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The Epistemology of Admissibility: Why Even Good Philosophy of Science Would Not Make for Good Philosophy of Evidence

*Brian Leiter**

I. INTRODUCTION

In its 1923 decision in *Frye v. United States*,[1] the United States Court of Appeals for the District of Columbia set out what was, for seventy years, the most influential test for the admissibility of scientific evidence in federal court. In *Frye*, the question was whether the results of a lie detector test were admissible on behalf of the defense. The Court of Appeals agreed with the trial court that such evidence was inadmissible, famously holding, that scientific evidence "must be sufficiently established to have gained general acceptance in the particular field in which it belongs."[2] In 1993, the United States Supreme Court ended *Frye*'s reign of influence with its decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*[3] Holding that Federal Rule of Evidence 702,[4] governing the admissibility of scientific evidence, did *not* codify *Frye*'s "general acceptance" test,[5] the Court went on to say that the key question was whether any proffered piece of evidence constituted "scientific knowledge" within the meaning of the Rule.[6] The Court then enumerated a nonexclusive list of

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1. 293 F. 1013 (D.C. Cir. 1923).
2. *Id.* at 1014.
3. 509 U.S. 579 (1993).
4. Federal Rule of Evidence 702 states: "If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise."
5. *See Daubert*, 509 U.S. at 588.
6. *See id.* at 589-90.

factors for courts to consider in assessing whether proffered evidence constitutes "scientific knowledge": for example, whether the theory on which the evidence is based is "falsifiable" (in Karl Popper's sense);[7] whether "the theory or technique has been subjected to peer review"; and whether the theory enjoys "general acceptance."[8] Since *Daubert*, trial judges must now weigh a complex set of philosophical and methodological factors in deciding upon the admissibility of proffered scientific evidence, rather than falling back upon the simple proxy of "general acceptance."

The Supreme Court's repudiation of *Frye* as the exclusive test for admissibility of scientific evidence has already generated substantial scholarly literature.[9] From a philosophical standpoint, the most interesting recent discussion has been Heidi Feldman's argument that *Daubert*'s rejection of *Frye* and its broadening of the criteria for admissibility serve to bring the Federal Rules of Evidence more in line with what Feldman dubs "revised empiricist" philosophy of science, and thus with actual scientific practice.[10] Feldman commends this move toward a "more scientific approach to admissibility,"[11] especially because its consequence will be, according to Feldman, a greater recognition of scientific uncertainty, especially in the area of mass tort litigation.[12]

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7. *See id.* at 593 (citing KARL POPPER, CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE 37 (5th ed. 1989)).

8. *Id.* at 593-94.

9. *See infra* notes 52, 58 for relevant articles.

10. *See* Heidi Li Feldman, *Science and Uncertainty in Mass Exposure Litigation*, 74 TEX. L. REV. 1 (1995).

11. *Id.* at 1-2. At times, Feldman purports to remain neutral on the question of whether *Daubert* sets a good standard. *See id.* But only a page later she calls *Daubert* "a sensible approach to the admissibility of scientific evidence." *Id.* at 3. She later criticizes *Frye* for setting standards that do not accurately represent scientific practice. *See id.* at 6-7. The whole tenor of the piece, in short, is that *Daubert* represents a good development in the law of evidence, even if this will not reduce "uncertainty" in mass exposure litigation.

12. I am not concerned in this Article with Feldman's other main thesis (which occupies the second half of her paper): namely, that making standards of admissibility more "scientific" will not, as "conservatives" believe, reduce uncertainty, and hence, reduce litigation. *See id.* at 18-43. To the contrary, "science is severely uncertain about the causal effects of the substances and products that figure so prominently in contemporary tort litigation." *Id.* at 2-3. I do note in passing that the argument of this half of Feldman's paper strikes me as being either trivial (uncertainty about causation "strikes at the heart of the tort system," *id.* at 34) or unsupported ("one example," Feldman concedes, "does not establish the contention" that scientific standards of admissibility will yield uncertainty in most mass exposure cases, *id.* at 33, but no other evidence is adduced; indeed, one colleague involved in breast implant litigation tells me

In this Article, I take issue with Professor Feldman's argument. For even if "revised empiricist" philosophy of science *were* the dominant, or even the correct, philosophy of science, there would be no reason to think admissibility standards ought to conform to it: Feldman seems to confuse the philosopher's question, "What is the best account of scientific method?" with the lawyer's question, "What is the best criterion for judges to use in deciding the admissibility of scientific evidence?" Yet the philosopher can provide the lawyer with good *epistemological* reasons for keeping these questions separate—reasons that Feldman ignores or downplays. Philosophy of science and the rules of evidence are both concerned with "epistemic norms": norms for how scientists should form beliefs in the former case; norms for how juries should form beliefs about disputed matters of fact in the latter case. A central theme of contemporary "naturalized" epistemology has been the need to tailor our normative advice about belief formation to the realities of how epistemic processes—processes for the acquisition of knowledge—work, whether they involve individual mechanisms (e.g. perception) or social mechanisms (e.g. trials).[13] In particular, norms for belief-formation must be sensitive to the *epistemic limits* of would-be knowers: that is, the handicaps—intellectual, cognitive, temporal, material—that all real knowers operate under. If scientific knowers differ, as of course they do, from the "knowers" that comprise juries and that sit on the bench, then it should be surprising, from the perspective of naturalized epistemology, that

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that the scientific evidence is rather clear that implants do not cause health problems). The more interesting, and important, contribution of Feldman's paper, as I read it, is contained in the first half on *Daubert* and philosophy of science. In correspondence, Feldman suggests that she does not share this understanding of the relative importance of the different sections of her paper.

