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David S. Caudill

Richard E. Redding

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Junk *Philosophy* of Science?: The Paradox of Expertise and Interdisciplinarity in Federal Courts

David S. Caudill*
Richard E. Redding**

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* Professor of Law, Washington and Lee University School of Law; Ph.D. (philosophy), Vrije Universiteit te Amsterdam; J.D., University of Houston; B.A., Michigan State University. A modified version of this Article was presented as a paper on Sept. 19, 1999 at the Critical Legal Conference held at the University of London and sponsored by Birkbeck College Law Department. The authors are grateful to Professors Lewis H. LaRue and Jack Wilson for their critical comments on earlier drafts of this Article and for the research assistance of Jonathan Gould, J.D. candidate, 2001, Washington and Lee University School of Law.

** Assistant Professor and Associate Director, Institute of Law, Psychiatry, and Public Policy, University of Virginia School of Law; Ph.D. (psychology), University of Virginia; J.D., Washington and Lee University; M.S., Vanderbilt University; B.A., Hampden-Sydney College.

I. Introduction

Writing about *Daubert v. Merrell Dow Pharmaceuticals, Inc.*[1] – which set new standards for the admissibility of scientific evidence in the federal courts – is a bit like writing about Salem witchcraft, as explained by colonial historian Mary Beth Norton:

> Random Early Americanist: "What are you working on now?"
> Me (with some hesitation): "Salem witchcraft."
> Early Americanist . . . : "But . . . surely there's nothing new to say."[2]

Professor Norton, however, had become "progressively dissatisfied with [the] limited framework" in the leading study of the social mileau of 1692 Salem because of its insistence that gender is irrelevant to the story.[3]

As to the ongoing debates about law and science, we have become progressively dissatisfied with the limited philosophical framework that predominates in the judicial and scholarly accounts of the validity and admissibility of scientific testimony. Although "*Daubert*, perhaps, represents nothing more, *or less*, than that henceforth the legal culture must assimilate the scientific culture,"[4] the interdisciplinary interaction between law and the *philosophy* of science has, to date, been disappointing.[5] While the process of assimilation

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1. 509 U.S. 579 (1993).

2. Mary Beth Norton, *Finding the Devil in the Details of the Salem Witchcraft Trials*, CHRON. HIGHER EDUC., Jan. 21, 2000, at B4.

3. *Id.* Professor Norton explained:

> Along with many other historians, I had once accepted Paul S. Boyer and Stephen Nissenbaum's 1974 *Salem Possessed: The Social Origins of Witchcraft* . . . as the final word on the subject . . . . [However,] I found it inconceivable that gender could have played absolutely no role in the development and outcome of the crisis.

Id.

4. 1 DAVID L. FAIGMAN ET AL., MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY ix (1997).

5. Just as scholars who engage in "historical, philosophical, or managerial studies of science and technology . . . become increasingly autonomous from the sciences and technologies they study," DAVID J. HESS, SCIENCE STUDIES: AN ADVANCED INTRODUCTION 149 (1997), their work is largely absent from the representations of science dominating legal literature.

certainly has captured the attention of legal scholars – much has been written about *Daubert*[6] ("an avalanche of commentary"[7]) – only a few commentators have focused on *Daubert*'s minimalistic description of the scientific process or the philosophy of science that underlies *Daubert* and its progeny.

According to many recent scholarly accounts, the "fairy tale" view of science as objective and its history as a linear progression toward truth about nature has been replaced in the federal courts by the modern view of science as tentative, uncertain, and embedded in culture. In our view, this account is a *new* fairy tale about how those who have rejected the old tale have settled matters. This new fairy tale suggests a certain stability with respect to the history and philosophy of science that does not exist. There are not two schools of thought concerning the nature of scientific activity, but rather a complex and ongoing theoretical discourse that renders interdisciplinary "borrowings" by law from science studies intensely problematical.[8] The dualistic model is, therefore, an interdisciplinary trap. It oversimplifies the debates about the characteristics of scientific activity and eclipses at least one major insight about science: It is problematical to define science as either a natural or a social activity. The latest and most helpful scholarly work among those who study scientific activity reformulates contemporary accounts of science to avoid the dualistic trap that scholars have repeated over and over again in most commentary on *Daubert*.[9]

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6. *See* Dick Thornburgh, *Junk Science – The Lawyer's Ethical Responsibilities*, 25 FORDHAM URB. L.J. 449, 455 (1998) (pointing out that judges and scholars have cited *Daubert* in more than 1000 federal and state cases and in more than 1000 law review articles).

7. Edward J. Imwinkelried, *Evidence Law Visits Jurassic Park: The Far-Reaching Implication of the* Daubert *Court's Recognition of the Uncertainty of the Scientific Enterprise*, 81 IOWA L. REV. 55, 58 (1995) (noting "avalanche of commentary" generated within first year following *Daubert*, citing two symposia, and referring to "massive – and growing – volume of commentary").

8. Even defining "science studies" is intensely problematical. The term usually refers to more radical studies of science – movements like the Sociology of Scientific Knowledge (SSK); Science, Technology, and Society Studies (STS); Science and Culture Studies; or post-modern studies of science. These studies are referred to by critics as a "veritable carnival of approaches and methodologies," including feminists, Marxists, ethnomethodologists, and deconstructionists "who find significance in rhetoric" or "who emphasize the role of patronage and the power of empire." Noretta Koertge, *Scrutinizing Science Studies*, *in* A HOUSE BUILT ON SAND 1, 3 (Noretta Koertge ed., 1998). Koertge winced at this use of the term "science studies," because she felt "privileged to work in one of the oldest 'science studies' departments [Indiana University] in the country (although we never used that label)." All serious scholars in history, philosophy, and sociology of science could be said to be engaged in science studies. *Id.* at 5.

In this Article, we use the term "science studies" to refer generally to traditional and critical views of science, but readers should recognize that the term is often reserved for more radical approaches.

9. *See generally* Philip Kitcher, *A Plea for Science Studies*, *in* A HOUSE BUILT ON SAND, *supra* note 8, at 32-56. Kitcher identified a realist-rationalist cluster of ideas which included

We argue that the view proposed by many commentaries on *Daubert* – that the Court vacillated between traditionalist and constructivist views of science – sets up an erroneous dichotomy that not only eclipses the diversity of positions in science studies but also misses entirely a third approach adopted by some federal courts in applying *Daubert*. This third approach is neither traditional nor constructivist. Rather, it is a pragmatic approach to scientific evidence we call "pragmatic constructivism," which "constructs a science for the case" and satisfies neither traditionalists nor constructivists. At first blush, it even appears that this pragmatic approach sidesteps altogether the complex philosophical questions about the nature of science and scientific progress.

But why should we, in law, care about the philosophy of science? After all, science does not need philosophical reflection or criticism to succeed,[10] and we would not question the legal admissibility of a scientific explanation

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canons of reason and evidence, and a view of science as progressive, as offering representational knowledge about the world and as fallible but roughly correct. *Id.* at 34. Kitcher also identified a socio-historical cluster of ideas, which included a view of scientists as cognitively limited and functioning within complex social groups with long histories, and a view of science as embedded in social structures. *Id.* at 36. The "challenge today is to do justice to both clusters of themes," *id.* at 45, but there is substantial disagreement and debate about how that is done. Thus, there are numerous "third way[s]" to avoid the seeming choice between an overly realist view of science that ignores science's history and social structures, and a social constructivist view of science that disengages science from natural reality. For a lively debate that captures various "third way[s]," see TAKING THE NATURALIST TURN, OR HOW REAL PHILOSOPHY OF SCIENCE IS DONE (Werner Callebaut ed., 1993) [hereinafter Callebaut] (confirming continuum of positions between realist-rationalist and socio-historical poles).

10. *See* JOSEPH ROUSE, ENGAGING SCIENCE: HOW TO UNDERSTAND ITS PRACTICES PHILOSOPHICALLY 3 (1996). Rouse stated:

> [W]e are not entirely clear on what is at stake among competing philosophical interpretations of scientific knowledge. What difference would it make if we were to discover that science is best understood instrumentally or realistically or in terms of its historically progressive rationality? . . . [I]t is striking how limited are the ambitions of so many contemporary philosophical interpreters of the sciences.

Id.; *see also* RICHARD J. BERNSTEIN, BEYOND OBJECTIVISM AND RELATIVISM: SCIENCE, HERMENEUTICS, AND PRAXUS 89 (1983) (arguing history of science "essential" for historians but not for scientists). Bernstein observed that:

> Sometimes critics of Feyerabend and Kuhn have interpreted them as saying that physicists ought to be historians and hermeneuticians, but this is silly . . . . Knowledge of science's past and study of its history are essential for philosophers and historians who want to understand the nature or image of science but are not necessarily relevant for developing the skills required to be a practicing scientist . . . .

Id. Significantly, scholars have challenged the notion that the history of science is crucial to our conception of science today. *See* Thomas M. Norton-Smith, *A Consideration of the Role of the History of Science in the Philosophy of Science*, 31 MIDWEST Q. 330, 335 (1990) (arguing that while recent historical examples of scientific discovery and progress may be helpful, older historical examples may not be so relevant to understanding present-day science).

because the scientist did not have a sufficient philosophical account of the scientific enterprise. Yet when judges are faced with conflicting scientific testimony and are required to evaluate the validity and admissibility of that testimony, they need a generalized template – an abstract picture of how science works. Judges know from experience that science is sometimes tentative and uncertain, that scientists often disagree, that scientists have other interests (in their careers, in helping a client, in getting paid), and that once-established theories are later replaced. They also know, however, that law needs scientific expertise. Accordingly, they will, perhaps unwittingly, develop a philosophy of science to square these realities.

For some, however, the scientific enterprise may function as a particularly stable source of insights for law, such that a judge's philosophical or ideological inclinations should not affect his or her evaluation (under *Daubert)* of scientific reliability.[11] After all, the scientific method purports to be an objective standard outside of legal discourse. We will, however, demonstrate that a philosophical position on science is unavoidable and that judicial assessments of scientific reliability after *Daubert* can vary according to undisclosed philosophies of science. Specifically, the development of *Daubert* in subsequent federal court opinions subtly reveals three interpretive principles that implicitly reflect judicial philosophies of science. These principles function inconsistently to modify *Daubert* standards of admissibility. After illustrating the varied operations of the three principles, we show how more liberal approaches to admissibility are consistent with contemporary insights from the history, philosophy, and sociology of science, whereas the more restrictive or conservative approaches reflect an outdated vision of the scientific enterprise.

In Part II below, we describe the limited philosophical framework that we criticize. In Part III, we construct our own framework, oriented to philosophical concerns, for analysis of cases involving the admissibility of expert testimony under *Daubert*. Part IV begins with a re-reading of *Daubert* to identify the philosophies of science reflected in the opinion. We then turn to post-*Daubert* cases to show how the federal courts manipulate *Daubert* guidelines and several auxiliary principles to fit different philosophical perspectives on the scientific enterprise. In order to demonstrate that the popular notion found in court opinions and legal scholarship of "two views of science" is highly oversimplified, Part V provides a brief survey of the debates concerning the history, philosophy, and sociology of science. Part VI traces the "two views" thesis in *Daubert* and in selected commentaries on *Daubert*.

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11. *But see* Richard E. Redding & N. Dickon Reppucci, *Effects of Lawyers' Socio-Political Attitudes on Their Judgments of Social Science in Legal Decision Making*, 23 LAW & HUM. BEHAV. 31 (1999) (finding trial judges' evidentiary rulings on socially controversial evidence biased by their own sociopolitical opinions).

In Part VII, we explain how legal literature has misappropriated the "social constuctivist" view of science, argue that the positivism that scholars set up against constructivist views is not the only alternative, and identify a third position – pragmatic constructivism – that has been appropriated in law and exemplified in several post-*Daubert* federal court opinions. This position, which represents a weak constructivist approach towards scientific evidence, finds support among some contemporary science studies scholars. We conclude, however, that this interdisciplinary borrowing often fails because the hoped-for stability (for law) from the insights of science studies runs up against the hidden instability of those disciplines.

Thus, the paradox referred to in our title is that the field of science studies is too complex and unsettled for interdisciplinary engagement with law. Any position taken by the United States Supreme Court, therefore, would have been controversial and subject to endless attack, requiring a time-consuming defense. For example, if the Court viewed science as a social enterprise supported by rhetorical conventions and by institutional practices subject to bias, it would have been difficult to decide how to investigate and identify rhetorical networks and institutional bias. Likewise, if the Court viewed science as a realist enterprise, in reaction to historicist and social constructivist accounts of science, it would have been difficult to choose between competing contemporary accounts of science as representational model-building, as a probabilistic inquiry, or as conceptual evolution, to name a few important realist projects.

But once the Court began talking about science, the interdisciplinary engagement necessarily began and the contours of a philosophical position began to appear. The only two options at that point were to explicate and adequately defend the Court's position, which was unlikely, or to maintain enough ambiguity to let lower court judges and scholarly commentators deal with the problem, which was the path taken in *Daubert*. The paradox persists, nevertheless, because even judges who want to avoid entanglement with debates in the philosophy of science must somehow characterize science and unwittingly take a position in those debates. The only escape, described later in this Article as "pragmatic *legal* constructivism," is to set aside the philosophical problems of defining science. This move ends up conceding, unintentionally, that law's science is almost wholly independent from the scientific enterprise.

II. Daubert: *A Limited Framework for Understanding Science*

The United States Supreme Court was on controversial ground when, in *Daubert*, it began to reflect upon the characteristics of scientific knowledge and methodology. While it seemed necessary to clarify the elements of scien-

tific validity when setting a new standard for the admissibility of scientific evidence in federal courts, the task proved problematical. Chief Justice Rehnquist, in his partial dissent, immediately observed that the twenty-two amicus briefs filed with the Court were "markedly different from typical briefs."[12] He also claimed not to understand "what is meant" by the statement that "the scientific status of a theory depends on its 'falsifiability'" and remarked that he suspected that some trial judges would likewise be confused.[13] More importantly, the Court's analysis required an engagement with a highly contested field of inquiry. For decades, debates over the nature of scientific discovery and progress have thrived among historians, philosophers, sociologists, and more recently, anthropologists, ethnologists, and even literary theorists, not to mention reflective scientists.[14] The *Daubert* Court was unable, therefore, to enter the field of science studies, to find the established definitions of scientific knowledge and methodology, and to bring those stable insights home to law. Rather, the Court was required to engage an ongoing controversy.

From the face of Justice Blackmun's opinion in *Daubert*, however, one might not notice that defining science is both problematical and controversial. The opinion certainly is framed as progressive and as an opportunity to re-articulate and re-interpret admissibility standards under the Federal Rules of Evidence, but the Court's observations about the scientific enterprise are presented almost as facts of general notice. Everybody knows, it would seem, that "there are no certainties in science"[15] and that publication in scientific journals "does not necessarily correlate with reliability."[16] For whatever reason, probably rhetorical, Justice Blackmun did not mention that the amicus briefs reflected at least two competing pictures of scientific activity or that the Court was not simply describing science, but choosing among alternative descriptions.

The undisclosed controversy in *Daubert*[17] is supposedly between adherents of the traditional picture of science as an objective inquiry into nature

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12. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 599 (1993) (Rehnquist, J., concurring in part and dissenting in part).

13. *Id.* at 601.

14. *See generally* RONALD N. GIERE, SCIENCE WITHOUT LAWS (1999); DAVID L. HULL, SCIENCE AS PROCESS: AN EVOLUTIONARY ACCOUNT OF THE SOCIAL AND CONCEPTUAL DEVELOPMENT OF SCIENCE (1988); PHILIP KITCHER, THE ADVANCEMENT OF SCIENCE: SCIENCE WITHOUT LEGEND, OBJECTIVITY WITHOUT ILLUSIONS (1993); LARRY LAUDAN, BEYOND POSITIVISM AND RELATIVISM: THEORY, METHOD AND EVIDENCE (1996); DEBORAH MAYO, ERROR AND THE GROWTH OF EXPERIMENTAL KNOWLEDGE (1996); ANDREW PICKERING, THE MANGLE OF PRACTICE: TIME, AGENCY, AND SCIENCE (1995); ROUSE, *supra* note 10.

15. *Daubert*, 509 U.S. at 590.

16. *Id.* at 593.

17. The undisclosed controversy in *Daubert* is obvious in the amicus briefs submitted in the case and in the flood of scholarly commentary that followed the opinion. *E.g.*, Erica Beecher-Monas, *Blinded by Science: How Judges Avoid the Science in Scientific Evidence*, 71

or reality, as opposed to critics who recognize that science is a social and cultural activity, that theoretical commitments affect observation, that the history of science is not one of linear progress, and that science is always tentative and uncertain. At first glance, that dichotomy is helpful in understanding the influential amicus briefs in *Daubert*, the *Daubert* opinion itself, the arguments among legal scholars about *Daubert*, and the extra-legal controversy among historians, philosophers, and sociologists of science that was imported into law in *Daubert*. Indeed, the two-view dualism is a useful description of a set of challenges, raised roughly in the 1970s, to traditional accounts of empirical scientific methodology as the most rational, logical, and objective source of knowledge.[18] Critics could point to the failure of historians to

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TEMP. L. REV. 55 (1998); M. Neil Browne et al., *The Epistemological Role of Expert Witnesses and Toxic Torts*, 36 AM. BUS. L.J. 1 (1998); Daniel J. Capra, *The* Daubert *Puzzle*, 32 GA. L. REV. 699 (1998); Ellen E. Deason, *Court-Appointed Expert Witnesses: Scientific Positivism Meets Bias and Deference*, 77 OR. L. REV. 59 (1998); Margaret Farrell, Daubert v. Merrell Dow Pharm., Inc.: *Epistemology and Legal Process*, 15 CARDOZO L. REV. 2183 (1994); Michael H. Gottesman, *From* Barefoot *to* Daubert *to* Joiner: *Triple Play or Double Error?*, 40 ARIZ L. REV. 753 (1998); Imwinkelried, *supra* note 7; Brian Stuart Koukoutchos, *Solomon Meets Galileo (and Isn't Quite Sure What to Do with Him)*, 15 CARDOZO L. REV. 2237 (1994); Erin K. L. Mahaney, *Assessing the Fitness of Novel Scientific Evidence in the Post-*Daubert *Era: Pesticide Exposure Cases as a Paradigm for Determining Admissibility*, 26 ENVTL. L. 1161 (1996); John H. Mansfield, *Scientific Evidence Under* Daubert, 28 ST. MARY'S L.J. 1 (1996); Allison Morse, *Social Science in the Courtroom: Expert Testimony and Battered Women*, 21 HAMLINE L. REV. 287 (1998) (stating that *Daubert* "vacillates between two paradigmatic world views, the Positivist and the Constructivist").

18. *See* Paul Horwich, *Introduction* to WORLD CHANGES: THOMAS KUHN AND THE NATURE OF SCIENCE 2 (Paul Horwich ed., 1993). Horwich stated:

> According to the old picture, each scientist proceeds by increasing his stock of observed facts, employing a set of nondeductive logical principles to determine which set of theoretical sentences best explains these facts, and thereby accumulating theoretical knowledge. This rather tempting conception [assumes] . . . that there is such a thing as the absolute truth independent of language and theory [and] . . . that there exists a theory-neutral body of observable facts . . . .

Id. Horwich's account of the new philosophy of science, like most accounts, begins with the publication in 1962 of Thomas Kuhn's *The Structure of Scientific Revolutions*:

> Kuhn's critique called into question many of the central elements of the traditional picture – the concept of absolute truth, the observation/theory distinction, the determinacy of rational choice, and the normative function of the philosophy of science – and it provided an alternative model of scientific change that dispensed with these notions altogether.

