equally clear that “most of the philosophical literature” is concerned with knowledge in a different sense: namely, as true belief plus some other condition (having to do with justification).²⁹ Goldman, of course, thinks the other condition must be cashed out in terms of empirical facts about the causal mechanisms that reliably produce true belief. That means, quite obviously one would have thought, that the final program of social epistemology—which would necessarily have to attend to the social mechanisms by which belief is (causally) inculcated—would be naturalistic precisely because it makes the existence of “knowledge” turn on *a posteriori* facts.

Since Redmayne apparently admits the relevance of social epistemology, and has merely “tendentious” definitional reasons for excluding social epistemology from the domain of naturalized epistemology, it appears that he

23. *Id.* at 57.

24. Redmayne, *supra* note 4, at 853.

25. See generally GOLDMAN, KNOWLEDGE, *supra* note 6 (discussing the laws of evidence in chapter four and seven).

26. An earlier version of the paper was in fact called *Social Epistemology and the Law of Evidence*, though it is hard to see why a change in title should occasion a substantive criticism.

27. See GOLDMAN, KNOWLEDGE, *supra* note 6, at 24.

28. *Id.* at vii.

29. *Id.* at 23.

has failed to show that naturalized epistemology is “not doing any important work”³⁰ in our approach to evidence law.³¹

Now Redmayne is also unsympathetic to Quine’s radical replacement naturalism—he is not alone in this, obviously—and takes issue with Leiter’s sympathy for Quine in other writings of his. Since the radical Quinean program isn’t at issue in Allen & Leiter’s joint work, we will confine discussion of this aspect of his paper to a (lengthy) footnote.³²

30. Redmayne, *supra* note 4, at 853.

31. One other clarification needs to be given. Our claim that “the only way to assess instrumental claims is to do so empirically, that is, by finding out what means *really* bring about what ends” is not, contra Redmayne, a kind of replacement naturalism that “is not easy to reconcile with Goldman’s work on social epistemology.” Allen & Leiter, *supra* note 1, at 1499; see Redmayne, *supra* note 4, at 860. Goldman’s entire project in *Knowledge in a Social World* is an exercise in ought-implies-can. See GOLDMAN, KNOWLEDGE, *supra* note 6. This is not replacement naturalism, since it does not involve replacing the normative question about the relationship between sensory inputs and theoretical outputs with a descriptive question about the causal connection between these two.

32. Redmayne observes that Quine argues for replacement naturalism based on the failure of foundationalism. See Redmayne, *supra* note 4, at 855-56. However, Redmayne complains this “is only one of several possible theories of epistemic justification.” *Id.* at 855. Redmayne mentions, e.g., coherentism (citing BonJour *circa* 1985), reliabilism (Goldman), and founderherentism (Haack), among others. See *id.* at 855 nn. 28, 30 & 31. Among “possible” theories, of course, this list seems rather truncated. See *id.* It omits: divine inspirationalism (all genuine knowledge derives from divine inspiration), Leiterian inspirationalism (all genuine knowledge derives from inspiration from Leiter), lunatic veritism (all genuine knowledge consists only of true beliefs, however acquired), and Potterian coherentism (all genuine knowledge coheres with claims in the *Harry Potter* novels). Now, while these views are possible, they are also silly, but their possibility does underline the fact that what motivates Quine’s move from the failure of foundationalism to replacement naturalism is precisely the unpromising nature of the “possible” alternatives to foundationalism. BonJour, for example, recognizing this, abandoned coherentism long ago (this goes unnoted by Redmayne), and Haack’s idiosyncratic view has the vices of both the coherentism (that BonJour abandoned) and the foundationalist tradition attacked by Quine. Redmayne has not even attempted to show that these views are serious competitors, and the mere fact that their authors are well-known does not make them such. (Goldman’s reliabilism, precisely because it parts company with the internalism about justification characteristic of the foundationalist tradition that Quine attacks, has better prospects, but would presumably fall prey, in Quinenan eyes, to skepticism about conceptual analysis.)

Redmayne also takes issue with Leiter’s claim that the Neurath’s boat metaphor for our epistemological situation—the idea that in epistemological matters, we are in the same situation as sailors who want to rebuild their ship while still at sea—supports the claim that, in the absence of foundationalism, epistemology can do no more than describe our actual epistemological practices. See *id.* at 856-57. Redmayne’s rejoinder to Leiter bears quoting:

The notion of changing planks surely suggests reflection and attempts to critically re-order our theories. What the boat metaphor does suggest is that we have no infallible foundations—any one of the planks may need to be replaced at some stage.