13. This view of epistemology, which now dominates philosophical research in the area, owes most to the work of Alvin Goldman. *See, e.g.*, ALVIN I. GOLDMAN, EPISTEMOLOGY AND COGNITION (1986) [hereinafter GOLDMAN, EPISTEMOLOGY]; Alvin I. Goldman, *Foundations of Social Epistemics*, 73 SYNTHESE 109 (1987). For an application of similar themes to the epistemology of science, see Larry Laudan, *Normative Naturalism*, 57 PHIL. SCI. 44 (1990). For a general survey of the influence of the "naturalistic" approach, see Philip Kitcher, *The Naturalists Return*, 101 PHIL. REV. 53 (1992). The Goldmanesque branch of naturalized epistemology (which retains the normative ambitions of traditional epistemology) must be distinguished from the Quinean branch, which makes "epistemology" a purely descriptive science of human cognition. I try to sort out these strands of naturalism in my *Naturalism and Naturalized Jurisprudence*, *in* ANALYZING LAW: NEW ESSAYS IN LEGAL THEORY (Brian Bix ed., forthcoming 1998).

the same norms for belief-formation should apply to both groups. Yet it is precisely this conclusion that Feldman appears to draw.[14]

II. PHILOSOPHY OF SCIENCE AND PHILOSOPHY OF EVIDENCE

Feldman, drawing on an account by the philosopher of mind Patricia Smith Churchland,[15] presents with admirable clarity one episode in the evolution of twentieth-century philosophy of science: the movement from logical empiricism to what Feldman calls "revised empiricism."[16] I shall just recap the bold outlines of the story.[17] Logical empiricists held that what distinguishes science is its commitment to *testability*, to seeing whether scientific claims are borne out by our observations. Karl Popper[18] suggested that the hallmark of science was not simply "testability," but more precisely *falsifiability*: that is, the possibility that the theory can be shown to be inconsistent with our experience.[19] In

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14. I should note that throughout the article, Feldman is silent on a crucial comparative question. Assuming that we want juries to take greater cognizance of scientific uncertainty (but as Feldman correctly notes, we do not always want this, *see* Feldman, *supra* note 10, at 42-43), the only reason to prefer *Daubert* is if it is more likely to permit evidence of genuine scientific uncertainty than would *Frye*. (Strictly speaking, we want to know how each set of standards strikes the balance between: admitting evidence of genuine scientific uncertainty versus admitting evidence of dispute between genuine science and "junk" science.) Yet Feldman adduces no evidence on this comparative question: for all we know, *Frye* may reveal *genuine* scientific uncertainty as often as *Daubert*. Indeed, it is striking that in Feldman's own lengthy and interesting hypothetical case, concerning litigation over breast implants, *see id.* at 18-33, the *Frye* test would almost certainly permit admission of the conflicting scientific opinion, since as Feldman repeatedly notes, it all satisfies the "general acceptance" prong of *Daubert*—which is, of course, taken over from *Frye*. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 594 (1993).

Indeed, there is an even stronger reason to think that *Daubert* will be *more* restrictive, rather than less restrictive, than *Frye*: for *Frye* applied only to *novel* scientific evidence, whereas *Daubert* reaches *all* scientific evidence. This means that there will be many cases under a *Daubert* regime in which the trial judge will be empowered to assess the admissibility of scientific evidence that under the *Frye* regime would have raised no special evidentiary issues.

15. *See* PATRICIA SMITH CHURCHLAND, NEUROPHILOSOPHY: TOWARD A UNIFIED SCIENCE OF THE MIND-BRAIN 252-75 (1986).

16. *See* Feldman, *supra* note 10, at 10-48.

17. This is my own version, though it has basic affinities with Feldman's.

18. As Feldman rightly notes, on standard accounts of "logical empiricism," Popper is not a member of that school, though he has certain affinities. *See* Feldman, *supra* note 10, at 10 n.47. For an accessible and more detailed discussion, see Sean O'Connor, *The Supreme Court's Philosophy of Science: Will the Real Karl Popper Please Stand Up?*, 35 JURIMETRICS J. 263 (1995).

19. *See Daubert*, 509 U.S. at 593 (citing KARL POPPER, CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE 37 (5th ed. 1989).

the 1950's and 1960's, this traditional empiricist account of scientific method ran into serious trouble over two main issues.[20] First, "theoretical" statements (which are to be tested) cannot be simply demarcated from "observation" statements (the ones against which we test theory). Observation, various philosophers argued, is itself "theory-laden."[21] It appears, then, that theories are not tested against the world, but rather against other (implicit) theories about the world. Second, the problem of "auxiliary hypotheses" renders all testing (and especially falsification) problematic.[22] Recall, for example, the Biblical story of King Solomon,[23] in which Solomon must decide which of two women is the real mother of a particular child.[24] Suppose Solomon hypothesizes that woman A is the *real* mother of the child, while woman B is not. Solomon tests the hypothesis by proposing that each woman get half of the child. If his hypothesis is correct, then he predicts that A will decline to "split" the child, and will let woman B keep the whole child. But notice that this prediction depends on an *auxiliary hypothesis* that the Biblical story never mentions: namely, that a *real* mother's concern for the well-being of her child is always stronger than her jealousy that another should have *her* child. Now suppose that, contrary to the prediction, A is eager to split the child, rather than let B just have the child. Logically, this is compatible with the hypothesis that A is the real mother, *if we reject the auxiliary hypothesis*. If we remain committed, however, to the auxiliary hypothesis, then experience falsifies the original hypothesis. But notice that *experi-*

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20. For a more adequate discussion of the relevant developments, see Frederick Suppe, *The Search for Philosophic Understanding of Scientific Theories, in* THE STRUCTURE OF SCIENTIFIC THEORIES 3 (Frederick Suppe ed., 2d ed. 1977).