Id. at 1. In another formulation, Kuhn "assailed the universal adjudicating power of experiments, and therefore their independence from theory. Instead of arguing that observation must precede theory, Kuhn contended that theory has to precede observation." PETER GALISON, HOW EXPERIMENTS END 8 (1987). If Kuhn emphasized how theoretical expectations organize perception, some sociologists of science have gone further to denigrate the role of nature by emphasizing how class, religion, professional interests, group commitments, and presuppositions "condition the admissible phenomena in such a way as to render a particular theory and

"rationally reconstruct" discovery, to episodes in the history of science when empirical assumptions changed radically, and to the influence of presuppositions on observation, all in an attempt to describe accurately and modestly the genesis and growth of scientific knowledge. Even in the 1970s, however, it would have been improper, except in the most general sense, to say that there were two views of science. Disagreements persisted among the critics, with some more radical in their critique of scientific orthodoxy than others. Most significantly, the critical schools of the 1970s continued to develop, studies of scientific activity appeared in numerous disciplines, and a lively debate continues about how best to describe the power and limitations of scientific methodology and knowledge. There are almost as many positions in the debate as there are leading scholars in the field.[19]

In many recent accounts, the acknowledgment that science is always a human enterprise involving social institutions and linguistic conventions does

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its associated experiments closed and self-referential." *Id.* at 10. Some are more willing than others to downplay the role of nature in science. If those who think nature is accessible to science are realists and those who concede that nature influences science are closet realists, a few more positions are available before you get to full-blown social constructivism in science. For example, facts can be viewed as constructed against the *resistances* of the natural (and social!) order – that veridical reality will only allow our constructions to go so far. *See* JONATHAN POTTER, REPRESENTING REALITY: DISCOURSE, RHETORIC AND SOCIAL CONSTRUCTION 36 (1996); Karen Barad, *Agential Realism: Feminist Interventions in Understanding Scientific Practices, in* THE SCIENCE STUDIES READER 1, 2 (Mario Biagioli ed., 1999) ("Agential Realism . . . acknowledges that there is a sense in which 'the world kicks back' . . . ."); K. D. Knorr Cetina, *The Care of the Self and Blind Variation: The Disunity of Two Leading Sciences, in* THE DISUNITY OF SCIENCE: BOUNDARIES, CONTEXTS AND POWER 287, 308 (P. Galison & D. Stump eds., 1995); Joseph Rouse, *Beyond Epistemic Sovereignty, in* THE DISUNITY OF SCIENCE, *supra*, at 398, 517-18 n.27 (regarding "resistance," Rouse does not "respect any sharp distinction between actions by people and behavior by things").

19. For example, Philip Kitcher, a leading philosopher of science, characterized his view as realist and progressive. Realism is suspect, however, "not only among those who are skeptical about the progressiveness of science but also for champions of alternative accounts of scientific progress." KITCHER, *supra* note 14, at 127. Laudan, Kuhn, and van Frassen would object to Kitcher's realism but then offer accounts of scientific progress. Many "contemporary sociologists of knowledge, Bloor, Barnes, Collins, Shapin, Latour, Pickering, and others . . . surely find [Kitcher's] account of progress incredible." *Id.* Hull contrasted externalists who emphasize influential social factors with internalists who highlight reason, argument, and evidence. HULL, *supra* note 14, at 10-11. Some externalists, however, adopt a realist orientation, and differences of opinion exist among internalists. Laudan "is as unhappy with traditional realist philosophers of science as relativists are." *Id.* Therefore, "[a]mong those who study science, there is as much intragroup conflict as intergroup conflict, possibly more." *Id.* at 11. The terms "intergroup" and "intragroup" suggest that there are groups in conflict – externalists vs. internalists, realists vs. social constructivists – but also tremendous diversity on each side. Most importantly, the turn away from traditional positivist accounts of scientific rationality is not necessarily a turn to some version of social constructivism. Giere's representationalism, *supra* note 14, Mayo's work in probability theory, *supra* note 14, and Hull's evolutionary account of scientific progress, *supra* note 14, serve as examples.

not imply that scientific models do not represent reality[20] or that scientific knowledge is constructed without regard to interactions with nature.[21] For example, some of the major scholars in the field, without giving up on their effort to account for the social aspects of and influence on science, also are accounting for the ways in which "reality" resists some constructions and stabilizes others.[22] Many view the clear distinction between nature and society as a false dualism that gets in the way of a workable account of science.[23] From an interdisciplinary perspective, therefore, law's recent engagement with the history, philosophy, and sociology of science is impoverished and limited to importing a simplistic version of an old debate that has since been enriched by contributions from numerous disciplines.

Why do we continue to speak of two views of science? Several explanations come to mind. First, there is the usual necessity, especially in law, to reduce complex debates and highly contested fields of inquiry for purposes of discourse and evaluation. In legal disputes, we are accustomed to having two sides in conflict – one can be rejected in favor of the other. Second, we can use the debates in other disciplines rhetorically in legal argument, perhaps to show that law is out of step with the "new" or dominant view in another discipline, or even to highlight that the disarray of another discipline is of little use in law. Finally, we want to appropriate relevant interdisciplinary knowledge without letting the other disciplines' internal debates undercut our borrowings. In short, our characterization of another discipline needs to be simplified for use in legal understanding and argument.

An analogy to the use in law of interdisciplinary insights about the nature of scientific activity might be the use in literary studies of textual scholarship. Textual scholars use various methods, partly scientific and partly rhetorical, to "establish" a literary text from its various sources.[24] A literary scholar who wants to study or teach a particular text needs an answer, the text, not an unending debate about all the problems of textual scholarship. The field of textual scholarship is problematic and controversial, but to avoid getting lost in its details one looks for the bottom line, the best construction of a text. The

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20. *See generally* GIERE, *supra* note 14. Giere wrote:

> One wonders whether there is not a middle way .... The winning combination, I suggest, is one that gives up the search for criteria of scientific rationality, abandons the attempt to separate the content and methods of science from psychological and sociological reality, but preserves the view of science as a representational activity.

Id. at 44.

21. *See generally* ROUSE, *supra* note 10.

22. *See supra* note 18.

23. *See, e.g.*, ROUSE, *supra* note 10, at 148-49 (discussing "natural world" as opposed to "social world").

24. *See generally* D.C. Greetham, *Textual Forensics*, 111 PMLA 32 (1996).

situation in textual scholarship is not unlike the study of scientific activity insofar as the field is a highly complex debate among numerous academic competitors, and it is hard to find a dominant viewpoint. It is, however, easy to see that almost all of the positions in the field are critical of the traditional view of science as an objective activity that confirms theories and thereby accumulates knowledge. Thus, law constructs a story about the discipline of science studies. There are two views, the old and the new, but in law we want the new.

The authors of the most influential amicus briefs in *Daubert* could therefore represent to the Court that the traditional view was old-fashioned and that everybody basically holds the new view nowadays. Critics of junk science might want a return to an old view, but serious scholars, who studied science in the 1970s, adopted a new view. Supposedly, not much has changed since.[25] Justice Blackmun then could observe that science is an uncertain enterprise, implying that novel theories may eventually replace the established theories of mainstream science.[26] Scholars seemingly accepted the "new" view of science, and the Court went on the describe the characteristics of science.[27]

Interestingly, the new view adopted by the Court is a bit of late 1970s criticism from the history and philosophy of science combined with a fairly conservative commitment to traditional views of science. There is, for example, the Court's concession that science is uncertain. Practicing scientists admit that; however, it is hardly a critique of the scientific establishment.[28] While concluding that novel, unpublished science is often good, the Court also seems to suggest that good, unpublished science is rare.[29] Finally, the Court praised the adversary system as an arbiter of scientific disputes, and

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25. *See* Brian Leiter, *The Epistemology of Admissibility: Why Even Good Philosophy of Science Would Not Make for Good Philosophy of Evidence*, 1997 B.Y.U. L. REV. 803, 809-12 (1997) (noting "unfortunate impression" in law schools that "Kuhn . . . marks the last important development in post-positivist philosophy of science"). "[C]ontemporary philosophers of science do not speak univocally, to be sure, but if anything is characteristic of their work, it is that the vast majority . . . repudiate the Kuhnian conception of science" that became popular in the 1970s. *Id.* at 811-12.

26. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 590 (1993) (stating science is uncertain); *id.* at 593 (stating publication not dispositive); *id.* at 597 (stating authentic innovations might not be admitted in court, which implies that mainstream science might be wrong).

27. *See id.* at 592-95.

28. *See* Robert Boyd, *The Tussle for Truth: Why Science Always Seems to Be Fraught with Uncertainties*, TORONTO STAR, Dec. 28, 1997, F8 (quoting Nobel Prize-winning chemist John Polanyi: "[H]aving wrong ideas, as we often do, is a necessary part of the process of having right ones"). Scientists "work by trial and error" and "upset old dogmas and establish new ones." *Id.*

29. *See Daubert*, 509 U.S. at 593 ("Some propositions . . . are too . . . new . . . to be published. But submission to the scrutiny of the scientific community is a component of 'good science' . . . .").

although this might seem to be a critique of the institutions of mainstream science as non-neutral arbiters of good science, no doubt is cast on the activity of science on its own turf. Justice Blackmun is simply contrasting the courtroom with the laboratory in how knowledge is obtained.[30] We are left with a traditional list of characteristics of science, slightly tempered with a nod to the insights of 1970s history and philosophy of science.[31] In post-*Daubert* jurisprudence, the traditional view is further tempered by the observation that not all good science will have all of the traditional characteristics.[32] This flexibility devolves into a pragmatic use of the characteristics of science as "factors" that help a trial judge assess scientific evidence.

In *Daubert*, the Court did not really adopt the traditional view as a legal standard, and the progressive view was so ill-defined that it could not serve as any sort of standard. The new view seemingly included all possible philosophical, historical, and sociological insights about the limitations on scientific activity, whether they be theoretical presuppositions, social and political influences, inexact instrumentation, or scientific fraud. Some of these insights were probably influential in *Daubert*, but in the end the Court almost bracketed the question of the nature of science as unanswerable. The science we get is sometimes biased toward established theories and is often uncertain. Whether or not science eventually gets things right is beside the point, however, because courts need science now. Nevertheless, through the adversary system and a judge who will keep out really bad science, courts will let one side win in a sort of legal construction of science. Indeed, the suggestion in *Daubert* that admitting scientific testimony does not mean that it is correct[33] is almost celebrated by some lower federal courts.[34] Such a view does not reflect a deference to science that the critics of junk science wanted, but neither is it a critique of science as a social construction and an historical affair. Rather, it is an avoidance tactic with respect to the complexities of the history, philosophy, and sociology of science.

If the *Daubert* Court were to have genuinely engaged in the debates over the nature of science, the very first insight imported would be that there are not two views of the nature of science in the field. The instability of the field might have led the Court to appreciate the difficulty in defining science and

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30. *See id.* at 596-97.

31. *See id.* at 593-94 (stating that science is testable, published, generally accepted knowledge produced by methodology with low rate of error).

32. *See* Kumho Tire Co. v. Carmichael, 526 U.S. 137, 152 (1999) ("[W]e conclude that the trial judge must have considerable leeway in deciding in a particular case how to go about determining whether particular expert testimony is reliable.").

33. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 598 (1993) (referring to shaky but admissible evidence).

34. *See infra* Part VII.B.

to understand why its definition of science was doomed to become not a standard but a flexible list of factors. Decades ago, historians, philosophers, and sociologists debunked the traditional view of science precisely because it did not adequately describe science.[35] More recently, after historicist and social constructivist approaches became popular, many have reacted against the social and historical turn (though not by returning to traditional positivism).[36] Therefore, the "new" view is not a unified but a complex debate over how to best account for science's uncertainty and institutionalized bias while acknowledging science's power and success.

The second insight from the field is that stable insights are hard to come by. A third insight from seriously engaging the field of studies of science is that the troubling debate over whether science is explainable by reference to nature or to society is best viewed as another trap.[37] While holders of the traditional view tended to ignore social influences, some early critics tended to overemphasize the social context and, in order to distance themselves from the traditionalists, to view all facts as social constructs. This trap – this sense that one must decide between viewing science as grounded in nature and reality versus viewing science as a social production grounded in belief, representation, and rhetoric – has actually functioned within the field of studies of science to renew and revitalize the effort to explain scientific power and progress. Unfortunately, for those outside the field, like lawyers and legal scholars, the trap functions as a barrier to any serious consideration of what is happening in studies of science. Whenever the history, philosophy, and sociology of science is viewed solely as a debate between realists and social constructionists or between those who believe in nature and those who view nature as a social construct, serious engagement with the field no longer appears to be worth the effort. Some other representation of the field is necessary before the history, philosophy, and sociology of science can be a rich source of interdisciplinary insights for law.

In summary, the persistent problem in defining the scientific enterprise is whether science describes or deals in nature and reality. While "most scientists, most members of the public, and some historians and philosophers of science" think so, the least reflection on science reveals limitations on scientific access to nature and reality due, for example, to current theoretical models, institutional arrangements, innovation in instrumentation and mea-

35. *See* GIERE, *supra* note 14, at 4 ("[T]he very categories in which the Enlightenment view of science was formulated are inadequate to capture the actual practice of science, both historically and in its contemporary forms."); LAUDAN, *supra* note 14, at 3 ("[T]he positivists had mistaken ideas . . . about the solutions to certain prominent problems."); ROUSE, *supra* note 10, at 1 (referring to "widely perceived failures of the positivist/empiricist program").

36. *See* Callebaut, *supra* note 9, at 11-13.

37. *See* BERNSTEIN, *supra* note 10, at 16.

surement, and so forth.[38] Whether those limitations are viewed as accidental and tentative limitations to be overcome in science or as constitutive and inevitable parts of science roughly determines the way science will be defined. That is what the so-called "science wars" are about. On one side are those who challenge the notion that science represents nature, including historians who document paradigm shifts (and imply that contemporary science is "just another paradigm" to be overthrown), sociologists who trace social determinants of scientific progress, anthropologists who view science as "just another" cultural practice, and literary scholars who emphasize the rhetorical and textual aspects of science. Each of these views tends to denigrate the role of nature. On the other side are those scientifically-minded who are threatened by such studies and who argue that the historical, cultural, and linguistic aspects of science are secondary and do not prevent access to nature.[39]

As we have emphasized, the most interesting developments in the history, philosophy, and sociology of science involve accounts of how science is both naturalistic, or at least representational of natural reality, and social. Unfortunately, however, all we have are competing accounts. The best represent careful consideration of how scientific judgments arise out of and are dependent upon a history of rhetorical and experimental conventions, scientific communities, evolving technologies of measurement and instrumentation, and the biases and cognitive capacities of individual scientists. Accounts that fail to consider the influence of such "factors" often are considered reductionistic nowadays, but such factors can be acknowledged and then viewed as "internal" to science to avoid the implication that scientific inquiry is driven by social, not natural, phenomena. Accounts that fail to consider the naturalistic or representational aspect of science are likewise considered reductionistic nowadays because the social factors are viewed as "external" determinants of science. But science can be conceived as a co-production of social and natural forces to avoid the implication that science has nothing to do with natural reality. It is sometimes difficult to see how a "co-production" theory is different from the acknowledgment of "internal" social factors. Both are efforts to avoid reductionism, but in the former the distinction between unavoidable internal practices and avoidable external influences breaks down. Both, however, explain the limitations and uncertainties of science in response to concerns about how science might be characterized.

In the next section, we discuss how *Daubert* and its progeny reveal several different judicial perspectives on the philosophy of science.

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38. GIERE, *supra* note 14, at 1.

39. *See* Daniel Lee Kleinman, *Why Science and Scientists Are Under Fire – and How the Profession Needs to Respond*, CHRON. HIGHER EDUC., Sept. 29, 1995, at B1-2.

III. Two Unavoidable Things to Avoid

> But I do not think [the gatekeeping responsibility] imposes on [judges] either the obligation or the authority to become amateur scientists . . . .[40]

> To reach this conclusion [judges] do not have to become philosophers of science and set forth the necessary and sufficient conditions of "real" science.[41]

Justice Rehnquist's concern in *Daubert* about amateur judicial scientists has been eclipsed by the necessities of *Daubert* hearings on admissibility of expert scientific testimony.

> While meticulous *Daubert* inquiries may bring judges under criticism for donning white coats and making determinations that are outside their field of expertise, the Supreme Court has obviously deemed this less objectionable than dumping a barrage of questionable scientific evidence on a jury, who would likely be even less equipped than the judge . . . .[42]

Even on remand, in the Ninth Circuit Court of Appeals's second *Daubert* opinion, Judge Kozinski took a "deep breath" and proceeded with the "heady task" of resolving "disputes among respected, well-credentialed scientists about matters clearly within their expertise."[43] Judges "who do not have a scientific background (and most do not)" must "do the best they can" to decide "whether a scientist's testimony is real science or not."[44] Indeed, the first impression that arises after reading a stack of lengthy post-*Daubert* federal court opinions is that judges take seriously their gatekeeping role. Rather than appearing baffled, judges demonstrate new-found familiarity with scientific evidence from various fields.[45] Disagreements, of course, occasionally arise as appellate panels produce divided opinions[46] or find that a trial judge was too restrictive[47]

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40. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 600-01 (1993) (Rehnquist, J., concurring in part and dissenting in part).

41. Rosen v. Ciba-Geigy Corp., 78 F.3d 316, 318 (7th Cir. 1996).

42. Allison v. McGhan Med. Corp., 184 F.3d 1300, 1310 (11th Cir. 1999).

43. Daubert v. Merrell Dow Pharms., Inc., 43 F.3d 1311, 1316 (9th Cir. 1995).

44. *Rosen*, 78 F.3d at 318.

45. *Daubert* requires judges to come to terms with the scientific method. In the past, "[t]he principal failing in the legal system's approach to scientific evidence has been courts' unwillingness to grapple with the basics of the scientific method." David L. Faigman et al., *Check Your Crystal Ball at the Courthouse Door, Please: Exploring the Past, Understanding the Present, and Worrying About the Future of Scientific Evidence*, 15 CARDOZO L. REV. 1799, 1823 (1994).

46. *See, e.g.*, Greenwell v. Boatwright, 184 F.3d 492, 449 (6th Cir. 1999).

47. *See, e.g.*, *In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 760 (3d Cir. 1994) (reversing trial court's decision); Ambrosini v. Labarraque, 101 F.3d 129, 131 (D.C. Cir. 1996) (same).

or too lenient.[48] White coats are donned nevertheless with regularity and even with some success.

On the other hand, it is not so clear whether judges should don the "tweed" coat of the philosopher of science. Judge Posner thinks not:

> When the Supreme Court in *Daubert* told judges to distinguish between real and courtroom science, it was not with the object of discovering the essence of "science," if there is such an essence. The object . . . was to make sure that when scientists testify in court they adhere to the same standards of intellectual rigor that are demanded in their professional work.[49]

And yet, when Judge Posner goes on to say that such scientists' evidence is "admissible even if the particular methods they have used in arriving at their opinion are not yet accepted as canonical,"[50] the vague contours of a philosophy of science begin to appear. Posner has already distinguished between real "mainstream" or "canonical" science, real "novel" science based on unique methods, and courtroom or "bad" science. While Posner has perhaps not become a philosopher of science because he is concerned only with evaluating science in the context of litigation, he is reflecting on science in abstraction from the particulars of the case before him in order to establish a framework for his evidentiary assessment. That is what Justice Blackmun was doing in *Daubert*, and as numerous commentators have pointed out, he was "doing" philosophy of science.[51]

In what sense, then, can judges avoid a philosophy of science? One possible answer is that, by following *Daubert*, judges are adopting its philosophy of science. For various reasons, however, *Daubert* serves badly as a philosophical account of scientific activity. Depending on how one reads *Daubert*, one might conclude that there is no clear philosophy of science in the opinion,[52] or that the philosophy of science presented is not a good one,[53] or even that two conflicting philosophies of science are in play, rendering the philosophical contribution incoherent.[54] Our view is that the *Daubert* guidelines and general observations about science are ambiguous enough to permit

48. *See, e.g.*, Brock v. Caterpillar, Inc., 94 F.3d 220, 225 (6th Cir. 1996) (reversing trial court's decision).

49. Rosen v. Ciba-Geigy Corp., 78 F.3d 316, 318 (7th Cir. 1996).

50. *Id.*

51. *See, e.g.*, Farrell, *supra* note 17, at 2185 ("[T]wo generalized epistemological positions . . . in *Daubert*."); Imwinkelried, *supra* note 7, at 65 (noting Justice Blackmun's adoption of modern philosophy of science).

52. This is our view, as explained *infra* Part III.

53. *See, e.g.*, Mansfield, *supra* note 17, at 12.

54. *See, e.g.*, Farrell, *supra* note 17, at 2198, 2207 (discussing incoherent adoption of two "world views").

various philosophical positions on science and that those positions are represented in various post-*Daubert* federal court opinions. Moreover, we believe that taking a philosophical position, whether or not acknowledged or explicated, is inevitable.