Once the philosophical concepts are straightened out, it becomes plain that much of current evidentiary scholarship is quite compatible with modern naturalized epistemology. Moreover, evidence scholars should not be made uneasy that their pursuit of propositions with truth value is somehow quaint and out of step with modern relativistic thinking that consigns questions of truth to either the trash bin or pure politics. Not surprisingly, when Redmayne, who is a distinguished evidentiary scholar in his own right, actually analyzes specific legal questions (inferences from silence and the use of DNA evidence), he does so in ways perfectly countenanced by our approach. He carefully analyzes the relevant problem, searches for data, considers its reliability and relevance, and so on.

What, then, is the problem? Is there nothing here but terminological disputes with no practical significance? We think not. Indeed, we think Redmayne's paper is a perfect example of the possible significance of our original effort to solidify the ground beneath the feet of evidence scholars. Having convinced himself that our explication of naturalized epistemology was either wrong or irrelevant to evidence scholarship, or both, Redmayne

Nevertheless, there is a foundationalist point here, in that in our theorizing at a particular point in time we are assuming that certain planks provide a privileged epistemological position; fallible though they may be, we will not give them up while we are standing on them.

Id. at 857. Unfortunately, Redmayne has here conflated two distinct perspectives: the perspective of the epistemologist who is wondering what can be said about truth justification; and the perspective of those in the boat (e.g., working scientists) who are investigating what is true and what is justified. The whole point of the Neurath's boat metaphor—as is abundantly clear in both Quine and Leiter—is that those in the boat, of course, privilege certain theoretical positions and treat them as inferentially foundational in constructing their theories of the true and justified. The claim that there is nothing left but descriptive sociology if (what Redmayne calls infallible) foundationalism fails is a point about the perspective of the epistemologist. To be sure, the epistemologist can describe the criteria of truth and justification the people on the boat use, but that is just descriptive sociology, a descriptive sociology that confers no meta-sanction on those criteria. As Redmayne puts it (not realizing, apparently, that this concedes the point), “at a particular point in time we are assuming that certain planks provide a privileged epistemological position.” Redmayne, *supra* note 4, at 857. Indeed we are, and there is nothing in the Quinean program to preclude us from describing what planks those are at any particular point in time. The same applies to Redmayne's point that, following Quine's “engineering” approach to questions of normativity, we might try “to develop more general and abstract epistemic principles . . . which would pick out common elements in the examples” of fruitful epistemic principles that Redmayne describes. *Id.* at 859.

Now Redmayne is correct that predictive success is assigned a great deal of weight by Quine, and we might fairly ask, as Redmayne does, “[w]hy should we be impressed by prediction?” *Id.* at 857. Quine hasn't a clear answer to that question, but Leiter attempts to supply one, in a Quinean spirit, in an article to which we refer the interested reader. See Brian Leiter, *Why Quine Is Not a Postmodernist*, 50 SMU L. REV. 1739, 1750 (1997).

turned to what philosophical perspectives might better serve our interests, and he found . . . nothing. He briefly flirts with probabilism, although it is hard to see “probabilism” as a philosophical perspective (a task not simplified by his failure to be clear about what he means by the term). It is, as we understand his use of the term, a tool that all rational individuals will employ from time to time, but even probabilism is a disappointment as “we have reason to suspect that sometimes these criteria should be rejected.”³³ What is to be done? “It seems that the only good way to manage the problem is, broadly speaking, to draw on our intuitions.”³⁴

We do not believe Redmayne really believes this; but if he does, we would recommend yet another reading of our original article. The naturalistic turn in epistemology came about in no small measure because of the failure of formal theories (elaborated intuitions) to achieve the goal of justifying assertions of knowledge. Justification, in other words, seemed not to be a matter of just consulting one’s intuitions, and the effort to justify assertions of knowledge in that manner came to be perceived as the problem rather than the solution. In order to gain knowledge, one needs not just to think but to investigate. The two go hand in hand, as we tried to elaborate in our original article, and ironically as Redmayne’s own discussions of practical issues demonstrate as well. He is not relying on his “intuitions” in his discussions of silence and DNA; he largely is considering the evidence and its implications. He is, in short, working well within the modern tradition of naturalized epistemology.

33. Redmayne, *supra* note 4, at 867.

34. *Id.*