21. The classic sources on this point include: NORWOOD RUSSELL HANSON, PATTERNS OF DISCOVERY (1958); the papers collected in 1 PAUL K. FEYERABEND, REALISM, RATIONALISM AND SCIENTIFIC METHOD (1981); and THOMAS KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (2d ed. 1970). For related and also influential critiques of the theory/observation distinction that was central to positivist philosophy of science, see Grover Maxwell, *The Ontological Status of Theoretical Entities, in* 3 SCIENTIFIC EXPLANATION, SPACE AND TIME 3 (Herbert Feigl & Grover Maxwell eds., 1962); Hilary Putnam, *What Theories Are Not, in* LOGIC, METHODOLOGY, AND PHILOSOPHY OF SCIENCE 240 (Ernest Nagel et al. eds., 1962); and Peter Achinstein, *The Problem of Theoretical Terms*, 2 AM. PHIL. Q. 193 (1965).

22. *See* PIERRE DUHEM, THE AIM AND STRUCTURE OF PHYSICAL THEORY (Philip P. Wiener trans., Princeton Univ. Press 1954) (1914); WILLARD VAN ORMAN QUINE, WORD AND OBJECT (1960).

23. *See* 1 *Kings* 3:16-28.

24. I owe the idea for this charming example to Gila Sher.

ence itself appears to do no work in determining which hypothesis we discard. This seems to undermine the empiricist commitment to testability.

The "revised empiricists," on Feldman's recounting, acknowledge both problems (though Feldman does not frame them quite this way) and seek a solution in the recognition that science "is a collective process comprised of institutionalized educational and scholarly practices that shape and check individual judgment."[25] In other words, that "theory" is demarcated from "observation," or that some auxiliary hypotheses are held in place, while testing other hypotheses, are conceded to be matters that are fixed at any particular time by the practices of the scientific community. Yet science still "progresses" on this account. As Feldman puts it (following Kuhn):

> [S]cience progresses as scientists trade in one theory for another, as they collectively come to recognize that a rival to the established theory better satisfies the various scientific desiderata—predictive power, simplicity, unity of theory, fruitfulness, and so on. . . . The impetus for change arises from shortcomings in the settled view. As scientists work with a theory, they find that there are phenomena it either cannot explain or can explain only by adding ad hoc premises and assumptions. . . . As scientists become disenchanted with the resources of prevailing theory, they consider alternatives more carefully.[26]

Thus, "revised empiricism," unlike logical empiricism, assigns proper weight to the role of social factors in the constitution of scientific knowledge: "[S]cientists' collective judgments—facilitated and established through devices such as peer review and publication and measured by general acceptance—are as distinctively characteristic of science as testability itself."[27] This supports Feldman's ingenious rationalization and defense of the Court's inclusion of "peer review" as a factor in considering admissibility in *Daubert*.[28] Although the Court did not (as Feldman notes) have anything like this rationale in mind, Feldman's argument supplies a clever post-hoc justification of the test the Court set out.

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25. Feldman, *supra* note 10, at 15.

26. *Id.* (footnotes omitted). It remains unclear, in this account, why this is "progress" along any epistemic dimension. *See infra* note 39 and accompanying text.

27. Feldman, *supra* note 10, at 10.

28. *See id.* at 16.

A. *A Short Detour in the Philosophy of Science: Empiricism or Realism?*

Historically and philosophically, there are worries about certain aspects of this picture. Feldman claims, for example, that "revised empiricism . . . dominates late-twentieth-century philosophy of science."[29] By "empiricism," Feldman plainly does *not* mean the technical doctrine in philosophy of science that it is reasonable *not* to accept as real the unobservable entities posited by scientific theories; for such a view may have been the majority view among Logical Empiricists in the middle of this century, but it is plainly a minority view today.[30] Feldman means "empiricism" in a much broader, and less philosophically contentious sense: namely, as a commitment to the testability of theories against experience. In this sense, of course, almost everyone in philosophy of science for the past several hundred years has been an "empiricist."

The key question, then, is what is meant by "revised" empiricism. Feldman is explicit that the views that purportedly "dominate" contemporary philosophy of science are those of the "contemporary theorists—including Thomas Kuhn, Imre Lakatos, and Helen Longino [who] maintain commitments to empiricism and to the idea that science is a distinctive human enterprise."[31] This is odd on several scores, not the least of which is that Kuhn and Lakatos are hardly contemporaries.[32] For one thing, this

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29. *Id.* at 10.

30. The leading proponent is BAS C. VAN FRAASSEN, THE SCIENTIFIC IMAGE (1980). *See also*, Lawrence Sklar, *Foundational Physics and Empiricist Critique*, *in* 14 SCIENTIFIC THEORIES 136 (C. Wade Savage ed., 1990).