To say that judges do not have to become philosophers of science following *Daubert* is a bit like saying a federal judge can avoid becoming a constitutional theorist. While it is certainly true that a judge may not be interested in all of the scholarly debates about constitutional interpretation, it would be naive to presume that a judge can avoid taking a position on how to read the Constitution. When a judge decides a constitutional case without identifying himself or herself as belonging to a particular school of constitutional theory, commentators stand ready to classify the judge's approach. Similarly, while there is virtually no discussion about the debates in the history and philosophy of science in post-*Daubert* opinions concerning expert testimony, judges cannot avoid adopting a perspective on scientific activity. There is a difference, for example, between a judge who emphasizes taking a "hard look" at the "testimony of paid experts" to "detect and exclude junk science" that "may creep into the courtroom"[55] and a judge who stresses that a gatekeeper is not an "armed guard." For the latter,

> *Daubert* neither requires nor empowers trial courts to determine which of several competing scientific theories has the best provenance. It demands only that the proponent of the evidence show that the expert's conclusion has been arrived at in a scientifically sound and methodologically reliable fashion.[56]

Both perspectives find quotational support in *Daubert,* but the *Daubert* guidelines are framed differently.

Our argument – that a judge's perspective on science will affect his or her decisions on admissibility of scientific testimony – will seem to many readers uncontroversial. After all, the notion that judges are not objective is commonplace. Consider the recently published book review sarcastically entitled *News Flash: Judges are Ideologically Motivated.*[57] The scientific enterprise, however, is supposed to provide stable insights for law, rendering a judge's philosophical or ideological inclinations largely irrelevant to his or her evaluation (under *Daubert)* of scientific reliability.[58] Nevertheless, a

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55. Greenwell v. Boatwright, 184 F.3d 492, 501 (6th Cir. 1999) (Merritt, J., dissenting).

56. Ruiz-Troche v. Pepsi Cola of P.R. Bottling Co., 161 F.3d 77, 85 (1st Cir. 1998).

57. *See generally* Robert M. Griffin, *News Flash: Judges Are Ideologically Motivated,* 26 AM. J. CRIM. L. 395 (1999) (reviewing SUSAN U. PHILIPS, IDEOLOGY IN THE LANGUAGE OF JUDGES: HOW JUDGES PRACTICE LAW, POLITICS, AND COURTROOM CONTROL (1999)).

58. *But see* Redding & Reppucci, *supra* note 11 (finding trial judges' evidentiary rulings on socially controversial evidence biased by their own sociopolitical opinions).

position on science is unavoidable, and judicial assessments of scientific reliability after *Daubert* reflect influential philosophies of science.

To understand how a court's use of *Daubert* reveals a philosophical perspective on science, we might first emphasize that interdisciplinary borrowings from philosophy are characterized best as perspectives or beliefs, not as stable knowledge. With only slight exaggeration, D.M. Armstrong has remarked that "[p]hilosophy . . . contains no *knowledge* at all" and is more like religion than commonsense or science.[59] When taking a philosophical position, one needs to

> remember that people who are just as bright as us, in some instances brighter, have studied the same material, considered the same arguments, and have come to the opposite conclusion to us. This should give us epistemic pause.
>
> . . . [While] I believe that in philosophy we should all "fight for our corner" and argue vigorously for our point of view, we should also practice a moderate and mitigated scepticism and remain conscious of the more or less serious difficulties our own position is sure to face.[60]

Turning to the philosophy of science, conceived as the effort to give foundational accounts of scientific inquiry and practice, Armstrong's deflationary modesty seems appropriate. There is no single philosophy of science to be understood and adopted, but rather a choice of positions, each representing a different account of science. Joseph Rouse, for example, identified four major traditions in 20th Century philosophical discussions of science, including: (1) the logical empiricist and Popperian program, (2) historical rationalism, (3) scientific realism, and (4) social constructivism.[61] Ronald Giere likewise

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59. D.M. Armstrong, *A Naturalist Program: Epistemology and Ontology*, 73 PROCEEDINGS & ADDRESSES OF THE APA, No. 2, 77, 79 (1999) (containing text of 11th Annual Romanell Lecture on Philosophical Naturalism).

60. *Id.* at 82, 89.

61. ROUSE, *supra* note 10, at 8. Rouse stated that, "[f]or the logical positivists and for Popper, . . . the legitimating features of scientific knowledge were to provide a principled demarcation of the boundaries of science and of empirical knowledge more generally." *Id.* He continued:

> Their successors . . . still frame global arguments to address questions of legitimation. Scientific realists . . . argue that . . . the best explanation for the instrumental success of [a mature science's] theory-dependant methods is the approximate truth of its theories . . . . The historical rationalist tradition may seem to move away from such global interpretations of science . . . . But [many] still develop general historiographical frameworks that situate the history of any particular scientific discipline . . . in order to assess the legitimacy of its achievements . . . . [Social constructivists contend that] all scientific beliefs must be accounted for by *social* factors, [and that] any adequate interpretation of scientific knowledge claims must be neutral with respect to their epistemic or political legitimacy and hence to that extent [are] committed to some form of epistemic relativism.

Id. at 8-9.

contrasted: (1) logical empiricism and functionalist sociology of science, (2) Kuhnian paradigm theory, (3) post-Kuhnian historical schools and views of science as a social construct, (4) evolutionary models, and (5) models based on cognitive mechanisms.[62] Significantly, Rouse and Giere each identified these traditions as a prelude to his own view, which is not quite captured in these traditions. Rouse's cultural studies approach can be seen as a revision of social constructivism, while Giere's view can be seen as a reaction against social constructivism. Numerous other accounts compete to summarize the complex debates in the history, philosophy, and sociology of science,[63] but we only wish to emphasize that there are various positions in the field and that all of them have contemporary adherents.

IV. Philosophies of Science in Daubert

Whenever judges begin to talk about the features of scientific validity, they will inevitably begin to stake out a position among the various philosophies of science that attempt to give a compelling account of how science works. We might summarize the last forty years in the history, philosophy, and sociology of science as a series of acknowledgments of the "social" aspects of science, together with a series of reactions against what are perceived to be excesses of the "social turn," both in contrast to the naive under-

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62. *See* GIERE, *supra* note 14, at 33 (stating that for logical empiricists, science has properties of being both "*representational* and *rational*" and leaving "an unbridgeable gap between the content and methods of science and all other aspects, such as psychology of scientists or their social organization"); *id.* at 33-34 (stating that Robert Merton's functionalist sociology of science took "for granted that the function of science is to produce 'certified knowledge,'" when he "renounced any role for the sociologist in analyzing the *content* of this knowledge or the *methods* by which it became certified;" sociologist's role is "complimentary to that of the logical empiricist philosopher of science"); *id.* at 37 (stating that Kuhn explained that at "any point in a stage of normal science there will always be anomalies" that can lead to crisis, which can then lead "to the proliferation of new approaches" and revolutionary overthrow of old paradigm in favor of "new" normal science); *id.* at 41 (following Kuhn, "there arose within the philosophy of science a 'historical school' [that] retained the earlier philosophical goal of showing how the development of science could be, if not progressing toward truth, at least objectively 'rational'"); *id.* at 43 (stating that alternative to Merton has developed in sociology which "portrays science as being nonrepresentational, nonrational, and lacking any fundamental separation between content and methods, and social structure"); *id.* at 46, 54 (stating that for evolutionary models of science, theories are "embodied in people and their artifacts, both abstract and material," thus scientists, not theories, are "basic individuals in a theory of science;" individual scientists, together with their ideas, are "selected from a population exhibiting considerable variation"); *id.* at 49 (stating that models based on cognitive mechanisms supplement evolutionary models by accounting for "the mechanisms underlying the analogous processes of variation, selection, and transmission").

63. *See generally* GALISON, *supra* note 18 (discussing history of physics and various views as to how theories emerge from laboratories); POTTER, *supra* note 18 (discussing different accounts of science).

standing of science as an exclusively empirical, experiential, or observational affair. We can further identify two extreme positions regarding science that are, for purposes of understanding the use of science in law, basically irrelevant. First, the notions that science is made up of indubitable "knowledge of the immediately given,"[64] reports of experience in "an unbroken, cumulative language of observation,"[65] or appeals to "direct sensory experience – and nothing else"[66] are generally viewed as the outdated fantasy of logical positivism. Neither *Daubert* nor the more influential briefs submitted in that case relied on such a picture of science. Second, the interesting notion that science is "totally *constituted* by human interests and interactions"[67] and involves no "causal interaction with the world"[68] is of little use to courts evaluating scientific validity. In between those extremes lies the complicated project of identifying the "social" aspects of science, which includes all non-empirical or non-observational features of science whether related to the presuppositions and capacities of individual scientists or to the values and practices of scientific communities and institutions.

The most obvious and uncontroversial "social" factor is the influence of theoretical presuppositions on perception, selection and design of experiments, and data collection and analysis.[69] Other factors include, with some

64. PETER GALISON, IMAGE AND LOGIC: A MATERIAL CULTURE OF MICROPHYSICS 784 (1997).

65. *Id.*

66. GIERE, *supra* note 14, at 32.

67. *Id.* at 43.

68. *Id.* at 45.

69. *See generally* Richard E. Redding, *Reconstructing Science Through Law*, 23 S. ILL. U. L.J. 585 (1999). Scientists are by no means immune from being swayed by their own passions and personal and political agendas. According to Michael Rein:

> The scientist is often saliently illogical in his work, particularly when he is defending a preferred view or attacking a rival one; [i]n his experimental research, he is often selective, expedient, and not immune to distorting the data; [t]he scientist is probably the most passionate of the professionals; his theoretical and personal biases often color his alleged "openness" to the data. . . . He is not the paragon of humility or disinterest but is, instead, often a selfish, ambitious, and petulant defender of personal recognition and territoriality; [f]ar from being a "suspender of judgment," the scientist is often an impetuous truth spinner who rushes to hypotheses and theories long before the data would warrant.

MICHAEL REIN, SOCIAL SCIENCE AND PUBLIC POLICY 6 (1976). These assertions are supported by the following observations of scientists at work:

> A senior investigator insists that her assistants use theory X rather than theory Y to interpret their findings. . . . Investigators demonstrate the weaknesses in theory and method of all those who oppose their position, but do not admit their own shortcomings. . . . Investigators cite favorably those who were likely to review their work for publication, hoping to increase the chances that their own work will be published – that is, viewed as "accepted truth."

overlap, the cognitive capacities of individual scientists, the training, indoctrination, and professionalization of scientists, political and personal values and interests, rhetorical conventions and negotiation techniques, availability of material resources, institutional support and restraints, and even cultural and gender bias.[70] The empirical and observational activities of science are thus influenced, contextualized, or limited by these psychological, institutional, microsocial, and macrosocial mechanisms. The degree to which social factors interfere with empirical science or, in the converse formulation, the degree to which the natural world resists scientific theory and practice, is a matter of never-ending debate in the history, philosophy, and sociology of science.

If we eliminate the extreme positions in the debate as overreactions – (1) those who "imply that science is beyond politics and values, that it is an enterprise driven solely by norms of objectivity and rigor" (an obvious overreaction to social critics of science),[71] and (2) those sociologists of science who deny "the individual any role at all" and who hold that "reality has nothing to do with what we say of it" (an obvious overreaction to individualism and realism)[72] – a rich debate remains as to how to characterize science. The distinctions between positions, however, become more nuanced and sophisticated, having less to do with the recognition of the social aspects of science than with the conclusions to be drawn from that recognition. In the end, do you trust science and acknowledge its epistemological superiority, or do you become skeptical and question its status as a producer of "truth" in culture and law?

Consider, for example, the acknowledgments by scientists that science is full of inconsistencies and wrong ideas,[73] that some scientific methods are arbitrary,[74] that the institutions of science are political,[75] that some scientific

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KENNETH J. GERGEN, THE SATURATED SELF: DILEMMAS OF IDENTITY IN CONTEMPORARY LIFE 93 (1991) (citing BRUNO LATOUR & STEVE WOOLGAR, LABORATORY LIFE: THE SOCIAL CONSTRUCTION OF SCIENTIFIC FACTS (1979)). For a defense of scientific progress and an explanation of its successes in knowledge production notwithstanding interested actors, see HULL, *supra* note 14, at 354-96.

70. *See supra* note 69.

71. Kleinman, *supra* note 39, at B2.

72. Barry Barnes, *How Not to Do the Sociology of Knowledge*, *in* RETHINKING OBJECTIVITY 28, 31 (Allan Megill ed., 1994).

73. *See* Boyd, *supra* note 28, at F8 (acknowledging science is full of wrong ideas).

74. *See* Michael Scriven, *Methods of Reasoning and Justification in Social Science and Law*, 23 J. LEGAL EDUC. 189, 197-99 (1970) (arguing that scientific methods often are arbitrary and that science and law use equally arbitrary methods of reasoning). The rules of statistical significance provide a good example of the arbitrariness in scientific discourse. *See generally* Sandra Scarr, *Rules of Evidence: A Larger Context for the Statistical Debate*, 8 PSYCHOL. SCI. 16 (1997) (stating that scientific institutions are political).

75. *See* HESS, *supra* note 5, at 2 ("Scientists have come to recognize the political nature of the institutions of science, and their research problems have become increasingly tied to public and private agendas outside their disciplines."). *See generally* THEODORE M. PORTER,

studies are deceptive,[76] that science cannot exist without a "gatekeeping" reward system and authority structure,[77] and that some of our leading scientists are wrong.[78] For some, these acknowledgments are not challenges to the superiority of science, but mere bumps in the road. Wrong ideas beget right ideas, institutional bias can be eliminated, deceptive studies can be identified, "gatekeeping" produces good science, and disagreements are healthy. For others, these acknowledgments confirm that science is always a cultural activity,[79] that agendas are "hidden behind a rhetoric of objectivity,"[80] and that scientific certainty rests problematically on a network of individual and social interests, powerful elites, and a trusting public.[81] In the former view, lawyers should pay attention to mainstream science and let it control our courts. In the latter view, law needs to control science.[82]

TRUST IN NUMBERS: THE PURSUIT OF OBJECTIVITY IN SCIENCE AND PUBLIC LIFE 211-12 (1995) (stating scientific institutions are political).

76. *See* Daniel S. Greenberg, *Turning Science into Gold*, WASH. POST, Nov. 30, 1999, at A29 ("Journal of the American Medical Association . . . concluded that favorable results [of a series of drug tests] were puffed up through repetitions in various publications, while negative information was played down or ignored.").

77. *See* David Goodstein & James Woodward, *Inside Science*, AM. SCHOLAR, Autumn 1999, at 90 (concluding science aims to discover truths about world and that reward system and authority structure serve to produce useful results).

78. *See, e.g.*, Robert Wright, *The Accidental Creationist*, NEW YORKER, Dec. 13, 1999, at 56-65 (criticizing Stephen Jay Gould's evolutionary theory as wrong and as unwittingly helpful to creationists).

79. *See* Richard Levins, *Ten Propositions on Science and Antiscience*, 46-47 SOCIAL TEXT 101, 103-04 (1996) (describing science's dual nature). Levins proposed that

> [s]cience has a dual nature. On the one hand, it really does enlighten us about our interactions with the rest of the world, producing understanding and guiding our actions . . . .
>
> On the other hand, as a product of human activity, science reflects the conditions of its production and the viewpoints of its producers or owners. The agenda of science, the recruitment and training of some and the exclusion of others from being scientists, the strategies of research, the physical instruments of investigation, the intellectual framework in which problems are formulated and results interpreted, the criteria for a successful solution to a problem, and the conditions of application of scientific results are all very much a product of the history of the sciences and associated technologies and of the societies that form and own them. The pattern of knowledge and ignorance in science is not dictated by nature but is structured by interest and belief.

Id. See generally SCIENCE AS PRACTICE AND CULTURE (Andrew Pickering ed., 1992).

80. HESS, *supra* note 5, at 1; *see also* D. McCloskey, *The Essential Rhetoric of Law, Literature, and Liberty*, 5 CRITICAL REV. 203, 219 (1991) ("The legal and literary style of reasoning is already how scientists argue. The official descriptions and surface rhetoric of science obscure the fact, but fact it is.").

81. *See* BRUNO LATOUR, WE HAVE NEVER BEEN MODERN 1-9 (C. Porter trans., 1993) (arguing that scientific facts are simultaneously real, collective, and discursive).

82. *See generally* Richard E. Redding, *How Common-Sense Psychology Can Inform Law and Psycholegal Research*, 5 U. CHI. L. SCH. ROUNDTABLE 107 (1998); Redding, *supra* note 69,

We might now ask what position the *Daubert* Court took in this debate. Did the Court acknowledge the social aspects of science, and to what degree? Just as important, what is the picture of science that develops in lower federal courts following *Daubert*? Re-reading *Daubert* with these questions in mind, the opinion can be divided into three phases. First, the Court explained that the district court granted respondent Merrell Dow's motion for summary judgment based on Dr. Lamm's review of "more than 30 published studies involving over 130,000 patients" and on his conclusion that no study showed Bendectin could cause birth defects.[83] Petitioner's own eight experts concluded otherwise, but their testimony was found not to be admissible because it was not "sufficiently established to have general acceptance in the field."[84] Petitioner's experts' analyses and re-analyses, the district court found, had not been published or subjected to peer review, and the Ninth Circuit Court of Appeals agreed that expert opinion that diverges "significantly from the procedures accepted by recognized activities in the field" is not generally accepted as reliable.[85] Relying on *Frye v. United States*,[86] the court of appeals found unpublished re-analyses "particularly problematic in light of the massive weight of the original published studies."[87]

The second phase of the opinion rejects *Frye* in favor of the Federal Rules of Evidence, emphasizing Rule 402's liberal standard of relevance and the absence of "general acceptance" as a standard in Rule 702.[88] The Court highlighted the separation of science from law by mentioning law's reliance on "lawyer adversaries and sensible triers of fact to evaluate conflicts."[89] As if that standard might let anything in, the Court confirmed that judges are to screen evidence to ensure it is reliable, methodologically sound, and based on good grounds.[90] Even in this second phase of the opinion, the Court began to

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at 598-601 (arguing that common-sense psychology inherent in law can inform social science, and providing examples where social science has lagged law's behavioral assumptions). Law often influences the course of scientific progress. *See* Mike Redmayne, *Expert Evidence and Scientific Disagreement*, 30 U.C. DAVIS L. REV. 1027, 1071-75 (1997) (providing examples showing how litigation involving scientific issues prompts additional scientific research and controversy).

83. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 582-83 (1993).

84. *Id.* at 583 (quoting Daubert v. Merrell Dow Pharms., Inc., 727 F. Supp. 570, 572 (S.D. Cal. 1989) (quoting United States v. Kilgus, 571 F.2d 508, 510 (9th Cir. 1978))).

85. *Id.* at 584 (quoting Daubert v. Merrell Dow Pharms., Inc., 951 F.2d 1128, 1130 (9th Cir. 1991) (quoting United States v. Solomon, 753 F.2d 1522, 1526 (9th Cir. 1985))).

86. *See* Frye v. United States, 293 F. 1013, 1014 (1923) (introducing general acceptance test).

87. *Daubert*, 509 U.S. at 584 (quoting *Daubert*, 951 F.2d at 1130).

88. *See id.* at 585-92.

89. *Id.* at 589 (quoting Weinstein, *Rule 702 of the Federal Rules of Evidence Is Sound; It Should Not Be Amended*, 138 F.R.D. 631, 632 (1991)).

90. *See id.* at 589-90.

define and discuss characteristics of the scientific enterprise: "[T]here are no certainties in science" – science does not represent "what is immutably 'true,'" but rather is a "*process* for proposing and refining" theories.[91] The implication, of course, is that some theories may turn out to be wrong, so courts should admit all relevant expert testimony that follows the scientific method, rather than defer to majoritarian views of expertise among scientists as the lower court did in *Daubert*.

The third phase of the opinion[92] defines science in a series of "general observations" that have become the four-part test for admission of scientific evidence: (1) scientific knowledge is testable (or refutable, or falsifiable); (2) it is usually subject to peer review and published, though not always (since some "propositions . . . are too particular, too new, or of too limited interest"); (3) as to particular scientific techniques, the error rate should be considered; and finally, (4) "general acceptance" remains a factor – though not required, a known technique unable to attract support may be viewed with skepticism.[93] Notice that the first and third factors (which involve a definition of science based on the scientific method), testability and an acceptable rate of error, are unqualified and appear to establish a benchmark of scientific validity.