31. Feldman, *supra* note 10, at 13.

32. Kuhn recently died and, in any case, made his last significant contributions to the field more than two decades ago; Lakatos died in the 1970s, and his major work, of course, predates Kuhn's work. Longino, by contrast, is a contemporary, though her views could hardly be described as "dominating" contemporary philosophy of science. Indeed, one would have thought the most powerful account reconciling the "social" forces at play in science with the essential objectivity of scientific progress is that found in the work of Philip Kitcher. *See, e.g.*, PHILIP KITCHER, THE ADVANCEMENT OF SCIENCE (1993) [hereinafter KITCHER, ADVANCEMENT OF SCIENCE]; Philip Kitcher, *The Division of Cognitive Labor*, 87 J. PHIL. 5 (1990). Kitcher, however, takes himself quite explicitly to be moving beyond the Kuhnian tradition—in part by embracing the scientific realism, discussed *infra* note 37. (He also construes Longino, as I think most philosophers of science do, as presenting a more debunking account of the epistemic pretensions of science, whatever her stated ambitions. *See* KITCHER, ADVANCEMENT OF SCIENCE, *supra*, at 303 n.1.)

way of putting it reinforces the unfortunate impression—apparently widespread in the humanities,[33] and certainly in law schools[34]—that Kuhn, in particular, marks the last important development in post-positivist philosophy of science. While Feldman correctly emphasizes the later Kuhn's attempt to distance his views from the more explicitly debunking views of science in writers like Feyerabend,[35] it is still misleading to suggest that Kuhn's views *circa* 1970 dominate contemporary philosophy of science. Indeed, the major event in post-Kuhnian and post-Quinean[36] philosophy of science—an event nowhere mentioned in Feldman's discussion—has been the turn away from the technical doctrine of "empiricism," noted above, in favor of "realism": the view that scientific theories are literally true (i.e. the unobservable entities posited by such theories really exist). Only on a realist interpretation of science, many contemporary philosophers have thought, is it possible to understand the sense in which science makes objective progress.[37] Indeed, many contemporaries have made the "realistic" turn precisely because they doubt that views like the later Kuhn's will suffice to account for objective progress in science.[38]

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33. *Cf.* LARRY LAUDAN, SCIENCE AND RELATIVISM vii-xi (1990) (lamenting tendency to view the relativism of Kuhn and Feyerabend as the dominant views in philosophy of science).

34. *See* Brian Leiter, *Intellectual Voyeurism in Legal Scholarship*, 4 YALE J.L. & HUMAN. 79, 93-95 (1992) (noting tendency of law professors to view philosophy of science as having ended with Kuhn and Feyerabend in the 1960s).

35. *See* PAUL FEYERABEND, AGAINST METHOD (Verso 1988) (1975).

36. Quine's attack on the distinction between "true in virtue of meaning" versus "true in virtue of fact" marks one of the other major watersheds in the collapse of the positivist program in philosophy of science. *See* Brian Leiter, *Why Quine is Not a Postmodernist*, 50 SMU L. REV. 1739, 1746-1747 (1997).

37. This picture, in one form or another, informs the work of genuine contemporaries like Richard Boyd, Clark Glymour, Richard Miller, Paul Churchland, Philip Kitcher, and Peter Railton, among others. *See, e.g.*, PAUL M. CHURCHLAND, SCIENTIFIC REALISM AND THE PLASTICITY OF MIND (1979); KITCHER, ADVANCEMENT OF SCIENCE, *supra* note 32; RICHARD W. MILLER, FACT AND METHOD (1987); Richard N. Boyd, *Constructivism, Realism, and Scientific Method*, *in* INFERENCE, EXPLANATION, AND OTHER FRUSTRATIONS: ESSAYS IN THE PHILOSOPHY OF SCIENCE 131 (John Earman ed., 1992) [hereinafter Boyd, *Constructivism*]; Richard N. Boyd, *Realism, Underdetermination and a Causal Theory of Evidence*, 7 NOUS 1 (1973); Clark Glymour, *Explanation and Realism*, *in* SCIENTIFIC REALISM 173 (Jarrett Leplin ed., 1984); Peter Railton, *Explanation and Metaphysical Controversy*, *in* 13 SCIENTIFIC EXPLANATION 220 (Philip Kitcher & Wesley Salmon eds., 1989).

38. In fact, another contemporary movement in philosophy of science draws precisely this conclusion from the Kuhnian critique of logical empiricism. On this view—associated with writers like David Bloor, Steven Shapin, Paul Feyerabend, Bruno Latour and others—science is not about the objective growth of knowledge, but rather

Why, after all, should *our* "institutionalized process" of inquiry and *our* epistemic "desiderata" be reliable guides to the truth? "Revised empiricism" runs the risk of collapsing the epistemic pretensions of science into a mere sociological artifact; only by accepting theories as literally true, philosophers have recently argued, can we defend the claim that science is epistemically special.[39] Contemporary philosophers of science do

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a sociological artifact, whose results are to be explained purely in sociological terms, rather than *epistemic* ones. *See, e.g.*, BARRY BARNES, SCIENTIFIC KNOWLEDGE AND SOCIOLOGICAL THEORY (1974); DAVID BLOOR, KNOWLEDGE AND SOCIAL IMAGERY (1974); FEYERABEND, *supra* note 35; BRUNO LATOUR, SCIENCE IN ACTION (1987); STEVEN SHAPIN, A SOCIAL HISTORY OF TRUTH (1994); Steven Shapin, *History of Science and Its Sociological Reconstructions*, 20 HIST. SCI. 485 (1982). Science, on this view, is not epistemically special, it is just sociologically special. Although Kuhn himself tried to resist this conclusion, many philosophers have taken Kuhn's (and Quine's) view to entail precisely this "relativistic" result. *See* LAUDAN, *supra* note 33; Kitcher, *supra* note 13, at 96. It is just these worries that have pushed many contemporaries away from the *empiricism*, and toward scientific realism. *See, e.g.*, KITCHER, ADVANCEMENT OF SCIENCE, *supra* note 32; Glymour, *supra* note 37.