The second and fourth factors, on the other hand, are established as useful but not determinative, confirming that publication in peer-reviewed journals and general acceptance would be inappropriate considerations for novel scientific theories.[94] This flexibility leads to the conclusion that *Daubert* is less deferential to the scientific establishment than *Frye* was even though the Court also acknowledged a concern that judicial screening of evidence "will sanction a stifling and repressive scientific orthodoxy."[95] Instead of stressing that the four factors are less deferential to scientific orthodoxy, the Court distinguished science, which considers multiple hypotheses, from law, which has no time for "conjectures that are probably wrong" and in which "authentic insights and innovations" might be rejected to resolve a dispute quickly.[96] That response to the concern over repressive scientific orthodoxy is more than a bit unsettling because earlier in the opinion the Court characterized the Rules as

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91. *Id.* at 590 (quoting amicus brief of American Association for the Advancement Science, at 7-8, and amicus brief of Nicolas Bloemberger et al., at 9).

92. *See id.* at 592-95.

93. *See id.* at 593-94 (defining science).

94. Note that publication in peer-reviewed journals may be necessary for general acceptance, but it is not sufficient. Publication "does not imply that the scientific community stands behind the article's findings . . . only that three to five scientists judged the method to be competent and the analysis to be interesting." Robert Timothy Reagan, *Relevance, Reliability, and the Validity of Scientific Evidence*, 52 OKLA. L. REV. 291, 300 (1999).

95. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 596 (1993).

96. *Id.* at 597.

liberal and science as uncertain. The adversary system was supposed to be the safeguard.[97] In concluding, the *Daubert* Court implied that novel views are probably wrong,[98] without realizing that novel views often may appear to be wrong when compared to mainstream science – that's what makes a view "novel."[99] But even novel science must conform to minimally acceptable standards and practices of the scientific method.

In addition to the four *Daubert* factors – two unqualified (testability, rate of error) and two qualified (publication, general acceptance) – the development of *Daubert* in subsequent federal court opinions subtly reveals three interpretive principles that function inconsistently to modify *Daubert* standards of admissibility. The three principles, which implicitly reflect judicial philosophies of science, can be termed (1) the flexibility principle, (2) the methodology/conclusions distinction, and (3) the acknowledgment of conflicting admissible scientific testimony. Courts following *Daubert* have manipulated these principles to reveal that *Daubert* can be read more liberally and generously, or more conservatively and restrictively, with respect to admissibility of scientific testimony. The conservative approach is characterized by: (1) making the four factors less flexible (e.g., using all four as determinative), (2) blurring the methodology/conclusions distinction, and (3) failing to acknowledge the possibility of conflicting, admissible scientific testimony. Conversely, the more liberal approach is characterized by: (1) emphasizing the flexibility, non-exclusiveness, and even the potential non-applicability of the four factors, (2) maintaining a strong methodology/conclusions distinction, and (3) acknowledging the possibility of conflicting, admissible scientific testimony. After illustrating the varied operations of the three principles (which may usefully inform practitioners about the rhetoric employed by judges and the terms on which they must argue for a narrow versus liberal application of *Daubert*), we will show that the more liberal approach is consistent with contemporary insights from the history, philosophy, and sociology of science, while the more conservative approach reflects an outdated vision of the scientific enterprise.

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97. *See id.* at 589, 596.

98. *See id.* at 597 ("The scientific project is advanced by broad and wide-ranging consideration of a multitude of hypotheses, for those that are incorrect will eventually be shown to be so . . . . Conjectures that are probably wrong are of little use, however, in the project of reaching a quick, final, and binding legal judgment . . . .").

99. *See* Koukoutochos, *supra* note 17, at 2252 (pointing out that Court confused "the admissibility of evidence with the sufficiency of that evidence on the merits"). Koukoutochos explained: "To say that exclusion from the courtroom is acceptable because an expert scientific opinion is 'probably wrong' is to beg the question: how can a judge know, at the threshold, without considering all the evidence, *which* opinions 'are probably wrong?'" *Id.*

A. Flexibility: "Features" of Science Become "Factors" to Consider

First, the flexibility of the four *Daubert* factors was acknowledged in *Daubert* itself, both in the text – the "inquiry envisioned . . . is . . . a flexible one" – and in a footnote:

> A number of authorities have presented variations on the reliability approach, each with its own slightly different set of factors . . . . To the extent that they focus on the reliability of evidence as ensured by the scientific validity of its underlying principles, all these versions may well have merit, although we express no opinion regarding any of their particular details.[100]

Flexibility regarding the test or factors to be used, of course, does not imply that certain factors can be ignored. In *Belofsky v. General Electric Co.*,[101] the district judge read *Daubert* as suggesting that "a district court should take" the four factors into account when evaluating scientific testimony, but also noted that *United States v. Downing*,[102] a Third Circuit Court of Appeals decision, listed similar but different factors.[103] The judge concluded that a "district court should take into account all of the factors listed by either *Daubert* or *Downing* as well as any others that are relevant."[104] However, the United States Supreme Court, in *Kumho Tire Co. v. Carmichael*,[105] recently reemphasized the flexibility of the *Daubert* factors:

> [A] trial judge must have considerable leeway in deciding in a particular case how to go about determining whether particular expert testimony is reliable. That is to say, a trial court should consider the specific factors identified in *Daubert* where they are reasonable measures of the reliability of expert testimony.[106]

To the extent that the *Daubert* factors describe the methodologies of the "hard" sciences, but not the methodology of experts in clinical practice or nonscientific testimony, this flexibility makes sense. However, the Court's failure to understand the difficulty in applying the *Daubert* guidelines (which are

100. *Daubert*, 509 U.S. at 594-95 n.12.

101. 980 F. Supp. 818 (1997).

102. 733 F.2d 1224 (3d Cir. 1985).

103. *See* Belofsky v. General Elec. Co., 980 F. Supp. 818, 821 n.2 (1997) (citing United States v. Downing, 753 F.2d 1224, 1238-39 (3d Cir. 1985)).

104. *Id.* (citing *In re* Paoli R.R. Yard PBC Litig., 35 F.3d 717, 742 (3d Cir. 1994)).

105. 526 U.S. 137 (1999).

106. Kumho Tire Co. v. Carmichael, 526 U.S. 137, 152 (1999). Significantly, the flexibility emphasized in *Kumho Tire* related to an engineer's testimony and responded to the question (raised in the wake of *Daubert*) of whether the four *Daubert* factors applied to nonscientific testimony. Nevertheless, the flexibility emphasized in *Kumho Tire* has not been limited to nonscientific testimony cases, as the discussion following indicates.

based on the paradigm of the scientific method) to non-scientific testimony[107] further betrays an unwillingness to squarely face the question of what counts as science, and whether we can "scientize" all expert testimony by transferring principles of scientific methodology to non-scientific disciplines.

Following *Kumho Tire*, federal courts reiterate with regularity this new level of flexibility, returning to the *Daubert* factors as a "helpful, not determinative," tool or framework,[108] emphasizing that a judge "may consider one or more of the *Daubert* factors when doing so will help determine the expert's reliability,"[109] and observing that "the *Daubert* factors may not all apply even to the admissibility of pure scientific testimony."[110]

The abuse of discretion standard of review for expert testimony evaluations, confirmed in *General Electric Co. v. Joiner*[111] as appropriate,[112] highlights the flexibility of *Daubert* assessments.[113] Indeed, Justice Stevens in his *Joiner* dissent pointed out that while the majority found no abuse of discretion in disallowing the testimony of *Joiner* experts, "the court has not held that it would have been an abuse of discretion to admit the expert's testimony."[114] Judge Bright, in his dissent in *United States v. Waters*,[115] where the majority approved the exclusion of polygraph evidence without holding a *Daubert* hearing, made the same point by remarking that the court may well have admitted the evidence under *Daubert* standards.[116] The abuse of discretion

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107. *See* K. Issac DeVyver, Comment, *Opening the Door But Keeping the Lights Off:* Kumho Tire Co. v. Carmichael *and the Applicability of the "*Daubert*" Test to Nonscientific Evidence*, 50 CASE W. RES. L. REV. 177, 193-99 (1999) ("The Newtonian method, however, cannot ensure the reliability of nonscientific evidence.").

108. Terran v. Secretary of Health & Human Servs., 195 F.3d 1302, 1316 (Fed. Cir. 1999). The *Daubert* factors are not to be used as a "definitive checklist or test." Kumho Tire Co. v. Carmichael, 526 U.S. 137, 150 (1999) (quoting Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 573, 593 (1993)).

109. United States v. Paul, 175 F.3d 906, 910 (11th Cir. 1999).

110. Black v. Food Lion, Inc., 171 F.3d 308, 311 (5th Cir. 1999) ("There are many kinds of experts and expertise, [and] the *Daubert* inquiry is always fact-specific.").

111. 522 U.S. 136 (1997).

112. *See* General Elec. Co. v. Joiner, 522 U.S. 136, 136 (1997) (stating that applying strict abuse of discretion standard reflects preference for admissibility of evidence under Federal Rules of Evidence).

113. *See* Ruiz-Troche v. Pepsi Cola of P.R. Bottling Co., 161 F.3d 77, 83 (1st Cir. 1998) (stating abuse of discretion standard is not ordinarily "appellant-friendly"); *see also* Randolph N. Jonakait, *The Standard of Appellate Review for Scientific Evidence: Beyond* Joiner *and* Scheffer, 32 U.C. DAVIS L. REV. 289, 294-97, 302-03 (1999) (arguing *Joiner*'s abuse of discretion standard means that appellate courts will defer to judgment of trial courts concerning scientific evidence, resulting in lack of uniformity across jurisdictions in gatekeeping function).

114. *Joiner*, 522 U.S. at 155 (Ginsberg, J., dissenting).

115. 194 F.3d 926 (8th Cir. 1999).

116. *See* United States v. Waters, 194 F.3d 926, 937 (8th Cir. 1999) (Bright, J., dissenting).

standard, which is "not appellant-friendly,"[117] only adds to the flexibility of the four *Daubert* factors for assessing the reliability of scientific testimony.

The four *Daubert* factors and the flexibility of their application emphasized in *Kumho Tire* and *Joiner* do not so much reflect a particular philosophy of science as they allow various philosophies of science to remain in play in federal courts. If a judge is inclined to view science as "*autonomous* from the rest of society in that its legitimacy and authority are grounded in universal principles that transcend any particular social context" (Giere's definition of "enlightenment rationalism"),[118] *Daubert* provides ample aphorisms and observations to support that view. Science is unique among "fields of human inquiry" because of its empirical foundations, the scrutiny of the scientific community ("a component of 'good science'"), and its "exhaustive search for cosmic understanding."[119] Such a judge, in exercising his or her considerable leeway in employing the *Daubert* factors, might follow the hints in *Daubert* that unpublished theories may have undetected "substantive flaws in methodology,"[120] and that theories "'able to attract only minimal support within the community' . . . may properly be viewed with skepticism."[121]

On the other hand, if the judge views science as a social practice characterized not only by its successes but by its uncertainty and limitations, whether due to theoretical presuppositions, selection and design of experiments, professionalization, institutional and personal interests, or availability of material resources, that view also finds support in *Daubert*. There are no certainties in science: "[S]cientific conclusions are subject to perpetual revision;" moreover, publication or general acceptance are not always reliable warrants for "well-grounded but innovative theories."[122] Such a judge may not find the publication and general acceptance "factors" to be "reasonable measures of the reliability of expert testimony."[123] For example, the district court in *Volk v. United States*,[124] hearing an appeal from a conviction entered by a magistrate judge, held that the trial court did not err in failing to hold a *Daubert*

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117. *Ruiz-Troche*, 161 F.3d at 83 (quoting Lussier v. Runyun, 50 F.3d 1103, 1111 (1st Cir. 1995)).

118. GIERE, *supra* note 14, at 57.

119. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593, 597 (1993). *But see* D. Michael Risinger et al., *Brave New "Post-*Daubert *World" – A Reply to Professor Moenssens*, 29 SETON HALL L. REV. 405, 436 n.99 (1998) ("Some courts mistakenly assume that science deals only with . . . theories . . . which are directed at exploring the cosmos . . . and not with more concrete and everyday phenomenon.").

120. *Daubert*, 509 U.S. at 593.

121. *Id.* at 594.

122. *Id.* at 590, 593, 597.

123. Kumho Tire Co. v. Carmichael, 526 U.S. 137, 152 (1999).

124. 57 F. Supp. 2d 888 (N.D. Cal. 1999).

hearing on testimony based on field sobriety tests.[125] The court acknowledged that the *Daubert* factors could be applied, but relied on *Kumho Tire* to conclude that "no mechanistic application of *Daubert*" is required as long as "the record reflects an adequate consideration of the reliability of the testimony before admitting it."[126] In *American Computer Innovators v. Electronic Data Systems*,[127] the district court held a *Daubert* hearing regarding the testimony of an investment consultant, finding the proffered opinion "shaky" but admissible because personal experience, reasoning, and methodology were not "purely speculative [and without] any rational basis."[128] The opinion emphasized that the *Daubert* factors, "[t]he points of inquiry proposed by Justice Blackmun," are only examples and not intended to limit the tools that might be used by a judge to test the reliability of the proposed testimony.[129] Likewise, in *United States v. Paul*,[130] a case involving admissibility of expert handwriting analysis, the Eleventh Circuit Court of Appeals twice referred to the flexibility of the *Daubert* factors, "one or more" of which may be considered but which do not apply "to all experts or in every case."[131] Citing *Kumho Tire*, the court highlighted the "broad latitude" enjoyed by trial judges not only with respect to reliability determinations, but also "when [each] decides how to determine reliability."[132]

Although the above opinions exemplify the flexibility of *Daubert* with respect to "non-scientific" testimony and the *Kumho Tire* guidelines, the same level of flexibility was emphasized in *In re Paoli R.R. Yard PCB Litigation*,[133] decided just after *Daubert*, with respect to clinical medical testimony.[134] The Third Circuit Court of Appeals warned against setting the threshold for admissibility too high in applying *Daubert*:

> [N]o particular combination of techniques chosen by a doctor to assess an individual patient is likely to have been generally accepted. But unlike a methodology used in conducting a scientific study, lack of general acceptance is not a sign of unreliability .... Nor is it likely that the particular combination will have been published and subject to peer review ....[135]

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125. *See* Volk v. United States, 57 F. Supp. 2d 888, 895 (N.D. Cal. 1999).

126. *Id.*

127. 74 F. Supp. 2d 64 (D. Mass. 1999).

128. American Computer Innovators, Inc. v. Electronic Data Sys. Corp., 74 F. Supp. 2d 64, 68-69 (D. Mass. 1999).

129. *Id.* at 67.

130. 175 F.3d 906 (11th Cir. 1999).

131. United States v. Paul, 175 F.3d 906, 910 (11th Cir. 1999).

132. *Id.* (quoting Kumho Tire Co. v. Carmichael, 526 U.S. 137, 142 (1999)).

133. 35 F.3d 717 (3d Cir. 1994).

134. *See In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 758 (3d Cir. 1994).

135. *Id.*

Heller v. Shaw Industries, Inc.,[136] also involving the admissibility of clinical medical testimony in the Third Circuit, confirmed that medical experts need not always rely on published studies:[137]

> To so hold would doom from the outset all cases in which the state of research on the specific ailment or the alleged causal agent was in its early stages, and would effectively resurrect a *Frye*-like bright-line standard... by excluding expert testimony not backed by published (and presumably peer-reviewed) studies.[138]

Without overstating the case – all the *Daubert* or *Downing* factors are to be "considered"[139] in the Third Circuit and the expert testimony in *Heller* was not viewed as admissible[140] – the framing of the *Daubert* factors as optional is significant. Science is vaguely characterized in the above opinions by the tentativeness, uncertainty, and disagreements surrounding its generally accepted conclusions found in its peer-reviewed publications. This picture of science is made more visible when compared to the picture in several other federal court opinions on the admissibility of expert testimony.

In *Koch v. Shell Oil Co.*,[141] for example, the court did not mention flexibility at all, but stated that "the court must consider the [*Daubert*] factors."[142] The court applied each factor and found them to weigh against admissibility.[143] This case is not as striking, however, as those cases in which the *Daubert* factors of general acceptance and publication (qualified in *Daubert* as inapplicable to novel theories) are downplayed in order to deny admissibility. After noting that the four *Daubert* factors "have been authoritatively identified as important," the Federal Circuit Court of Appeals in *Libas, Ltd. v. United States*[144] assumed for the sake of argument that a customs test for classifying fabrics was widely accepted.[145] The court then observed that general acceptance "is an imperfect proxy for reliability."[146] Rather than giving great weight to general acceptance, the court used the flexibility of *Daubert* – "not a rigid formula" – to emphasize the other three factors in

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136. 167 F.3d 146 (3d Cir. 1999).

137. *See* Heller v. Shaw Indus., Inc., 167 F.3d 146, 155 (3d Cir. 1999).

138. *Id.*

139. *In re Paoli*, 35 F.3d at 742.

140. *See Heller*, 167 F.3d at 165.

141. 49 F. Supp. 2d 1262 (D. Kan. 1999).

142. Koch v. Shell Oil Co., 49 F. Supp. 2d 1262, 1268 (D. Kan. 1999) (citing Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 592-93 (1993)).

143. *See id.* at 1268-69 (applying *Daubert* factors).

144. 193 F.3d 1361 (Fed. Cir. 1999).

145. Libas, Ltd. v. United States, 193 F.3d 1361, 1366 (Fed. Cir. 1999).

146. *Id.* at 1368.

Daubert.[147] In *Allison v. McGhan Medical Corp.*,[148] the Eleventh Circuit Court of Appeals approved the trial court's *Daubert* ruling excluding testimony that was based on peer reviewed publications.[149] Quoting *Daubert*, the court stated that publication "is not a sine qua non of admissibility" and "does not necessarily correlate with reliability," a proposition that would seem to lead to liberal admissibility of unpublished science.[150] The court used the aphorism to downplay the expert's conclusions. Moreover, the court observed that appellant could not explain why the studies relied on were different from more established studies,[151] even though other courts have emphasized that testimony cannot be excluded "simply because the conclusion was 'novel.'"[152] In *United States v. Cordoba*,[153] after emphasizing the flexibility of the *Daubert* factors, the court rejected the argument that a testable theory subjected to peer review is admissible; the court then found error rate and general acceptance weaknesses.[154] Similarly, in *Bushore v. Dow Corning-Wright Corp.*,[155] publication of observations that were in conflict with mainstream research did not help because publication does not equate to reliability.[156]

These cases present a particularly interesting vision of science as truth. All of the uncertainty and tentativeness of science emphasized in *Daubert* falls on the side of error and bad science that should not be admitted. The flexibility of the *Daubert* standard allows courts the leeway to downplay certain factors to admit testimony, such as general acceptance or publication, and to enforce strictly the four factors to disallow testimony. But it also permits courts to disallow testimony that is generally accepted or published because those factors do not correlate with reliability.

In short, judges are free to frame *Daubert* as more liberal than *Frye* insofar as novel theories are admissible, or less liberal than *Frye* insofar as general acceptance or publication is insufficient. We will highlight that difference in our discussion of the third principle – the acknowledgment of conflicting admissible scientific testimony. The flexibility principle, however,

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147. *Id.* at 1369.

148. 184 F.3d 1300 (11th Cir. 1996).

149. Allison v. McGhan Med. Corp., 184 F.3d 1300, 1314 (11th Cir. 1996).

150. *Id.* at 1313 (quoting Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593 (1993)).

151. *See id.* at 1314.

152. Heller v. Shaw Indus., Inc., 167 F.3d 146, 153 (3d Cir. 1999).

153. 194 F.3d 1053 (9th Cir. 1999).

154. United States v. Cordoba, 194 F.3d 1053, 1058 (9th Cir. 1999).

155. 1999 WL 1116920 (M.D. Fla. Nov. 15, 1999).

156. *See* Bushore v. Dow Corning-Wright Corp., No. 92-344-CIV-T-26C, 1999 WL 1116920, at *4 (M.D. Fla. Nov. 15, 1999) (stating that poor testing, high rate error, and lack of general acceptance all militated against admissibility).

initiates a framework that permits various perspectives on science to remain in play in federal courts.

B. Conclusions Versus Methodology?

Our second interpretive principle from *Daubert*, the distinction between conclusions and methodology, also functions in post-*Daubert* jurisprudence as a marker of philosophical predispositions. That is, to the extent that a strong distinction is maintained between conclusions and methodology, the likelihood that novel scientific theories will be admissible increases. Conversely, to the extent that the distinction collapses, federal courts can impose a more traditional view of science as a stable enterprise.

The distinction between conclusions and methodology was clear in *Daubert*: "The focus [of the four-factor inquiry], of course, must be solely on principles and methodology, not on the conclusions they generate."[157] Such a limited focus is consistent with the view that novel science may not be generally accepted or published, but will nevertheless be admissible if it rests on sound methodology. Shortly after *Daubert* was decided, however, the court in *In re Paoli* opined that the distinction between methodology and conclusions "has only limited practical import."[158] "When a judge disagrees with the conclusions of an expert, it will generally be because he or she thinks that there is a mistake at some step in the investigative or reasoning process of that expert."[159] Nevertheless, "the judge should not exclude evidence simply because he or she thinks there is a flaw . . . which renders the expert's conclusion incorrect."[160] The judge should exclude evidence only "if the flaw is large enough that the expert lacks 'good grounds' for his or her conclusion."[161] The new distinction in *In re Paoli* is therefore between conclusions based on large methodological flaws and conclusions based on "good grounds." Thus, the *Daubert* conception can be reformulated to say that the focus must be solely on whether there are good methodological grounds and not on the conclusions they generate. This is not so different from *Daubert*, because the only conclusions to be critically examined are those based on flawed methodology, which keeps the *Daubert* distinction alive. Indeed, the court in *In re Paoli* conceded that the distinction "remains of some import . . . to the extent that there will be cases in which a party argues that an expert's testimony is unreliable because the conclusions . . . are different from those of other experts. In such cases, there is no basis for holding the expert's testimony

157. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 595 (1993).

158. *In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 746 (3d Cir. 1994).

159. *Id.*

160. *Id.*

161. *Id.*

inadmissible."[162] The only qualification seems to be that a trial court does not abuse its discretion by examining an expert's conclusions "to determine whether they could reliably follow from . . . the methodology used."[163] The distinction otherwise remains intact.

The *Joiner* Court confirmed this qualification when it observed that "conclusions and methodology are not entirely distinct from one another."[164] The explanation is a bit different – rather than focusing on large methodological flaws to reject certain conclusions, *Joiner* focuses on the size of the "gap" between "the data and the opinion offered."[165] In his *Joiner* dissent, Justice Stevens observed that the distinction between methodology and conclusions began to collapse.[166] The methodology used by Joiner's experts was based on the "weight of the evidence" – taking all of the studies together to formulate a conclusion that is scientifically acceptable.[167] The trial judge, however, "examined the studies one by one and concluded that none was sufficient to show a link between PCBs and lung cancer."[168] If the trial judge had remained faithful to *Daubert*'s rule against "assessing the validity or strength of an expert's conclusions," the opinions of qualified experts reaching "relevant conclusions on the basis of an acceptable methodology" would have been admissible.[169] For Justice Stevens, the trial judge saw too great a gap between the data and the opinion because, contrary to *Daubert*, he focused on the conclusions and not on the methodology used.[170] To say that conclusions and

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162. *Id.* at 746 n.15 (citations omitted).

163. Heller v. Shaw Indus., Inc., 167 F.3d 146, 153 (3d Cir. 1999).

164. General Elec. Co. v. Joiner, 522 U.S. 136, 146 (1997).

165. *Id.*

166. *See id.* at 151 (Stevens, J., dissenting).

167. *Id.*

168. *Id.* (noting that "the Environmental Protection Agency (EPA) uses the same methodology"). *But see* Allen v. Pennsylvania Eng'g Corp., 102 F.3d 194, 198 (5th Cir. 1996) (stating that weight-of-evidence "methodology results from the preventive perspective that agencies adopt in order to reduce public exposure to harmful substances. The agencies' threshold of proof is, reasonably, lower than that appropriate in tort law.").

Grounding admissibility in an assessment of individual studies ignores the possibility that the sum findings of individual studies may be sufficiently valid and reliable, though each study considered alone may have methodological shortcomings or relatively low statistical power to detect certain differences. Meta-analysis is a widely used method in science for increasing the sample size by aggregating results across individual studies. *See generally* META-ANALYSIS FOR EXPLANATION (Thomas D. Cook et al. eds., 1994). *Cf.* Charles Kiesler, *Mental Hospitals and Alternative Care: Noninstitutionalization as Potential Public Policy for Mental Patients*, 37 AM. PSYCHOLOGIST 349 (1982) (evaluating ten studies flawed in different ways but all reaching same conclusion, and concluding that taken together, these studies usefully informed public policy).

169. *Joiner*, 522 U.S. at 154.

170. *See id.* at 153.

methodology "are not entirely distinct" was neither accurate nor helpful in assessing Joiner's experts.[171]

Recent federal court opinions are inconsistent as to how to treat the distinction between methodology and conclusions. In *Cabrera v. Cordis Corp.*,[172] the Ninth Circuit Court of Appeals stated that the test "is not the correctness of the expert's conclusions but the soundness of his methodology."[173] In *National Bank of Commerce v. Associated Milk Producers*,[174] the Eighth Circuit Court of Appeals held that "a district court is not free to choose between the conflicting views of experts whose principles and methodology are reliable and relevant."[175] Likewise, a district court in *Baker v. Indian Prairie Community School District Unit 204*[176] held that the *Daubert* inquiry "should be based 'solely on principles and methodology, not on the conclusions that they generate.'"[177]

In contrast, the Third Circuit Court of Appeals in *In re TMI Litigation*,[178] after quoting *Daubert* on the distinction, went on to quote *Joiner* ("conclusions and methodology are not entirely distinct")[179] and *Heller* (court "must examine the expert's conclusions in order to determine whether they could reliably flow" from the facts and methodology)[180] to conclude that "too great an analytical gap between the data and the opinion preferred" will render the opinion insufficiently reliable to admit.[181] In *Moore v. Ashland Chemical Inc.*,[182] the en banc Fifth Circuit Court of Appeals quoted *Joiner* at length to conflate the conclusions/methodology distinction and then found no error in the trial court's finding of "too great an analytical gap" in the testimony of Dr. Jenkins.[183] The dissent in *Moore* concluded otherwise:

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171. *Id.* at 155.

172. 134 F.3d 1418 (9th Cir. 1998).

173. Cabrera v. Cordis Corp., 134 F.3d 1418, 1421 (9th Cir. 1998) (quoting Daubert v. Merrell Dow Pharms., Inc., 43 F.3d 1311, 1318 (9th Cir. 1995)).

174. 191 F.3d 858 (8th Cir. 1999).

175. National Bank of Commerce v. Associated Milk Producers, Inc., 191 F.3d 858, 862 (8th Cir. 1999).

176. 1999 WL 988799 (N.D. Ill. Oct. 27, 1999).

177. Baker v. Indian Prairie Cmty. Sch. Dist. Unit 204, No. 96 C 3927, 1999 WL 988799, at *3 (N.D. Ill. Oct. 27, 1999) (citations omitted).

178. 193 F.3d 613 (3d Cir. 1999).

179. *In re* TMI Litig., 193 F.3d 613, 665 (3d Cir. 1999) (quoting General Elec. Co. v. Joiner, 522 U.S. 136, 146 (1997)).

180. *Id.* (quoting Heller v. Shaw Indus., Inc., 167 F.3d 146, 153 (3d Cir. 1999)).

181. *Id.* (quoting *Joiner*, 522 U.S. at 146).

182. 151 F.3d 269 (5th Cir. 1998).

183. Moore v. Ashland Chem. Inc., 151 F.3d 269, 277 (5th Cir. 1998) (quoting *Joiner*, 522 U.S. at 146).

> In the present case, there was no "analytical gap" between Dr. Jenkins' data and his opinion .... [The] district court excluded Dr. Jenkins' opinion simply because he did not have any hard scientific support for his clinical medical opinion, not because of a gap in reasoning. Dr. Jenkins' clinical medical opinion was, in fact, snugly based on the sound application of the well accepted methodology of his discipline.[184]

Moore is significant because the en banc court divided over how to apply *Daubert* to clinical medical testimony, such that the division over how seriously to take methodology/conclusion distinction is hidden in the margins.[185]

In *Greenwell v. Boatwright*,[186] however, the division on a Sixth Circuit Court of Appeals panel highlighted the implications of how a court uses the distinction between conclusions and methodology.[187] The majority emphasized that trial courts "are not to be concerned with the reliability of the conclusions generated by valid methods, principles and reasoning."[188] Because the plaintiffs on appeal were challenging the inferences and not the factual basis of an expert's testimony, the testimony could not be excluded.[189] The dissenting judge, however, characterized *Daubert* as a means to detect and to exclude junk science, which requires a careful assessment of "the scientific conclusions and reasoning of experts because jurors are frequently overly impressed by conclusory opinions of an expert paid by a party."[190] Judges "may no longer indulge in this assumption that an expert's conclusions and reasoning can all be corrected by cross-examination as in the past."[191] Judge Merritt collapsed the distinction between conclusions and methodology and even reformulated the endorsement found in *Daubert* of "the traditional means of testing evidence in the adversary system rather than the wholesale exclusion of evidence under an uncompromising test."[192] The "traditional means" are not working, so an uncompromising test may be necessary.

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184. *Id.* at 290 n.8.

185. *See also* United States v. Bighead, 128 F.3d 1329, 1336-37 (9th Cir. 1999) (dividing on how seriously to take methodology/conclusion distinction).

186. 184 F.3d 492 (6th Cir. 1999).

187. *See* Greenwell v. Boatwright, 184 F.3d 492, 497 (6th Cir. 1999).

188. *Id.* (quoting United States v. Bonds, 12 F.3d 540, 555 (6th Cir. 1993)).

189. *See id.*

190. *Id.* at 501 (Merritt, J., dissenting). Recent research, however, casts considerable doubt on the assumption that juries are overly impressed by expert scientific or statistical testimony. *See* Scott E. Sundby, *The Jury as Critic: An Empirical Look at How Capital Juries Perceive Expert and Lay Testimony*, 83 VA. L. REV. 1109, 1133-39 (1997) (reporting on jurors' reaction to expert testimony in capital cases and also citing other studies).

191. *Greenwell*, 184 F.3d at 502.

192. National Bank of Commerce v. Associated Milk Producers, Inc., 191 F.3d 858, 862 (citing Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 596 (1993)).

The strong distinction between conclusions and methodology is based in *Daubert* on the "capabilities of the jury and of the adversary system generally."[193] The strong distinction parallels the warnings in *Daubert* that the four factor test is flexible and that general acceptance and publication are not dispositive because "well grounded but innovative theories" will not meet those tests.[194] The breakdown of that distinction, however, parallels the warning in *Daubert* that "a gatekeeping role for the judge, no matter how flexible, inevitably on occasion will prevent the jury from learning of authentic insights and innovations," because law, unlike science, is not "advanced by broad and wide-ranging consideration of a multitude of hypotheses."[195] When Justice Blackmun spoke of "conjectures that are probably wrong,"[196] the propriety of viewing "a known technique which has been able to attract only minimal support" with skepticism,[197] and the unlikelihood of good, unpublished science,[198] the picture of science changed from a field of competing, well-grounded theories to one of competition in the courts between published, generally accepted science and "all forms of junk science that may creep into the courtroom."[199] These two pictures co-exist in federal courts and are determined in part by whether the distinction between conclusions and methodology is maintained.

C. Good Versus Junk Science, or Good Science in Conflict?

Finally, our third interpretive principle in the wake of *Daubert* is whether and how clearly courts acknowledge the obvious possibility of conflicting but admissible scientific testimony. Like the other two principles, the differences in approach among federal courts is subtle but detectible in how *Daubert* is framed and used as authority. In *Joiner*, for example, Justice Stevens suspected that the trial court, as well as the majority against which he was dissenting, were too anxious to label the testimony of Joiner's experts "junk science" because those experts had used "the same scientific approach" as the petitioners' experts who reached different conclusions.[200] In *In re Paoli*, the court confirmed that a judge's assessment that an expert's opinion is incorrect,

193. *Daubert*, 509 U.S. at 596.

194. *Id.* at 593.

195. *Id.* at 597.

196. *Id.*

197. *Id.* at 594.

198. *See id.* at 593 ("[S]ubmission to the scrutiny of the scientific community is a component of 'good science.'").

199. Greenwell v. Boatwright, 184 F.3d 492, 501 (6th Cir. 1999) ("Trial judges must be on guard against all forms of junk science that may creep into the courtroom.").

200. General Elec. Co. v. Joiner, 522 U.S. 136, 154 (1997).

"that there are better grounds for some alternative conclusion," does not make the opinion inadmissible, because the standard of reliability "is not that high."[201] Thus, some federal courts, like Justice Blackmun in *Daubert*, refer to "shaky but admissible" evidence.[202] Likewise, *Ruiz-Troche v. Pepsi Cola of Puerto Rico Bottling Co.*,[203] concluding that the district court "set the bar too high," held that "*Daubert* neither requires nor empowers trial courts to determine which of several theories has the best provenance."[204] In *Terran v. Secretary of Health and Human Services*,[205] the petitioner suggested that "the *Daubert* framework is narrowly intended to prevent the introduction of 'junk science' into trials," but the court disagreed and characterized *Daubert* as "a broader tool for analyzing the admissibility of scientific testimony."[206] In *National Bank of Commerce v. Associated Milk Producers*, the court reluctantly found no abuse of discretion in the trial judge's exclusions of expert testimony and pointed out that:

> [A] district judge is not to judge the validity of competing conclusions of expert witnesses, but rather to limit its gate-keeping role to determining whether the expert testimony offered by the parties meets the *Daubert* standards.... Moreover, we have cautioned that the district court may not rely solely on the lack of published studies . . . because in some cases scientific propositions may have been too new to have been published or tested.[207]

The *Heller* court, citing *In re Paoli*, also emphasized that a novel conclusion is admissible if based on reliable methodology.[208]

On the other hand, in *Allison v. McGhan Medical Corp.*, the court approved a finding of inadmissibility, noting that the appellant "does not explain why the [the studies relied upon by her experts] should trump more than twenty controlled epidemiological studies."[209] The court found no abuse of discretion in the trial court's "considering that the proffered conclusions in studies with questionable methodologies were out of sync with the conclusions in the over-

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201. *In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 744-45 (3d Cir. 1994).

202. American Computer Innovators, Inc. v. Electronic Data Sys. Corp., 74 F. Supp. 2d 64, 69 (Mass. 1999); Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 596 (1993); Heller v. Shaw Indus., Inc., 167 F.3d 146, 152 (3d Cir. 1999).

203. 161 F.3d 77 (1st Cir. 1998).

204. Ruiz-Troche v. Pepsi Cola of P.R. Bottling Co., 161 F.3d 77, 85 (1st Cir. 1998).

205. 195 F.3d 1302 (Fed. Cir. 1999).

206. Terran v. Secretary of Health & Human Servs., 195 F.3d 1302, 1316 (Fed. Cir. 1999).

207. National Bank of Commerce v. Associated Milk Producers, Inc., 191 F.3d 858, 864 (8th Cir. 1999).

208. *See* Heller v. Shaw Indus., Inc., 167 F.3d 146, 153 (3d Cir. 1999) (citing *In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 746 (3d Cir. 1994)).

209. Allison v. McGhan Med. Corp., 184 F.3d 1300, 1314 (11th Cir. 1999).

whelming majority of the epidemiological studies presented to the court."[210] Of course, in *Allison*, this was only one of many factors used to reject the testimony. However, the willingness to compare a novel view with mainstream or majoritorian science betrays a tendency to identify one side's theory as correct and any conflicting testimony as incorrect. That tendency is reflected in *Lust v. Merrell Dow Pharmaceuticals, Inc.*,[211] in which the court rejected testimony because the expert failed to identify and defend novel results,[212] and in *Conde v. Velsicol Chemical Corp.*,[213] in which the court faulted an expert for failing to explain the grounds for the differences between his view and the collective view of his scientific discipline.[214] Such deference to mainstream science militates against the view that science is uncertain and tentative and therefore that mainstream science is often wrong and replaced by novel views that then become mainstream science. Moreover, although such deference is inconsistent with one "reading" of *Daubert*, it is consistent with another. Courts are free, following *Daubert*, to emphasize the likelihood of good, conflicting science, or to suspect that one side in a conflict is peddling courtroom or "junk" science. These different emphases reflect different visions of science.

The struggles over good versus junk science are most readily apparent in cases involving disputes about the qualifications of scientific experts. In *United States v. Bighead*,[215] decided before *Kumho Tire*, the Ninth Circuit Court of Appeals considered whether *Daubert* guidelines applied to a clinician's testimony concerning the psychological sequelae of child abuse.[216] The testimony was based on experience in interviewing alleged abuse victims. Holding that *Daubert* was inapplicable, the majority characterized the witness's testimony as "specialized knowledge, not scientific knowledge."[217] But a forceful dissent argued that the majority "reads *Daubert* too narrowly," making it impossible to "distinguish[ ] [the expert testimony] from pseudoscience."[218] For the majority, the expert's reliance on clinical experience alone made her testimony non-scientific. The dissent, however, required a *Daubert* review in order to avoid letting pseudoscience into the courtroom: The expert was "not a psychologist and not a psychiatrist"[219] but only a therapist, and "there is a range of education, sensibility, and competence among

210. *Id.* at 1316.
211. 89 F.3d 594 (9th Cir. 1996).
212. Lust v. Merrell Dow Pharms., Inc., 89 F.3d 594, 598 (9th Cir. 1996).
213. 24 F.3d 809 (6th Cir. 1994).
214. Conde v. Velsicol Chem. Corp., 24 F.3d 809, 814 (6th Cir. 1994).
215. 128 F.3d 1329 (9th Cir. 1997).
216. United States v. Bighead, 128 F.3d 1329 (9th Cir. 1997).
217. *Id.* at 1330 (quoting United States v. Cordoba, 104 F.3d 225, 230 (9th Cir. 1997)).
218. *Id.* at 1335.
219. *Id.* at 1336.

such workers."[220] The dissent quoted, with disapproval, one expert's view that "[b]ecause we see it clinically, we see something we believe is real, clinically, and whether or not . . . scientists . . . agree that it is real, most of us have some sort of personal sense that it is."[221] The *Bighead* dissent adopted a good versus junk science stance and characterized the testimony as junk science that must not be allowed through the gates of *Daubert*.[222] For the majority, the testimony was simply not science and was not transformed into junk science merely because the scientific community had a different view.[223]

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220. *Id.* at 1338. This distinction between "hard" science, involving controlled empirical testing, versus "soft" science, involving clinical or experiential data, is frequently conflated with the distinction between science and non-science. Evidence on the battered woman's syndrome is a good example. Viewing such evidence as "unacceptably soft science" because it is derived from unreliable interview and self-report studies and the experience of clinicians, some commentators argue that it should be excluded under *Daubert*. Stephen J. Morse, *The Misbegotten Marriage of Soft Psychology and Bad Law*, 14 LAW & HUM. BEHAV. 595, 595 (1990); David L. Faigman & Amy Wright, *The Battered Woman Syndrome in the Age of Science*, 39 ARIZ. L. REV. 67, 68 (1997) ("In short, in the law's hasty effort to use science to further good policy, it is now obvious that the battered woman syndrome is not good science nor does it generate good policy."). Faigman et al., *supra* note 45, at 1832, stated:

> [I]n a wide range of cases, "scientific" opinions are offered into evidence with little or no empirical research data behind them .... These "clinical judgments" do not qualify as science in the absence of empirical evidence . . . these judgments could be tested for accuracy, but the requisite data are not yet available.

Id.; *see also* Risinger et al., *supra* note 119, at 437 ("A hypothesis that cannot be subjected to empirical testing is a metaphysical proposition that is by definition not part of science.").

Never mind that the testimony of experienced clinicians or researchers – some having interviewed hundreds of battered women – is based on "knowledge, skill, experience, [and] training" as required to qualify as expert testimony under Federal Rule of Evidence 702. *See* Morse, *supra* note 17, at 287 ("[V]oluminous anecdotal research rarely produces more than volumes of anecdotes."). Much of clinical practice is driven by practical experience, not science. *See, e.g.*, Marilee M. Kapsa & Carl B. Meyer, *Scientific Experts: Making Their Testimony More Reliable*, 35 CAL. W. L. REV. 313, 324 (1999) ("[O]nly 15% of the decisions a doctor makes every day are based on evidence.") (citing Nancy Gibbs, *A Week in the Life of a Hospital*, TIME, Oct. 12, 1998, at 68)). Much of the value of social science research comes in debunking common myths (e.g., that battered women stay with the batterer because they are masochistic), and myths are most prevalent when the relevant scientific research is still in its infancy. *See* Morse, *supra* note 17, at 321 (discussing role of myths in scientific research).