39. Several arguments have been influential here: what we might call arguments from explanatory success, confirmation, and understanding. The argument from explanatory success: only a realist interpretation of scientific theories can adequately explain the remarkable success of methodologies that are, admittedly, theory-laden in the ways Kuhn, Hanson et al. pointed out. *See, e.g.*, Boyd, *Constructivism, supra* note 37. The argument from confirmation: if we accept scientific theories as more than just "empirically adequate" (i.e. doing justice to the observable consequences), if we accept them, in particular, as true descriptions of reality (including the unobservable parts of reality), then such theories can be both better confirmed than their merely observational counterparts *and* maximize the confirmational impact of any given piece of evidence. Suppose, for example, we have empirical hypotheses A, B, and C, which we could explain via theory "Zed," which postulates unobservable features of the world to account for A, B, and C. If we accept Zed as a true description of reality, then evidence that confirms A will also confirm Zed, which in turns lends support to B and C. But without Zed, evidence for A would give us no reason to think B and C are correct, since A, B, and C are all, as it were, independent hypotheses. To put it colloquially: we get more "theoretical bang for the buck" when we are willing to accept theories as true descriptions of the unobservable parts of the world. But this is plainly a repudiation of empiricism. For one version of this argument, see Glymour, *supra* note 37. The argument from understanding: only a realist interpretation of scientific theories yields genuine understanding of the world. Understanding, it is claimed, requires reducing the number of distinct phenomena we must simply accept as "brute;" that is, we must show these phenomena to be unified by some underlying (typically unobservable) mechanisms or structures. So if understanding requires unification, and unification requires accepting the existence of unobservable properties and events, then scientific understanding requires realism, not empiricism. For versions of this argument, see Michael Friedman, *Explanation and Scientific Understanding*, 71 J. PHIL. 5 (1974); and especially, Philip Kitcher's Kantian interpretation of the demand for unification (as a condition, as it were, on the very possibility of scientific explanation and empirical knowledge) in *Explanatory Unification and the Causal Structure of the World*, *in* 13 SCIENTIFIC EXPLANATION 410 (Philip Kitcher & Wesley Salmon eds., 1989), and *Projecting the Order of Nature*, *in* KANT'S PHILOSOPHY OF

not speak univocally, to be sure, but if anything is characteristic of their work, it is that the vast majority (including nonrealist contemporaries like Laudan) repudiate the Kuhnian conception of science.

B. *Epistemic Norms*

Historical accuracy aside, what is really important and welcome in Feldman's approach depends only on its commitment to the idea of *objective* progress in science, notwithstanding the failings of logical empiricist philosophy of science. This brings us to a more serious worry: Feldman could be right in her philosophy of science, but wrong in her implicit philosophy of evidence. From the claim that *Daubert*'s standards of admissibility track the picture of science bequeathed by "empiricist philosophy of science," Feldman draws the conclusion that the *Daubert* standards are preferable. But this hardly follows: good philosophy of science makes for, well . . . good philosophy of science (and maybe even good science)[40] but not necessarily good philosophy of evidence. To connect the two we need something more.

Although Feldman presents no systematic argument on this issue, she does observe at one point that, "[i]f scientists cannot draw firm conclusions, the jury cannot do so in any principled fashion, even if the expert witnesses claim to have conclusive opinions on the question of causation."[41] This neatly expresses the core intuitive idea underlying Feldman's equation of good norms for science with good norms of evidence. We can state the argument more explicitly as follows:

(1) the rules of evidence serve an epistemic value—truth;

(2) to promote this value, the rules set out norms for the admissibility of evidence that are most favorable to the discovery of truth;

PHYSICAL SCIENCE 210 (Robert E. Butts ed., 1986). For critical discussion of these and other arguments, as well as yet a different defense of realism against empiricism, see Railton, *supra* note 37.

40. Significantly, some philosophers of science deny that if scientists took empiricist philosophy of science seriously they really could pursue good science. *See* Railton, *supra* note 37, at 245-47.

41. Feldman, *supra* note 10, at 31.

(3) the norms of "revised empiricism" describe the way in which science reaches truth;

(4) therefore, when the truth depends on science, the norms of admissibility should track the norms of revised empiricism.

This argument, though superficially attractive, is, unfortunately, neither valid nor true. From the fact (if it is a fact) that "revised empiricism" describes how scientists discover truth, it does not follow that it sets out epistemic norms for how jurors should discover truth. In short, the fourth premise does not follow from the first, second, and third premises, and thus the argument is invalid. We have already seen that there are reasons for wondering whether the third premise is true;[42] we shall have occasion to see momentarily that the first and second premises are also false.