221. *Bighead*, 128 F.3d at 1338 (Noonan, J., dissenting) (quoting RICHARD OFSHE & ETHAN WATTERS, MAKING MONSTERS: FALSE MEMORIES, PSYCHOTHERAPY, AND SEXUAL HYSTERIA 177-204, 195 (1994)).

222. *See also* Faigman et al., *supra* note 45, at 18323 (arguing judges should not permit end run around *Daubert* by admitting clinical testimony under Federal Rule of Evidence 702 as "technical or other specialized knowledge"). *Daubert* requires that expert knowledge be testable: "[J]udges' failure to query the empirical basis for clinical judgments would eviscerate the *Daubert* rule. All pseudoscience can be redescribed as specialized knowledge; expert opinion based on the research of Stephen King will be as welcome as that based on the research of Stephen Hawking." *Id.*

223. The science versus non-science distinction has perhaps been the most stark in state

Again, to the extent that courts in their broad discretion are relatively inflexible in their use of the *Daubert* factors, willing to collapse the distinction between conclusions and methodology, and less likely to believe that two conflicting theories may both be good science, a picture of science emerges that is traditional and positivistic. On the other hand, in courts that are flexible regarding the four factors, that maintain a strong conclusions/methodology distinction, and that readily acknowledge the possibility of good, conflicting scientific testimony, a picture emerges that is somewhat more in sync with current views in the history, philosophy, and sociology of science. Both views of science, remarkably, find support in *Daubert* though, as discussed below, the "two views" dichotomy seriously misrepresents the state of affairs when importing into law insights from the history and philosophy of science.

V. Importing Insights About Science

> We mistake... the authentically dogmatic and so legislative character of scientific philosophy, by which I mean the thought implicit in science's thinking about itself, as well as the academic discourse of the philosophy of science .... Can we think of science in terms of its function as a producer or theatre of symbols? And can we imagine that modern science, like the classical religions, implies a *politics of speech* ... ?[224]

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court child abuse cases involving autistic or mentally retarded children, where prosecutors sought to introduce communications from the victims elicited through "facilitated communication." The technique, which ostensibly allows communication through assisted typing, has been tested in a number of controlled experimental studies that not only failed to validate the technique but also found that the supposed communications were often the result of facilitator leading. Every scientific and professional organization on record has stated that the technique is without scientific support. *See* Brian J. Gorman, *Facilitated Communication: Rejected in Science, Accepted in Court–A Case Study and Analysis of the Use of FC Evidence Under* Frye *and* Daubert, 17 BEHAV. SCI. & L. 517, 518-19 (1999) (noting that scientific and professional organizations state technique is without scientific support).

Yet by characterizing the technique not as a scientific method or tool but as a translation much like an interpreter's translation of a foreign language, many courts have admitted testimony obtained through facilitated communication. As in *Bighead*, courts wishing to admit such evidence find comfort in the unscientific nature of the evidence. Courts excluding it find, after a *Frye*- or *Daubert*-type review, the evidence to be inadmissible pseudoscience. As if meaningful empirical testing could be undertaken in the courtroom, one court admitting facilitated communication evidence noted that it need *not* satisfy the criteria for the admissibility of scientific evidence because "the proffered facilitated communication lends itself to empirical rather than scientific proof." *Id.* at 535 (quoting State v. Warden, 891 P.2d 1074, 1087 (Kan. 1995)). Consequently, "under *Daubert*, trial courts can admit scientific evidence falsified and rejected by the scientific community. Additionally, under *Joiner*, 'good' theories of respected scientists can be excluded by the court's own falsification." *Id.* at 540.

224. Pierre Legendre, *Id Efficit, Quod Figurat (It Is the Symbol Which Produces Effects):*

A. Law and Interdisciplinarity

> [W]hat people call interdisciplinary work often involves answering questions in one discipline by applying its disciplinary methods to the materials usually studied by another.... One is always within whatever discipline one is in, and one simply assimilates and feeds information from and about other disciplines into one's pre-existing disciplinary matrix.[225]

Even the most casual reflection on the relations between law and other disciplines confirms a complex phenomenon. Insights from extra-legal disciplines provide bases from which to understand or to criticize or to reform legal institutions and practices. Interdisciplinary legal studies come to mind immediately – law and economics, law and psychology, law and literature, and so forth. The commitment to interdisciplinary approaches can be traced to American legal realism, whose heirs include the empirically-oriented Law and Society movement, the expansive Law and Economics movement, and even Critical Legal Studies[226] (a tradition now dissipated into feminist and critical race scholarship, postmodern legal theory, and various literary and psychoanalytic critiques of law). Beyond the academy, though perhaps not (in the minds of hopeful scholars) beyond its influence, legislatures and courts borrow from, integrate, and institutionalize knowledge from extra-legal disciplines. Legislative committees and trial judges rely on experts from fields relevant to social problems and legal controversies.

Despite the attractiveness of this picture, interdisciplinarity has its risks. Scholars, judges, or lawmakers can misunderstand or fail to appreciate the disagreements and controversies in extra-legal disciplines.[227] Examples of the latter include adopting one version of an historical employment, one school of psychology, one account of textual interpretation, one hypothesis in environmental studies, or one economic paradigm when such a version, school, account, hypothesis, or paradigm is the subject of considerable disagreement within its home field.

Two different types of interdisciplinary analysis are prevalent in legal discussions on law and science. First, the introduction and use of expert

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The Social Constitution of Speech and the Development of the Normative Role of Images, 20 LEGAL STUD. F. 247, 248 (P. Goodrich trans., 1996).

225. J.M. Balkin, *Interdisciplinarity as Colonization*, 53 WASH. & LEE L. REV. 949, 959 (1996).

226. *See* William W. Fisher III et al., *Introduction* to AMERICAN LEGAL REALISM xiii-xiv (W. Fisher et al. eds., 1996).

227. *See generally* Balkin, *supra* note 225 (describing obstacles to interdisciplinary research in law and legal academy); Redding, *supra* note 69 (describing how "law and psychology" research has often failed to be truly interdisciplinary).

scientific testimony in trial courts is an obvious example of interdisciplinary interaction. Some legal controversies require or benefit from scientific knowledge. A second type of interdisciplinary inquiry, initiated in *Daubert* and continued in scholarly and judicial literature concerning *Daubert*, involves the adoption and use of insights from the history, philosophy, and sociology of science, a collection of disciplines concerned with the nature of scientific discovery and progress.[228] The focus of this Article is on this second type of interdisciplinarity, that is, on the legal narratives about science – its history, essence, methodology, and certitude. Legal narratives rely on those extra-legal disciplines for which scientific activity is the object of inquiry.

Much of the discussion in law concerning the history, philosophy, and sociology of science is oversimplified for the sake of clarity and economy of language, and the debates are often not taken seriously. The result is not only a failure to appreciate the interdisciplinary enterprise, but also a failure to adequately address the fundamental question raised in *Daubert* about the nature of science. Legal scholars have made a useful distinction between traditional views of science (as the accumulation of objective knowledge) and new views of science (as culturally embedded) to help readers understand the decades-old debate among historians, philosophers, and sociologists of science.[229] As discussed below, however, the distinction is only a pedagogical starting point for understanding a continuum of positions.

B. Critical Reflections on Science

> A radical is someone who claims that scientific knowledge is entirely constructed "out of" social relations; a progressivist is someone who would say that it is partially constructed out of social relations but that nature somehow "leaks in" at the end. At the other side of this tug-of-war, a reactionary is

228. In an article like this one complaining about oversimplification in interdisciplinary legal research, we concede a certain oversimplification in referring to the "history, philosophy, and sociology of science" as the relevant disciplinary fields. Jonathan Potter recently pointed out that "in the last two decades one of the notable features of the field [of social studies of science] is the wide interdisciplinary collaboration among sociologists, philosophers and historians of science, psychologists, linguists, and literary analysts." POTTER, *supra* note 18, at 17. Bruno Latour, in his discussion of the deadlock in social studies of science, delighted in the "researchers [who] are looking for ways out of the deadlock: in literary theory, biology, cognitive science, cultural history, ethnology, ethnography of skills, moral economics, interactionism, and networks." Bruno Latour, *One More Turn After The Social Turn*, *in* THE SCIENCE STUDIES READER, *supra* note 18, at 276. Because the term "science studies" often has radical connotations, our reference to "the history, philosophy, and sociology of science" is an attempt to capture all traditional and contemporary studies of science discussed in this Article.

229. *See, e.g.*, Farrell, *supra* note 17, at 2189-98 (contrasting positivist view and constructionist view); Imwinkelried, *supra* note 7, at 59-65 (contrasting traditional popular conception of science with modern conception of science).

> someone who would claim science becomes really scientific only when it finally sheds any trace of social construction; while a conservative would say that although science escapes from society there are still factors from society that "leak in" and influence its development. In the middle, would be the marsh of wishy-washy scholars who add a little bit of nature to a little bit of society and shun the two extremes . . . .
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> . . . I claim that the only way to go on with [science studies] is to abandon this frame of reference . . . .[230]

The existing frame of reference, contrary to Bruno Latour's recommendation, is initially a useful representation of the ongoing academic debates about the nature of scientific discovery and progress. Latour's reference to "social relations," "social factors," and "social construction" with respect to the scientific enterprise highlights the problem of how to explain or account for the personal interests of scientists, the interests of scientific institutions or communities, the effects of scientific training, and the experimental and linguistic conventions of science. If one takes the position that science is an objective inquiry into nature or reality, which proceeds on the basis of factual observation, then how does one explain the rejection of established theories in favor of new theories throughout the history of science? The traditional answer, termed "reactionary" by Latour, is that science makes mistakes or errors that are corrected as science progresses toward truth. Because errors are explained by human sociological or psychological factors, even the most conservative accounts of science concede a human or subjective element.[231] Mistakes might be explained by reference to poor training, poor instruments, false presuppositions, or community or individual bias toward a particular (but

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230. Latour, *supra* note 228, at 279.

231. Jonathan Potter identified Robert Merton as a traditional sociologist of science who "started from a received view of the nature of scientific facts – that they are impersonal, empirically warranted, rigorously tested." POTTER, *supra* note 18, at 18. For Merton, modern science is sustained by norms like universalism, disinterestedness, and organized skepticism. *Id.* Potter stated:

> Although Merton stressed the importance of . . . norms for guiding the scientific activity of fact-finding, he also stressed that scientists do not always conform to these norms. At times there is fraud; scientists may keep results to themselves or pass them only to selected associates; there may also be prejudice against particular individuals or groups . . . .
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> . . . Prejudice against a group of researchers may result in the maintenance of a mistaken theory in the face of a correct alternative, or individual ambition may lead a scientist to falsify findings to fit a desired model . . . . False belief could be directly explained through a "social fact" (personality, prejudice, and so on) disrupting the proper operations of scientific norms . . . . Put simply, in this view of science, the facts themselves determine truth, while error is explained by processes of a psychological or sociological nature.

Id. at 19.

wrong) theory. For example, the amicus brief filed in *Joiner* by *The New England Journal of Medicine*, purporting to describe the nature of scientific evidence, represents a traditional view of science.[232] While science is characterized by "its complete reliance on objectively verifiable evidence,"[233] this characterization will not explain all changes in science. Accordingly, the amicus brief's author conceded that science is tentative and probabilistic and that "even the most honest researchers . . . are likely to be enthusiastic about their ideas and . . . not aware of flaws in the design of their study."[234] Nevertheless, science is "not a matter of opinion or argument," but "of evidence."[235] This strong positivist view of science is alive and well in many quarters of the natural and empirical social sciences.[236]

This formulation suggests that social and psychological factors are not part of science but are impediments to the scientific enterprise. Because the amicus brief emphasizes the unfortunate likelihood of such factors ("researchers may unconsciously misrepresent their work or exaggerate its importance"[237]), it adopts a "conservative" stance in Latour's frame of reference.[238] In short, scientists are human, but science itself, which can protect "against unwarranted conclusions" by peer review and other collaborative efforts, transcends opinion and argument.[239] Significantly, the scientific community, as opposed to individual, enthusiastic scientists, is represented as a stable foundation for science.[240] It is the activity of individual scientists, represented

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232. Brief of Amici Curiae New England Journal of Medicine and Marcia Angell, M.D., in Support of Neither Petitioners nor Respondents, *General Elec. Co. v. Joiner*, 522 U.S. 136 (1997) [hereinafter Amicus Brief].

233. *Id.* at 5.

234. *Id.* at 12-13.

235. *Id.* at 18.

236. *See* Risinger et al., *supra* note 119, at 439 ("It is probably not very controversial to say that, within the realm of factual inquiry, the properly tested products of the enterprise of science, where relevant, are *the most dependable sources of factual information available to human beings.*" (emphasis added)).

237. Amicus Brief, *supra* note 232, at 12.

238. *See supra* note 230 and accompanying text ("[T]here are still factors from society that 'leak in' and influence its development.").

239. Amicus Brief, *supra* note 232, at 12. *But see* Redding, *supra* note 69, at 592-97 (discussing studies showing bias in peer review process and throughout scientific process – from hypothesis generation to publication process).

240. In fact, some legal commentators argue that the scientific community is the only proper judge of scientific validity. *See* Paul S. Milich, *Controversial Science in the Courtroom: Daubert and the Law's Hubris*, 43 EMORY L.J. 913, 919, 923 (1994) (arguing that science should be only source of its reliability). Milich wrote:

> Scientists who have spent the greater portions of their professional lives wrestling with the complexities and mysteries of their disciplines must be amazed at the law's hubris in thinking that nonscientist judges can "get up to speed" on a scientific

as the changing social factor, that helps to explain away errors in the history of science.

A more progressive, using Latour's terminology,[241] view of science, which has been characterized as a "powerful and widespread" reaction to traditional ideals of empirical observation,[242] highlights the inevitable role of theoretical expectations in organizing perception.[243] Instead of viewing scientific theories as following from observation, and viewing observational experiments as the way to adjudicate between theories, theoretical presuppositions are seen as shaping observation and experiment from the outset.[244] This psychological model is also a social theory of the way scientific communities work. Thomas Kuhn's famous paradigm theory[245] emphasized how communal presuppositions can support a research program of "normal science" for a time, after which the successively appearing anomalies in the program are sufficient to bring on its revolutionary overthrow in favor of a new research program.[246] Note that the scientific community is the stabilizing aspect of science, as it was in the conservative view, but now the community has become a changeable social factor that directs scientific observation and experiment. The scientific community is no longer the cure to avoid the taint of social factors, but is the primary social factor. Likewise, sociological analysis is no longer reserved for falsehoods but serves to explain normal science.

In view of our criticism of certain legal narratives about the study of science, several observations can be made concerning the development of the progressive perspective on the social aspects of scientific theory and practice.

> argument and ultimately decide who has the better of the argument . . . . Science is the only source of its own reliability.

Id.

241. *See supra* note 230 and accompanying text.

242. *See* GALISON, *supra* note 18, at 9.

243. *See generally* THOMAS S. KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (1962).

244. *See* GALISON, *supra* note 18, at 9 (stating that Kuhn stresses "how theoretical expectations profoundly shape what is observed and when"); *see also id.* at 8-9 (discussing Kuhn's view of philosophy of science). Galison stated:

> . . . Kuhn assailed the universal adjudicating power of experiments, and therefore their independence from theory. Instead of arguing that observation must precede theory, Kuhn contended that theory has to precede observation. The history of science, for Kuhn, amply demonstrated the essential role theory plays in the conduct of experimentation, in the interpretation of data, and in the definition of "relevant" phenomena.

Id.

245. *See* KUHN, *supra* note 243, at viii (stating paradigms are "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners").

246. *See id.* at 10-22 (discussing paradigm theory).

While it is useful to speak of a transformation in the history, philosophy, and sociology of science growing out of the grand theories of Karl Popper,[247] Thomas Kuhn,[248] and Paul Feyerabend[249] in the 1960s and 1970s, the critique of science is not a unified field of inquiry. Indeed, the grand theorists themselves held quite different views on science. Popper, even in his challenge to logical empiricism, thought that experiments could adjudicate between theories.[250] Kuhn disagreed, but was uncomfortable with radical views that

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247. Karl Popper's CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE (1972) and OBJECTIVE KNOWLEDGE: AN EVOLUTIONARY APPROACH (1972) were certainly viewed in the 1970s as crucial to the breakdown of logical positivism, because Popper was willing to study the context of discovery – the historical dimension of the production of knowledge. *See generally* GERALD RADNITZKY, CONTEMPORARY SCHOOLS OF METASCIENCE: ANGLO-SAXON SCHOOLS OF METASCIENCE CONTINENTAL SCHOOLS OF METASCIENCE, xxiv-xxvi (1973). Interestingly, while Popper is known for inspiring critics of traditional philosophy of science, contemporary historiography tends to classify Popper as quite traditional. In other words:

> Despite [Popper's] own claims that it was he who killed positivism (1974), the accidental fact that the English edition of Popper's [*The Logic of Scientific Discovery* (1959)] appeared shortly before Kuhn's [*The Structure of Scientific Revolutions* (1962)] put him in a position to become a primary defender of the positivist faith against the Kuhnian heresy.

GIERE, *supra* note 14, at 211.

248. Kuhn is generally credited, alongside Paul Feyerabend, *see infra* note 249, for initiating the social and linguistic *turn* in the history and philosophy of science. In other words:

> The contemporary situation within the philosophy of science has been decisively shaped by its encounter with the work of Thomas Kuhn and Paul Feyerabend. Whatever Kuhn and Feyerabend themselves would say about the matter, their work has been widely interpreted within philosophy and elsewhere as an attack on the rationality and cultural authority of the sciences.

ROUSE, *supra* note 10, at 4. Their work, that is, "led inexorably to the strong forms of epistemic relativism for which post-postivism is notorious." LAUDAN, *supra* note 14, at 9.

249. Paul Feyerabend's AGAINST METHOD (1972) has been viewed as the radical fringe of the first wave of attacks on logical positivism and on scientific knowledge as not just relativistic but anarchistic: "Feyerabend gleefully champions 'irrationality' against the type of rationality defended by critical rationalists [like Popper]. He claims that if one applies the standards of Popper and his followers one is forced to conclude that science itself is a thoroughly irrational discipline – and ought to be." BERNSTEIN, *supra* note 10, at 4.

250. According to Galison:

> Popper did not disturb the logical positivists' basic faith that experiments unambiguously adjudicate among theories. Sir Karl's famous suggestion that theories be evaluated on their "falsifiability" – how open they were to experimental disconfirmation – depended essentially on the possibility of matching competing theories with the clear results of experimental evidence.

GALISON, *supra* note 18, at 7. Additionally:

> For Popper, Science is distinguished from non-science by the *activity* of trying critically to test hypotheses and resisting the temptation to make continual *ad hoc*

challenged the rationality of science.[251] Feyerabend, who practically viewed science as a religious practice, inspired much more radical accounts of the sociality of science. He believed science is not affected by social phenomenon but *is* a social phenomenon.[252]

Serious debates have followed, and a spectrum of positions are identifiable on several key issues. For example, on the question of whether science describes nature or reality, one may believe that: (1) the findings of science are determined by natural phenomena, (2) our access to nature is affected by biases that must be identified and rooted out, (3) theoretical expectations are an inevitable part of science, (4) social interests (e.g., professional interests) actually condition perception of external nature, (5) facts are constructed against the resistance of nature, or finally, (6) scientific facts are social constructs. Moreover, some realists concede that science rests on inevitable and influential social and linguistic structures, but rejecting social constructivism, they emphasize the approximation of scientific models or probabilities to natural reality. Any attempt to reduce this spectrum to two views, or to the old view and the new view, wrongly suggests that the debates have stagnated or that the "knowledge" in studies of science is somehow stable and easily exportable into other disciplines such as law.

> modifications to keep hypotheses going in the face of counter-evidence . . . . Popper was strongly critical of Kuhn's suggestion that periods of "normal," stable and unquestioning science are necessary for its development; for him this was simply bad science.

POTTER, *supra* note 18, at 24.

251. *See* GIERE, *supra* note 14, at 41 (stating that "in appealing to psychological and sociological factors to explain the historical development of science, [Kuhn] implicitly rejected any search for rational principles of theory choice"). *But see* ROUSE, *supra* note 10, at 4 n.6 ("It is now widely recognized that this image of Kuhn as an 'irrationalist' is quite at odds with Kuhn's own self-understanding . . . ."). *See generally* THOMAS S. KUHN, THE ESSENTIAL TENSION: SELECTED STUDIES IN SCIENTIFIC TRADITION AND CHANGE (1977) (responding to misinterpretation of his work as attack on rationality in science).

252. *See* BERNSTEIN, *supra* note 10, at 87 ("Feyerabend . . . presses his views much further than Kuhn."); LAUDAN, *supra* note 14, at 14 ("Feyerabend supposed that there was no rational machinery for resolving methodological differences between scientists."). In Laudan's words:

> [Feyerabend] insists . . . scientists have frequently utilized methods that fly directly in the face of all our "rational" guidelines. Thus, scientists sometimes proceed 'counter-inductively,' deliberately ignoring contrary evidence and the testimony of their senses. Oftentimes, scientists refuse to give up hypotheses that appear to have been decisively refuted. Frequently, they lie, cheat, suppress information, propagandize, and resort to all manner of other trickery to persuade others. . . . [S]uch apparently deceitful behavior is, on Feyerabend's view, absolutely crucial to the advance of science.

Id. at 100.

Most accounts of the last thirty to forty years of studies of science identify an old view associated with: (1) logical empiricism in philosophy,[253] (2) Merton's "norms of science" in sociology,[254] and (3) traditional stories of linear progress from ignorance to truth in the history of science.[255] If one contrasts this with a new view, associated with (1) Karl Popper in philosophy,[256] (2) modern sociology of scientific knowledge,[257] and (3) Kuhn's paradigm theory of the history of science,[258] the problems with such an account are evident. As Latour's continuum illustrated, with respect to social influences on science, there are numerous middle positions between the radicals-and-progressives and the reactionaries-and-conservatives. Moreover, the so-called "new" views of Popper in the philosophy of science, of the early sociologists of scientific knowledge, and of Kuhn in the history of science would all now be somewhere in the middle because of more radical developments in those fields.

While it is accurate to highlight Karl Popper's rejection of Rudolf Carnap's logical positivism[259] and inspiring "falsification" thesis,[260] social con-

253. *See, e.g.*, GALISON, *supra* note 18, at 7.

254. *See, e.g.*, POTTER, *supra* note 18, at 17-18 (finding Merton's *norms of science* include universalism, communism (i.e., knowledge is openly shared), disinterestedness, and organized skepticism).

255. *See, e.g.*, Steve Fuller, *Does Science Put an End to History, or History to Science?*, 46-47 SOC. TEXT 27, 31-32 (1996). Fuller wrote:

> According to Kuhn, scientists need to tell stories of collective progress .... The professional historian is wont to tell a story filled with so many accidents and failures that it would dispirit fledgling natural scientists. In a manner that Kuhn called "Orwellian," every scientific revolution must be followed by a rewriting of the discipline's history to make the victorious party appear the natural heirs ....

Id.

256. *See, e.g.*, GALISON, *supra* note 18, at 7 (describing Popper's break from logical positivism but confirming his fidelity to capacity of experiments to confirm theories).

257. *See, e.g.*, POTTER, *supra* note 18, at 25-34.

258. *See, e.g.*, GIERE, *supra* note 14, at 34 ("Thomas Kuhn's *Structure of Scientific Revolutions* (1962) has emerged as the single most influential work on the nature of science to be published since World War II.") *But cf.* Norton-Smith, *supra* note 10 (arguing that history of science provides very limited and questionable benchmark or model for understanding modern scientific progress; rather, we should consult current scientific practices because 90% of scientists who have ever lived are alive today, success of modern science is unparalleled, and modern scientific practices are radically different than those of past).

259. *See* GALISON, *supra* note 18, at 7.

260. Popper thought that science could never achieve absolute truth but could obtain successively closer approximations to it. But it "would seem that the same limitations which keep us from accessing the truth with certainty would also preclude us from knowing when we were 'approaching' it." Sean O'Connor, *The Supreme Court's Philosophy of Science: Will the Real Karl Popper Please Stand Up?*, 35 JURIMETRICS 263, 276 (1995); *see also* W.V.O. QUINE, FROM A LOGICAL POINT OF VIEW: 9 LOGICO-PHILOSOPHICAL ESSAYS 20 (1961) (providing influential post-positivist critique of Popper's falsifiability principle).

structivism, the view that scientific facts are manufactured[261] rather than discovered, is not obviously Popperian. Moreover, there are numerous versions of social constructivism – some more radical than others and thus further from the middle position that Popper has come to represent.[262] Even if, for purposes of scholarly discourse, we can identify "a cluster of important doctrines that distinguish" constructivism,[263] there are still plenty of "new" philosophers of science that are as uncomfortable with constructivism as they are with the "old" view.[264] Any simple distinction between old and new is problematical.

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261. The most eminent scientist of the twentieth century, Albert Einstein, noted that "[p]hysical concepts are free creations of the human mind . . . not . . . uniquely determined by the external world." ALBERT EINSTEIN & LEOPOLD INFELD, THE EVOLUTION OF PHYSICS: THE GROWTH OF IDEAS FROM EARLY CONCEPTS TO RELATIVITY AND QUANTA 33 (1938). *See generally* KARIN D. KNORR-CETINA, THE MANUFACTURE OF KNOWLEDGE: AN ESSAY ON THE CONSTRUCTIVIST AND CONTEXTUAL NATURE OF SCIENCE (1981); ANDREW PICKERING, CONSTRUCTING QUARKS: A SOCIOLOGICAL HISTORY OF PARTICLE PHYSICS (1984) (providing example of how physics is constructed). The view that scientists construct scientific "facts" rather than discover them has been expressed by numerous scientists and philosophers of science. *See, e.g.*, BRUNO LATOUR & STEVE WOOLGAR, LABORATORY LIFE: THE SOCIAL CONSTRUCTION OF SCIENTIFIC FACTS (1979); Kenneth J. Gergen, *The Social Constructivist Movement in Modern Psychology*, 40 AM. PSYCHOLOGIST 255 (1985); Sandra Scarr, *Constructing Psychology: Making Facts and Fables for Our Times*, 40 AM. PSYCHOLOGIST 499, 499 (1985).

262. *See* POTTER, *supra* note 18, at 35 ("The term 'constructionism' is used with a number of distinct and sometimes contradicting shades of meaning across the social sciences and even within [the Sociology of Scientific Knowledge] itself."); *see also* GIERE, *supra* note 14, at 43. Giere stated:

> Historians and sociologists associated with the Edinburgh school have produced an imposing set of historical cases which they claim illustrates the role of various kinds of interests [including personal, professional, social, political, or a combination] in the development of science.
>
> Other "new wave" sociologists of science are even more radical. It is not just that nonscientific interests "influence" scientists' judgements about theories. Rather, science is totally *constituted* by human interests and interactions. Science is simply a social construct, like morals or the law.

Id. at 43.

263. Arthur Fine, *Science Made Up: Constructivist Sociology of Scientific Knowledge*, *in* THE DISUNITY OF SCIENCE: BOUNDARIES, CONTEXT AND POWER 231, 233 (P. Galison & D. Stump eds., 1995) (giving as example view that beliefs that are relative to and explainable by reference to social circumstances and are locally caused (i.e., not caused by truth or rationality)).

264. *See* GIERE, *supra* note 14, at 44 ("Philosophers . . . have been seeking objective criteria for rational scientific progress. Sociologists . . . argue that scientific progress is no different from political or social progress. One wonders whether there is not a middle way. I think there is."); LAUDAN, *supra* note 14, at 3 (distancing himself from both positivist orthodoxy and epistemological and methodological relativism that replaced it); ROUSE, *supra* note 10, at 148 (rejecting "distinction between autonomous social and/or natural 'worlds,'" and thereby rejecting both naturalism and social constructivism). Significantly, Giere, Rouse, and Laudan have distinct and inconsistent analyses. Laudan argued for science as rational, and he was a strong

In social studies of science, the sociology of scientific knowledge can be characterized as the relativistic view that displaces traditional sociology of science, but Collins's early *Empirical Programme of Relativism*[265] must be distinguished from later constructivist and interest theories of scientific knowledge and practice.[266] Similarly, in the history of science, Kuhn's seemingly revolutionary views have been criticized and overtaken by historians, some influenced by social constructivism and social interest theorists and others returning to ideals of rationality. The so-called "strong program," which treats science like any other cultural activity, goes far beyond Kuhn's view that theory shapes science.[267] In other words,

> [w]e are witnessing, after the Kuhnian move from continuity and verity of scientific knowledge to discontinuity and relativity, another turn – from the

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critic of "the new crazies" – his term for everyone from Kuhn to the most radical social constructivists. LAUDAN, *supra* note 14, at 3. Rouse was a proponent of cultural studies of science and viewed Laudan as locked into old-style legitimation of science. *See* ROUSE, *supra* note 10, at 8-9, 36, 60, 64. Giere characterized his account as representational, naturalistic, cognitive, and evolutionary, and was critical of Laudan and, impliedly, Rouse. *See* GIERE, *supra* note 14, at 41-43, 53-55.

265. *See* POTTER, *supra* note 18, at 25-34. Potter contrasted Collins's work with:

> the traditional sociology of science, which focused on the social conditions or norms that enable the generation of true knowledge, and on the way particular social or psychological factors such as prejudices and personal ambitions led to scientific errors. For Harry Collins, . . . this traditional work legitimated any current *status quo* by presupposing the correctness of any current state of belief. It assumes that what scientists take as valid scientific knowledge needs no *social* explanation, for it is adequately accounted for by the nature of the *natural* phenomena that are being studied.

Id. at 25. While adopting a relativist stance toward science, however, "Collins adopts a realist stance when conceptualizing the activities and beliefs of scientists." *Id.* at 30. Thus, while Collins was disinterested in the truth of scientists' judgments, he made the same type of judgments about the social world. *Id.* at 32.

266. *See* POTTER, *supra* note 18, at 34-40 (discussing Collins). Constructionist and interest theories share with Collins's work a rejection of traditional sociological accounts of how "social norms . . . ensure the production of true knowledge, as well as the idea that the task of the social analyst is to account only for scientific errors." *Id.* at 34. Constructionist accounts, however, are generally more ethnographic and observational, generally focus on unfinished knowledge rather than controversies, and are sometimes less likely to hypothesize that the natural world makes no difference to science. *Id.* at 36-37. Social interest accounts, instead of emphasizing a scientists rhetorical (rather than technical, experimental) success, like Collins, emphasize individual interests, group allegiance, and political viewpoints to explain the success of certain theories. *Id.* at 37-38.

267. *See* LAUDAN, *supra* note 14, at 183 (identifying Mary Hesse, Martin Rudwick, Barry Barnes, Steven Shapin, David Bloor, and Kenneth Caneva as adherents). The "strong program" is a reference to the Edinbergh Strong Programme or the Edinbergh School of the Sociology of Knowledge, so named because of the tendency to view scientific knowledge as a social construction and not just as involving a social context. *Id.*

> Kuhnian predilection for science as theory to post-Kuhnian engagement with science as experiment.[268]

The turn to the history of science as a history of experimental practices, not simply of theories, is represented by ethnographic and anthropological studies of the scientific enterprise. These studies look at science as if it is another culture and scientists a "tribe." But even among those who make the turn to the study of practices, instruments, experiments, and techniques by visiting laboratories, museums, and fields[269] and give an account of the "different professional cultures, moral economics, forms of initiation, collaboration, and negotiation that develop in these sites,"[270] there are disagreements about the characterization of science as a social and cultural activity. Is scientific knowledge always local, interested, constructed, and supported by persuasive rhetorical networks, or does that picture denigrate the role of nature too much? This question persists in studies of scientific texts as literary and rhetorical,[271] of gender dimensions of science,[272] of different cognitive styles in different historical and national cultures,[273] and of the politics of science.[274]

On the other hand, there is a strong reaction against social studies of science that is best characterized not as a *return* to positivism, but as an alternative to positivism that corrects its errors yet retains its best features, including some form of realism.[275] Significantly, that project is not the same as the movement toward realism by those *engaged in* social studies of science, which movement engenders *another* strong reaction by critics of those who lapse into realism or become "closet" realists.[276] Indeed, the political termi-

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268. Hans-Jörg Rheinberger, *Experimental Systems, Graphematic Spaces, in* INSCRIBING SCIENCE: SCIENTIFIC TEXTS AND THE MATERIALITY OF COMMUNICATION 285 (Timothy Lenoir ed., 1998).

269. *See* DON IHDE, EXPANDING HERMENEUTICS: VISUALISM IN SCIENCE 135-36 (1998) (describing "going native" approach to science in which science critic is participant in scientific research teams, approach more common in Scandanavian and Dutch Universities); Knorr-Cetina, *supra* note 18, at 287-310 (reporting observations at large research laboratory).

270. Mario Biagioli, *Introduction: Science Studies and Its Disciplinary Predicament* to THE SCIENCE STUDIES READER, *supra* note 18, at xi, xv.

271. *See generally* INSCRIBING SCIENCE: SCIENTIFIC TEXTS AND THE MATERIALITY OF COMMUNICATION (Timothy Lenoir ed., 1998); POTTER, *supra* note 18, at 42-121.

272. *See generally* GIERE, *supra* note 14, at 200-16; EVELYN FOX KELLER, REFLECTIONS ON GENDER AND SCIENCE (1985); Cynthia Fuchs Epstein, *The Multiple Realities of Sameness and Difference: Ideology and Practice*, 53 J. SOC. ISSUES 259 (1997).

273. *See generally* Sharon Traweek, *An Introduction to Cultural and Social Studies of Sciences and Technologies*, 17 CULTURE, MED., & PSYCHIATRY 3 (1993).

274. *See generally* Stanley Aronowitz, *The Politics of the Science Wars*, 46-47 SOC. TEXT 177 (1996); Redding, *supra* note 69.

275. *See supra* note 264.

276. *See generally* Timothy Lenoir, *Was the Last Turn the Right Turn?*, *in* THE SCIENCE STUDIES READER, *supra* note 18, at 290-301.

nology used by Latour betrays the character of the debates about the nature of science – discussion is charged, ideological, and full of disagreements about each side's evidence and whether it is compelling. For our purposes, Latour's frame of reference represents the "discipline" or collection of sub-disciplines from philosophy, history, sociology, literary theory, cultural studies, ethnology, anthropology, and gender studies that is being called upon when the United States Supreme Court or interdisciplinary legal scholars reflect upon the nature of scientific discovery and practice.

VI. Legal Reflections on the Nature and Philosophy of Science

> In the philosophy of science the scope of the constructivist program fits in with the "big methodologists," like Popper... and Kuhn, each of whom, not surprisingly, rejects it.[277]

> Actors, interests, politics, power, and authority have acquired the status of key terms in a "strong program" to treat science on par with any other cultural activity whatsoever. That Thomas Kuhn is "among those who have found the claims of the strong program absurd"... might not surprise.[278]

A. Daubert *and Science*

The *Daubert* Court rejected the "general acceptance" test from *Frye v. United States*.[279] If the *Frye* test allowed judges to let the scientific community decide whether an expert's testimony was reliable, then *Daubert* appointed trial judges as gatekeepers to discern good from bad scientific theories.[280] When Justice Blackmun offered several observations about the nature of scientific knowledge and methodology, the Court logged onto Latour's frame of reference and took a position on the nature of scientific knowledge alongside the historians, philosophers, and sociologists of science who occupy that continuum.

In *Daubert*'s wake, judges and lawyers were concerned about whether the federal judiciary should enter that debate and take an "official" position on the nature of science.[281] On remand to the Ninth Circuit Court of Appeals,

277. Fine, *supra* note 263, at 232.

278. Rheinberger, *supra* note 268, at 285.

279. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 585-93 (1993) (citing Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923)).

280. *See id.* at 600 (Rehnquist, C.J., concurring in part and dissenting in part) ("I do not doubt that [Federal] Rule [of Evidence] 702 confides to the judge some gatekeeping responsibility in deciding questions of the admissibility of proffered expert testimony.").

281. Indeed, Chief Justice Rehnquist, in his partial dissent to *Daubert*, claimed not to understand "what is meant when it is said that the scientific status of a theory depends on its

Judge Kozinski, in a portion of his opinion titled "Brave New World," observed that "scientists often have vigorous and sincere disagreements as to what research methodology is proper."[282] He then concluded:

> Our responsibility, then, unless we badly misread the Supreme Court's [*Daubert*] opinion, is to resolve disputes among respected, well-credentialed scientists about matters squarely within their expertise, in areas where there is no scientific consensus as to what is and what is not "good science," and occasionally to reject such expert testimony because it was not "derived by the scientific method." Mindful of our position in the hierarchy of the federal judiciary, we take a deep breath and proceed with this heady task.[283]

Not surprisingly, *Daubert* has been the subject of voluminous critical commentary. Some find the new standard unworkable, some find the opinion unclear, and some offer new frameworks of analysis.[284] Meanwhile, federal judges (and many state court judges) have applied *Daubert* since 1994, and the Supreme Court has issued two opinions – *General Electric Co. v. Joiner* and *Kumho Tire Co. v. Carmichael* – to clarify the standard of review of admissibility rulings[285] and the applicability of *Daubert* to non-scientific experts.[286]

The twenty-two amicus briefs filed in *Daubert*[287] addressed "definitions of scientific knowledge, scientific method, scientific validity, and peer review."[288] All this interest was understandable in light of the fact that the

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'falsifiability'," suspected that some trial judges would likewise be confused, and worried about imposing on trial judges "either the obligation or the authority to become amateur scientists . . . ." *Id.* at 600-01 (Rehnquist, C.J., concurring in part and dissenting in part).

282. Daubert v. Merrell Dow Pharms., Inc., 43 F.3d 1311, 1316 (9th Cir. 1995).

283. *Id.* at 1316.

284. *See generally* Capra, *supra* note 17 (stating that trial courts are confused as to whether *Daubert* regime is more strict or more permissive; rejection of *Frye* is not compelling as policy matter; *Daubert* opinion vague); Gottesman, *supra* note 17 (arguing *Daubert* inconsistent with congressional intent regarding Federal Rules of Evidence); Imwinkelried, *supra* note 7 (extending implications of *Daubert* beyond vast amount of myopic commentary); Koukoutchos, *supra* note 17 (finding some progress in *Daubert* regarding judicial apprehension of science, but some regress as well); Mansfield, *supra* note 17 (noting confusion remains as to meanings of "scientific" testimony, "scientific validity," "falsifiability," and even "science").

285. *See* General Elec. Co. v. Joiner, 522 U.S. 136, 138-39 (1997) ("We granted certiorari in this case to determine what standard an appellate court should apply in reviewing a trial court's decision to admit or exclude testimony under *Daubert*.").

286. *See* Kumho Tire Co. v. Carmichael, 526 U.S. 137, 141 (1997) ("This case requires us to decide how *Daubert* applies to the testimony of engineers and other experts who are not scientists.").

287. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 598-99 (1993) (Rehnquist, C.J., concurring in part and dissenting in part) (stating that briefs were "markedly different from typical briefs, in that large parts of them do not deal with decided cases or statutory language").

288. *Id.* at 599 (Rehnquist, C.J., concurring in part and dissenting in part).

United States Supreme Court was preparing to issue a pronouncement on science and its authority in legal culture.[289] In simplest terms, the *Frye* "general acceptance" standard[290] enacted a certain colonization of law by the scientific elite,[291] and any revision to that standard would likely constitute a challenge to the dominance of science as the discourse of rationality, plausibility, and legitimacy.[292]

While Justice Blackmun conceded that "arguably, there are no certainties in science," he also confirmed that any "inference or assertion must be derived by the scientific method" to qualify as scientific knowledge.[293] Later in the opinion, he defined scientific methodology as generating falsifiable hypotheses, and identified the key standard for admissibility as being whether a theory or technique has been tested.[294] At this point, the Court adopted what Latour would call a reactionary position with respect to science – "someone who

289. *See* Mansfield, *supra* note 17, at 19. Mansfield stated:

> [Given] the large number of amicus briefs filed by eminent scientists and important scientific organizations, . . . [it] would appear that the scientists who allowed their names to be put on these briefs believed . . . that somehow the fate of science was at stake in the role of admissibility the Court would adopt and that there is a connection between the admissibility of evidence in courts of law and standards of proof to be satisfied in contexts important to science . . . .