Notice, though, how this implicit argument figures in Feldman's account. Feldman observes, for instance, that on the "revised empiricist" picture, "[r]ival views are a valuable and usual part of the scientific process, providing fodder and stimulation for those researching in the mainstream."[43] Thus *Daubert* is preferable, since it will permit the admission of such rival views more often than *Frye*.[44] Yet plainly no one (Feldman included) wants to contend that *all* rival views are valuable for science. Thalean cosmology and Hubbardian scientology are rival views to those espoused by modern science, but no one, presumably, thinks they should be admissible into evidence. Platitudes about the importance of "rival views" to good science—platitudes that not even logical empiricists deny—do nothing to answer the lawyer's question: what criteria should nonscientist judges employ in deciding which rival views actually warrant admission?

Feldman, however, dismisses in a footnote the "academic focus on admissibility,"[45] but without ever explaining why this is not the only relevant focus for lawyers. Feldman dubs *Daubert* the "sensible approach to the admissibility of scientific evi-

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42. *See supra* Part II.A.

43. Feldman, *supra* note 10, at 16-17.

44. This crucial assumption is actually never defended by Feldman, and is probably false. *See supra* note 14.

45. Feldman, *supra* note 10, at 3 n.10; *see also supra* text accompanying note 14.

dence"[46] on the grounds that it marks "a more scientific approach to admissibility."[47] "[A] blanket prohibition [like *Frye*'s] on testimony based on cutting-edge techniques, methods, and ideas," says Feldman, "runs contrary to a scientific approach to gathering information."[48] But *Frye*, of course, did not place a prohibition on the gathering of information by *scientists*! *Frye* and *Daubert* do not have anything to say at all about how scientists proceed, only how courts do. Even if "revised empiricism" describes the correct epistemic norms *for science*—norms for how scientists should form beliefs—it is simply not dispositive as to the epistemic norms (i.e. the rules of evidence) that should govern the courtroom.

Feldman's conflation of the two questions misses the central issue of that branch of naturalized epistemology known as "social epistemology"[49] (precisely the epistemology that informs much of what Feldman calls "revised empiricism"). The social epistemologist asks: under the real-world epistemic limits of a particular social process for the acquisition of knowledge, what epistemic norms actually work the best? Questions about what rules should govern admissibility are, in this sense, questions of social epistemology. As such, they must be informed by two principles of social epistemology: epistemic paternalism and the "ought implies can" principle.[50]

Paternalism in any domain of legal regulation supposes that rules should substitute the rulemaker's judgment about what is best for agents for the agents' own judgments. *Epistemic* paternalism substitutes the rulemaker's judgment about what is *epistemically* best for agents for their own judgment. Assuming that the primary *epistemic* value is truth, epistemic paternalism entails designing rules of evidence that are epistemically best for jurors, i.e. that lead them to form true beliefs about disputed matters of fact. Doing so requires, of course, taking into account

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46. Feldman, *supra* note 10, at 3.

47. *Id.* at 5.

48. *Id.* at 7.

49. "Social epistemology is concerned with the truth-getting impact of different patterns and arrangements of social intercourse . . . such as classrooms, courtrooms, and assemblies." GOLDMAN, EPISTEMOLOGY, *supra* note 13, at 5. Traditional "primary epistemology," by contrast, is concerned only with the individual's "cognitive processes, structures, and mechanisms." *Id.*

50. On the former, see Alvin I. Goldman, *Epistemic Paternalism: Communication Control in Law and Society*, 88 J. PHIL. 113, 115 (1991); on the latter, see Alvin I. Goldman, *Epistemics: The Regulative Theory of Cognition*, 75 J. PHIL. 509, 510 (1978).

both the epistemic frailties of jurors, and the epistemic limits of the rule-appliers, namely judges. Our rules of evidence are generally premised on both considerations.

The "ought implies can" principle requires that normative advice in epistemological matters *not* be designed for *ideal* knowers, but for real-world knowers: any piece of epistemic advice of the form, "knowers *ought* to do A before forming a belief about Z," must imply that "knowers *can* do A before forming a belief about Z." In the case of rules of evidence governing admissibility, this presents a double issue, for here we have both *primary* epistemic rules and *secondary* epistemic rules (and sometimes the very same rule can be understood as serving *primary* and *secondary* epistemic functions).[51] Primary epistemic rules take into account the epistemic shortcomings of jurors, such as their susceptibility to confusion and prejudice or their generally modest level of intellectual ability. Secondary epistemic rules take into account the epistemic shortcomings of judges, such as their general lack of expertise in scientific matters. The rule of evidence that excludes unscientific evidence is a primary epistemic rule in the sense that it is predicated on the assumption that jurors must be "protected" from junk science in forming beliefs about disputed matters of fact. The rule of evidence requiring judges to exclude unscientific evidence is a secondary epistemic rule in the sense that it requires judges to make an epistemic judgment about whether some piece of evidence is scientific or not. *Daubert* is worrisome, in part, because it articulates a secondary epistemic rule that seems insensitive to the "ought implies can" principle as applied to judges. To put it crudely, *Daubert* as interpreted by Feldman says: "Judges ought only to admit genuine science, as revised empiricist philosophy of science defines 'genuine.'" But it is not clear (as many commentators—and the dissenters in *Daubert*—have worried) that judges actually *can* apply successfully the relevant philosophy of science to the issues they will confront.[52]

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51. As Richard Friedman points out to me, there is a tertiary epistemic function in the background here as well: namely, that involved when drafting committees and courts of last resort draft primary and secondary epistemic rules.

52. Chief Justice Rehnquist questions the wisdom of requiring judges to become "amateur scientists." Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 601 (1993) (Rehnquist, C.J., concurring in part and dissenting in part); *see also Developments in the Law—Confronting the New Challenges of Scientific Evidence*, 108 HARV. L. REV. 1481 (1995) (noting that the "[t]heoretically appealing" criteria of testability and

Thus, even if Feldman is right that *Daubert* articulates a picture of science that is correct (corresponding as it does to the purported best-going philosophy of science, namely "revised empiricism"), it does not follow from this that the rules of evidence ought to track this picture. Courtrooms, after all, are not laboratories, and judges are not scientists. For one thing, the discovery of truth is only *one* of the aims of adjudication under the Federal Rules. The rules of evidence serve distinctly nonepistemic purposes as well: the promotion of various policy objectives (like encouraging the repair of dangerous conditions)[53] and the efficient and timely resolution of disputes.[54] It is not clear that making sure that the evidentiary landscape of the courtroom mirrors that of the laboratory promotes all three of these purposes. Indeed, it seems that it quite clearly undermines the third.

This might be warranted, of course, if there were some significant epistemic payoff (in terms of the likelihood that jurors would discover the truth). But this is where the commonplace observation that judges (let alone jurors!) are not scientists[55] acquires special importance: for *Daubert* requires an extraordinary exercise of judgment on matters of considerable intellectual complexity if the evidentiary landscape of the courtroom is to match that of the laboratory—rather, say, than that of the *Na-*

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falsifiability may be too complicated for courts to apply); Margaret G. Farrell, Daubert v. Merrell Dow Pharmaceuticals, Inc.: *Epistemiology and the Legal Process,* 15 CARDOZO L. REV. 2183 (1994) (arguing that judges are ill-equipped to handle the "daunting responsibility" of determining whether scientific principles and methods are scientifically valid, a determination best left to the scientists themselves, through the "general acceptance" criterion); Randolph N. Jonakait, *The Meaning of* Daubert *and What That Means for Forensic Science,* 15 CARDOZO L. REV. 2103, 2103 (1994) (describing the *Daubert* Court's decision as "incomplete and often misleading" because it demands that a judge determine whether something is "scientific" generally—as opposed to determining whether something is good biology or chemistry—and this is often impossible because there are no general standards and methods applicable to all science generally that distinguishes "good" science from "junk"); Barbara Frederick, Note, Daubert v. Merrell Dow Pharmaceuticals, Inc.: *Method or Madness?,* 27 CONN. L. REV. 237 (1994) (arguing that in hard cases involving "novel" scientific evidence, judges will necessarily substitute their own judgment for that of the scientific community, because the *Daubert* guides given to judges are so vague as to allow too much discretion).

53. *See* FED. R. EVID. 407.

54. *See* FED. R. EVID. 102, 408, 409 & 410. This point is, in fact, acknowledged by Feldman toward the end of her essay. *See* Feldman, *supra* note 10, at 42.

55. *See, e.g.*, CHRISTOPHER B. MUELLER & LAIRD C. KIRKPATRICK, EVIDENCE 743 (1995) ("[C]ourts are ill equipped to make independent judgments on the validity of science. Most judges are not scientists, and they do not have the time to spend at trial or beforehand to make fully considered independent decisions on validity.").

tional Enquirer. One might have a very high opinion of the intellectual caliber of the federal bench, and still worry that not only does *Daubert* ask too much, but also its doing so is an act of futility given the all-too-human limits of judges. Rather than making sure juries get an accurate picture of the world as seen by science, *Daubert* will ensure only that juries get an accurate picture of the world as seen through the often distorting lens of zealous and resourceful advocacy.[56]

We plainly want our science in the courtroom to bear *some* relation to real science, for the reasons set out above. But this goal must be pursued in light of the serious epistemic limits of courts—intellectual, temporal, material. This is why the "academic focus on admissibility" that Feldman dismisses in a footnote is actually the correct focus. To be sure, it is interesting to see that one (slightly outdated) view in philosophy of science arguably recommends *Daubert*—at least as a philosophy of science! But *Daubert* is not supposed to be a methodological handbook for good science; it is supposed to set out a standard for good adjudication. No such standard can be formulated in indifference to the epistemic limits of courts.[57]

III. CONCLUSION

I agree with those critics who argue that *Daubert* makes unrealistic demands on the epistemic capacities of the adjudicatory process.[58] But I also agree with those writers who find *Frye*

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56. Surprisingly, Feldman sets precisely these considerations aside—yet these are the considerations central to the lawyer's question. *See* Feldman, *supra* note 10, at 31-32.

57. The *Daubert* Court itself is sensitive to this point. They try to gloss it by expressing their confidence that the epistemic limits of courts do *not* preclude the application of *Daubert*'s standards. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 595-97 (1993).

58. *See, e.g.*, Paul S. Milich, *Scientific and Technological Evidence: Controversial Science in the Courtroom:* Daubert *and the Law's Hubris*, 43 EMORY L.J. 913 (1994) (questioning whether judges can actually evaluate disputed science on its own terms); Katherine M. Atikian, Note, *Nasty Medicine:* Daubert v. Merrell Dow Pharmaceuticals, Inc. *Applied to a Hypothetical Medical Malpractice Case*, 27 LOY. L.A. L. REV. 1513 (1994) (stating that *Daubert*'s vision of judges as "gatekeepers" is "unrealistic," for judges are not scientists; *Daubert* will in many cases impose "immense burdens" on judges of understanding the proffered scientific evidence, for which they are not trained); John W. Osborne, Comment, *Judicial/Technical Assessment of Novel Scientific Evidence*, 1990 U. ILL. L. REV. 497 (noting many commentators' concern that judges are not adequately prepared to make scientific assessments).

too restrictive.[59] But if *Frye* is wrong, it is not because of its outdated epistemology of science, that is, its failure to appreciate the insights of "revised empiricism" (itself arguably an outdated epistemology of science!). *Frye* is wrong for familiar lawyerly reasons of policy and ethics. To my mind, the most compelling consideration arguing against *Frye* is that the vast majority of products at the heart of mass exposure litigation will have been manufactured and marketed based on appraisal of their safety in terms of the "generally accepted" scientific theories of the day (were they not, claims would lie for more than negligence or strict products liability!). *Frye*, then, poses the risk of presumptively favoring defendants in such suits, to the extent that it lets only the prevailing scientific wisdom get through the gate.[60] Yet *Frye*'s great virtue is its concrete recognition of the epistemic limitations of the courtroom setting: courts are not laboratories, but places in which (generally) nonscientists resolve disputes. Thus, *Frye* sets down a convenient *proxy* for scientific knowledge, namely acceptance in the scientific community (whatever

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59. *See, e.g.*, Arvin Maskin, *The Impact of* Daubert *on the Admissibility of Scientific Evidence: The Supreme Court Catches up with a Decade of Jurisprudence*, 15 CARDOZO L. REV. 1929 (1994) (stating that the *Frye* standard is contrary to the "liberal thrust" of Rule 402 of the Federal Rules of Evidence); Laura Etlinger, Comment, *Social Science Research in Domestic Violence Law: A Proposal to Focus on Evidentiary Use*, 58 ALB. L. REV. 1259 (1995) (stating that the restrictive *Frye* test is not appropriate for testing admissibility of expert psychological testimony); *see also* Ronald N. Boyce, *Judicial Recognition of Scientific Evidence in Criminal Cases*, 8 UTAH L. REV. 313 (1964) (stating that *Frye* is impracticable because it does not allow for application of novel and reliable forensic techniques that are used increasingly); Frederic I. Lederer, *Resolving the* Frye *Dilemma: A Reliability Approach*, 26 JURIMETRICS J. 240 (1986) (stating that *Frye* is "unduly conservative" in admissibility of "novel evidence"; what is generally accepted might not represent the most accurate method of scientific fact-finding); Recent Case, *Evidence—Admissibility of Scientific Evidence—Fifth Circuit Limits Permissible Scientific Evidence to Generally Accepted Theories*, 105 HARV. L. REV. 791 (1992) (stating that the *Frye* standard is too stringent in toxic torts cases).

60. I am glossing over a much more complicated, but still exclusively "lawyerly," debate here. For example, one might think the presumption alluded to in the text is, in fact, warranted, if the defendant really did conform its product to the existing state of scientific knowledge. Moreover, in actual practice, *Frye* figured far more often as protection for criminal defendants, rather than defendants in products liability actions. Finally, the argument in the text supposes that the central rationale for *Frye* is epistemic: but this surely is not quite right. For there may also be a "policy" concern that juries are predisposed to return verdicts against large companies, and assuming we do not want to redistribute wealth that way, we may want the rules of evidence to make it harder, rather than easier, upon plaintiffs to appeal to novel scientific theories in proving their case. Since these complications are tangential to my main themes, I ignore them in the text. It bears noting again, though, that all these issues can be resolved in indifference to the issues in philosophy of science.

the ambiguities attendant upon the notion of "acceptance").[61] If *Frye* is problematic, it may only be because it sets the threshold of acceptance too high. Yet *Frye* succeeds in framing a realistic epistemology of admissibility, one that takes into account, as any good social epistemologist should, the epistemic limits of the relevant knowers.

61. For relevant discussion of this problem, see generally *Symposium on Science and Rules of Evidence*, 99 F.R.D. 187 (1983) (noting the "intractable ambiguity" of the *Frye* standard); Bert Black, *A Unified Theory of Scientific Evidence*, 56 FORDHAM L. REV. 595 (1988) (noting the "extreme incoherence and inconsistency" in post-*Frye* decisions as a result of the ambiguous "acceptance" standard); Paul C. Giannelli, *The Admissibility of Novel Scientific Evidence:* Frye v. United States, *a Half-Century Later*, 80 COLUM. L. REV. 1197 (1980) (calling into question the efficacy of the "general acceptance methodology" insofar as it fails to determine the "relevant scientific community" in which "general acceptance" is to be measured); Ed Koon, Note, *Evidence—New Federal Standard For Admission of Scientific Evidence:* Daubert v. Merrell Dow Pharmaceuticals, Inc., 17 U. ARK. LITTLE ROCK L.J. 135, 144 (1994) ("The *Frye* test gives no guidance as to which experts should be counted, nor does it explain whether 'general acceptance' means virtually everyone counted, a majority, or perhaps only a substantial number."); James Lang, Note, *Hearsay and Relevancy Obstacles to the Admission of Composite Sketches in Criminal Trials*, 64 B.U. L. REV. 1101 (1984) (stating that although "it is agreed" that unanimity of acceptance is not required under *Frye*, how much divergence is allowed is unclear).