Id.

290. *See* Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923) (stating that expert opinion based on scientific technique is inadmissable unless technique is "generally accepted" as reliable in relevant scientific community).

291. *See, e.g.*, Milich, *supra* note 240, at 914 (arguing in favor of *Frye* standard, which recognizes that "trial courts should not accept scientific evidence unless and until science accepts it").

292. Michel Serres, for example, has warned that law, like the arts and religion, is in decline, because science lately "has all the power, all the knowledge, all the rationality, all the rights, too, of course, to all plausibility or legitimacy." MICHEL SERRES & BRUNO LATOUR, CONVERSATIONS ON SCIENCE, CULTURE, AND TIME 87 (R. Lapidus trans., 1995). Chief Judge Markey, of the United States Court of Appeals for the Federal Circuit, worried that "juriscience" would replace jurisprudence. Howard T. Markey, *Jurisprudence or "Juriscience"?*, 25 WM. & MARY L. REV. 525, 525 (1984). "If the development of science is viewed as a race for dominance between science and law, science appears to be winning. We perceive the infallible, objective, dedicated nature of the scientific method as the solution to all of man's problems . . . ." *Id.* at 526. *But cf.* Brian Z. Tamanaha, *An Analytical Map of Social Scientific Approaches to the Concept of Law*, 15 OXFORD J. LEGAL STUD. 501, 533 (1995) (arguing that culture increasingly is confused with law and that we live in "the age of the symbolic ascendance of law").

293. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 590 (1993) (defining "scientific" and "knowledge" in Federal Rule of Evidence 702: "Scientific" implies "a grounding in the methods and procedures of science," and "knowledge" connotes "more than subjective belief or unsupported speculation").

294. *See id.* at 593. The falsifiability or testability criterion is identified as the core of good science by commentators in the positivist tradition who urge courts to exclude scientific evidence not subjected to empirical testing. *See generally* Faigman et al., *supra* note 45; Faigman & Wright., *supra* note 220; Morse, *supra* note 220; Risinger et al., *supra* note 119.

would claim science becomes really scientific only when it finally sheds any trace of social construction."[295] Justice Blackmun cited Popper's falsifiability thesis,[296] which represents in the philosophy of science a critique of the logical empiricist view that theory can be separated from experiment. For Popper, theory was essential to the practice of experimentation, which seems to be an acknowledgment of social and psychological influence. But Popper also kept his "faith that experiments unambiguously adjudicated between theories," so the social factor could be eliminated.[297] Professor Mansfield has pointed out that the references to Popper in *Daubert* "leave a mistaken impression as to the authoritative position [Popper's views] hold among scientists and philosophers of science."[298] We agree – the Court gave no indication that it was choosing one controversial position in a highly contested field.

The next paragraph of *Daubert* (following the adoption of the falsifiability standard) contains the famous aphorism that publication "does not necessarily correlate with reliability."[299] One paragraph later, Justice Blackmun made the same move regarding "general acceptance" – it remained a very important consideration, but it is not required.[300] Numerous commentators have identified this part of the opinion as "the social turn," because an acknowledgment that an unaccepted, novel theory may be good science implies that the accepted science just might be outdated.[301] This is called a "social" turn because the outdated science, which was obviously not entirely grounded

295. *See supra* note 230 and accompanying text.

296. *See Daubert*, 509 U.S. at 593 ("[T]he criterion of the scientific status of a theory is its falsifiability, or refutability, or testability." (citing K. POPPER, CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE 37 (1989))).

297. *See* GALISON, *supra* note 18, at 7 ("Sir Karl's famous suggestion that theories be evaluated on their 'falsifiability' – how open they were to experimental disconfirmation – depended essentially on the possibility of matching competing theories with the clear results of experimental evidence.").

298. Mansfield, *supra* note 17, at 11; *see also* Risinger et al., *supra* note 119, at 438 n.103 (noting conflict between Popper and his critics).

299. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593 (1993) ("[I]n some instances well-grounded but innovative theories will not have been published.").

300. *Id.* at 594. Significantly, the language of the opinion regarding general acceptance is quite guarded. Justice Blackmun did say that general acceptance can "have a bearing on the inquiry," and that lack of general acceptance" may properly be viewed with skepticism, but he did not clearly state the implication that a view that was not generally accepted could be admissible evidence. *Id.*

301. *See, e.g.*, Farrell, *supra* note 17, at 2196 ("[S]tatements of fact gain their validity not from their closeness to objective truth, but from the authority and legitimacy of those who pronounce them."); Imwinkelried, *supra* note 7, at 61 ("Rather than being characterized by constant progress, the history of science consists of a succession of superceded theories."); Koukoutchos, *supra* note 17, at 2239 (arguing that science is cultural and social activity); *id.* at 2244 ("Advancement in scientific understanding frequently comes from what was once denounced as unorthodox.").

in nature, likely became the received view in part because of communal bias and persuasive networks. Combine this implication with the Court's statements that there were no certainties in science, and you have a Kuhnian version of scientific progress: Normal science is biased in favor of a ruling theoretical paradigm that may be overthrown someday soon.[302] The *Frye* "general acceptance" standard would have allowed mainstream scientists to keep novel theories out of federal court, but on the suggestion of numerous amici,[303] the Court adopted a more modern approach based on insights from the history and philosophy of science. As Justice Blackmun noted, some amici were fearful of a "free-for-all" of pseudoscience, but conventional procedural safeguards should serve to prevent that.[304]

While Justice Blackmun also addressed the possibility that "authentic insights and innovations" may not be admitted under the new standard in some cases,[305] that "is the balance that is struck by rules of evidence designed not for the exhaustive search for cosmic understanding, but for the particularized resolutions of legal disputes."[306] This comment apparently suggests a sensitivity to the scientific orthodoxy of a ruling paradigm. The text as a whole, however, refers instead to the possibility of legal scientific orthodoxy, not scientific orthodoxy,[307] because Justice Blackmun characterized science itself as open-minded.[308] The general tenor of these remarks is, in Latour's terms, reactionary:[309] Science allows "a multitude of hypotheses" so that "those that are incorrect will eventually be shown to be so, and that in itself is an advance." [310] In other words, science advances by rooting out errors. No social turn here!

Evelyn Fox Keller has highlighted a polarizing tendency in the discourse about the nature of science – we are

> propelled either toward objectivism, or towards relativism. In one direction, . . . science return[s] to a premodern . . . conception in which . . .

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302. *See supra* notes 241-46 and accompanying text.

303. *See* Imwinkelried, *supra* note 7, at 64 ("Amicus submissions . . . shared this common denominator; all endeavored to disabuse the justices of the 'mythical' popular conception of science.").

304. *Daubert*, 509 U.S. at 595-96.

305. *Id.* at 597.

306. *Id.*

307. But the possibility of scientific orthodoxy may be even greater. *See generally* Redding, *supra* note 69 (discussing how law outpaced science in cases where there was scientific orthodoxy).

308. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 573, 597(1993) ("[T]he scientific project is advanced by broad and wide-ranging consideration of a multitude of hypotheses.").

309. *See supra* note 230 and accompanying text.

310. *Daubert*, 509 U.S. at 597.

> science [has been collapsed] back onto nature. Under the other, we are invited into a postmodernist . . . (and post-scientific) utopia in which . . . science run[s] free, no longer grounded . . . by nature – indeed, in which . . . nature [has] effectively disappeared altogether. Attempts to occupy a "middle ground" . . . must contend not only with the conceptual difficulty of formulating such a position, but also with the peculiarly insistent pressures of a public forum urging [the concept of science] toward one pole to the other.[311]

Thus, we can sympathize with Justice Blackmun because middle positions are difficult to formulate and there is pressure to adopt one of the extremes. In *Daubert*, however, the pressure seemed to be toward a reactionary view of science. In the three *Daubert* passages discussed above concerning the scientific enterprise, the first and last gave no indication that science is even partially a product of social relations. The middle passage indirectly echoed Kuhn's paradigm theory, which is at best a "wishy-washy" middle position in Latour's frame of reference[312] and at worst is a temporary nod to the history of science that is eclipsed by the linear view of scientific progress implied by the surrounding passages.

The significance of *Daubert*, of course, is not its contribution to the history and philosophy of science. No one expects a United States Supreme Court opinion to be a model of interdisciplinary research. *Daubert*, on the other hand, has spurred a lot of interest in the history and philosophy of science, much of it critical of Justice Blackmun's reflections on the nature of the scientific enterprise.[313] Yet, a tendency persists to oversimplify the history, philosophy, and sociology of science as reflecting two views, resulting in a failure to engage that disciplinary cluster of debates.

B. The Two-View Thesis in Law and Science Studies

Professor Imwinkelried, who consulted with the *Daubert* plaintiffs in preparing their Supreme Court brief, noted in 1995 that although *Daubert* "generated an avalanche of commentary," few articles focused on Justice Blackmun's description of the scientific process.[314] Imwinkelried's own analysis of *Daubert* contrasted the "traditional popular conception of science," which he described as an idealized Newtonian vision that "scientific propositions are capable of attaining true certainty,"[315] with the "modern conception

311. Evelyn Fox Keller, *The Gender/Science System: Or, Is Sex to Gender as Nature Is to Science?*, *in* THE SCIENCE STUDIES READER, *supra* note 18, at 234, 235.

312. *See supra* note 230 and accompanying text.

313. *See, e.g.*, Farrell, *supra* note 17, at 2198-2207 (describing Blackmun's view as "incoherent"); Mansfield, *supra* note 17, at 10-17 (criticizing *Daubert* majority's reasoning).

314. *See* Imwinkelried, *supra* note 7, at 58.

315. *Id.* at 60.

of science," which acknowledged uncertainty.[316] Inwinkelried then explicated the latter view to include the realization that progress was discontinuous, Kuhn's paradigm theory of scientific revolutions, Popper's falsifiability criterion, the recognition that errors were unavoidable in experimentation, and the inability of statistics based on group data to establish causality in particular cases.[317] Justice Blackmun, we are told, rejected the "'mythical' popular conception of science" in favor of this modern view.[318]

Imwinkelried's representation of two views, one old and one new, suggested a certain stability with respect to the history and philosophy of science that does not exist.[319] Imwinkelried did not, however, purport to describe that field, but rather to describe the conception of science in the contemporary scientific community.[320] His references to the end of the scientific era, the shattering of scientific claims to control everything by understanding natural law, and the acknowledgment of chaotic uncertainty are tempered when the reader realizes that the "disturbing revelations" referred to were simply the features of contemporary scientism.[321] Only the weakest sense of any "social" aspect of science is implied by references to the potential for errors in measurement and flawed research.[322]

Brian Stuart Koukoutchos, who authored an amicus brief in *Daubert* on behalf of "an eclectic group of physicians, scientists, historians of science, and sociologists of science," has also addressed the view of the scientific enterprise adopted in *Daubert*.[323] Koukoutchos contrasted the Ninth Circuit's incorrect assumptions that "science always progresses by the continuous accumulation of objective and irrefutable truths [that are] complete, universal, immutable, and eternal" with the proper view that the history of science is a revolutionary process, that science is a cultural, "socially embedded activity,"[324] that observed facts are conditioned by culture, that theories are imposed on facts, and that there are no whole truths, just half-truths.[325] The latter view acknowl-

316. *See id.* at 60-61.

317. *Id.* at 61-63.

318. *Id.* at 64-65.

319. *See id.* at 58-59 (stating that "the first Section of this article describes the popular, old conception of science . . . . The next Section . . . presents the contrasting, modern conception of science endorsed by Justice Blackmun in *Daubert*").

320. *See id.* at 60 (noting that "the contemporary scientific community no longer subscribes to the popular conception of science").

321. *Id.* at 55-56.

322. *See id.* at 63 (stating that "there can be errors in measurement" and that researcher's "model may be flawed").

323. Koukoutchos, *supra* note 17, at 2237.

324. *Id.* at 2239 (citing STEPHEN J. GOULD, THE MISMEASURE OF MAN 21 (1981)).

325. *Id.* at 2239-40.

edges the limitations of statistical evidence, the danger of consensus as a standard,[326] the mistakes made by the best scientists, and the limitations of peer review and publication as the *bete noir* of good science. The Ninth Circuit Court of Appeals missed these aspects of science.[327]

Again, we count only two views, the outdated vision of science as a producer of certainty and the new view found in *Daubert*. Koukoutchos seemed to view the social embeddedness of science and the effect of theory on facts as errors. In the forum of free, open, and vigorous debate, "the best tonic for bad science is good science"[328] and it is the "*eventual disappearance* of disagreement . . . that marks a field as a science."[329] Koukoutchos was not quite as convinced as Imwinkelried that *Daubert* adopted the correct, modern view of science because the closing paragraphs of Justice Blackmun's opinion "lapsed" into confusing science with truth.[330] The vision of science in *Daubert*, however, is represented as the view received by those who study scientific theory and practice.[331]

In commentary on a progressive opinion like *Daubert*, one expects a contrast between an older position, represented by the history of evidence law (Imwinkelried) or by *Frye* and the Ninth Circuit Court of Appeals's first *Daubert* opinion (Koukoutchos), and the new view adopted by the United States Supreme Court. Yet, any radical or even progressive vision (in Latour's frame of reference) that science is unavoidably a social phenomenon is eclipsed by the representation of two views of science, old and new, available to legal thought.

Professor Farrell's analysis of the "two generalized epistemological positions reflected in" *Daubert* was likewise structured by that opinion.[332] Farrell, however, was much more attuned to developments in the history, philosophy, and sociology of science. Although we criticize the utility of the representation that there are two views in studies of science primarily because of the erasure that it effects on the field, Farrell managed to compress a

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326. *See* Redding, *supra* note 82, at 120-22 (discussing danger of consensus as standard); Redding, *supra* note 69, at 586-88 (providing examples where scientific consensus, which ostensibly debunked law's common wisdom, turned out to be flawed, with law actually outpacing science).

327. Koukoutchos, *supra* note 17, at 2241-51.

328. *Id.* at 2251.

329. *Id.* at 2243 (emphasis added).

330. *See id.* at 2251-53 ("But I detect a lapse in *Daubert* . . . those who seek in science the immutable truth . . . are apt to be disappointed.").

331. *See id.* at 2238 ("All in all, *Daubert* constitutes several steps forward in judicial apprehension of science, and perhaps one step back.").

332. *See* Farrell, *supra* note 17, at 2185 ("This article begins by briefly exploring two generalized epistemological positions reflected in the *Daubert* decision.").

variety of theoretical positions in her two-part analysis.[333] Interestingly, she did not contrast the "old" view of scientific certainty with the "new" view in *Daubert*; instead, she divided the field differently. Farrell reserved the term "scientific positivism" for both the old-fashioned view (that science is a value free inquiry, that "the human mind and the objective world are distinct") and several more modern views (like Popper's falsification standard, as well as his understanding that while "subjective perception mediates" observation, objective knowledge can nevertheless be based on inter-subjective consensus about empirical observations).[334] Farrell then contrasted that framework, which is in play in *Daubert*, with the "constructivist view" that also was part of Justice Blackmun's opinion.[335] In the constructivist camp, she collected the views that "there are no objective, value-free facts," that observation is conditioned by scientific methods and instrumentation, that scientific knowledge is probabilistic, that the history of science is a "succession of superceded theories" (rather than a gradual accumulation of knowledge), and that scientific facts, processes, and even "truths" are social constructs.[336] The positions generalized as "constructivism" include Kuhn's paradigm theory and probalistic approaches, as well as the stronger positions developed by sociologists of science that view scientific validity as based on the power and authority of scientific communities.[337] Because both of these inconsistent perspectives were "unconsciously adopted in *Daubert*," Farrell characterized Justice Blackmun's opinion as incoherent.[338]

In our own analysis of *Daubert*, we argued that the social constructivist view of science was absent from the opinion. So where did Farrell find it? Farrell highlighted Justice Blackmun's valorization of the adversary system and his distinction between legal and scientific fact-finding: science has time to revise, law must act quickly; science searches for cosmic understanding, law must resolve disputes about past events.[339] From this Farrell concluded that Justice Blackmun was beginning – but only beginning – to see that "law constructs facts and legal truths . . . just as science constructs scientific facts and truth."[340] Farrell's point was that law should construct its own truths

333. *See id.* at 2189-98.

334. *Id.* at 2189-93.

335. *Id.* at 2193-98.

336. *Id.* at 2193-96.

337. *Id.* at 2196.

338. *Id.* at 2198-99.

339. *Id.* at 2199.

340. *Id.* at 2204. Farrell explained:

> The [*Daubert*] opinion extolled the virtues of law's adversary system . . . as "the traditional and appropriate means of attacking shaky but admissible [scientific]

rather than follow scientific constructs, because facts in each field serve different purposes. Law is just as prescriptive – and not simply "descriptive" of past events – as science.[341] Justice Blackmun, however, slipped back into scientific positivism when he failed to see that

> falsification is itself socially constructed – that is, whether a methodology can falsify the conclusion will be determined by the standards, equipment, measurement, and error rates agreed upon by those within the scientific community. Thus, falsifiability is as socially dependent as peer review and "generally accepted" factors on prevailing scientific paradigms and the norms of the scientific community that they support.[342]

Because science has no greater claim on truth than law and because law and science should each construct its own facts for its own purposes, Farrell concluded that courts should not automatically defer to scientific conceptions, but rather develop "a set of legal principles for using science's truth in the interests of justice."[343]

Farrell's superb study is notable for its cogent representation of a social constructivist position, one that is genuinely radical in Latour's terminology. In her analysis, science is just like any other cultural activity. Like law, science is an enterprise with power and authority, with theories and methodologies, with presuppositions and rhetorical networks, and with "social institutions . . . and the processes . . . through which . . . legal facts are produced."[344] Without questioning her analysis – she went on to convincingly demonstrate the utility of her proposal for epidemiological evidence[345] – we might return to her generalized description of the "constructionist view." She moved quickly past some older historians of science (e.g., Lakatos and

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> evidence" . . . . But, the court refuses to rely on these truth-seeking, legal methodologies except when the evidence to be tested has already been found scientifically valid. Justice Blackmun might have recognized that law constructs facts and legal truths through these means just as science constructs scientific facts and truth through testing and falsification for its purposes.

Id. (citing Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 596 (1993)).

341. *Id.* at 2204-05. This is, of course, not the same argument made by those who think law should be like science. *See, e.g.*, Nancy Levit, *Listening to Tribal Legends: An Essay on Law and the Scientific Method*, 58 FORDHAM L. REV. 263, 265-66 (1989) (arguing scientific criteria should be applied in law to provide criterion for rationality and validity); John Veilleux, Note, *The Scientific Model in Law*, 75 GEO. L.J. 1967, 1969-71 (1987) (arguing law is like science). Rather, Farrell is suggesting that science is no better than law. *See supra* note 340 and accompanying text.

342. Farrell, *supra* note 17, at 2205.

343. *Id.* at 2217.

344. *Id.* at 2197.

345. *See id.* at 2207-17.

Kuhn),[346] who would not have joined her in the move, to sociologists of science who "have concluded that scientific truths are socially constructed."[347] In the sociology of science, however, some would believe she has gone too far, and some that she has not gone far enough. Peter Galison, for example, found troubling the notion that all of science is interested, which denigrates the role of nature and exaggerates the flexibility of theory.[348] On the other side of Farrell, an even more radical approach recommends challenging "the abstract opposition . . . between immanent or internal analysis, regarded as the province of the epistemologist, which recreates the logic by which science creates its specific problems, and external analysis, which relates those problems to the social conditions of their appearance."[349] In other words, if "in the perspective of social construction we have lost the illusion of an ultimate reference called 'nature,' what do we gain by trying to compensate for this loss with the mirror image of 'society' as a new and insurmountable reference?"[350] That is why Bruno Latour wanted to abandon this frame of reference entirely:

> Since whatever happened had to be either the discovery of nature "out there" or the construction of society "up there," history had to be . . . explained by two lists of ingredients, one coming from nature, the other from society. Now, on the contrary, it is the experimental scene that produces and shapes new actants [things and people] that then increase the long list of ingredients that make up our world.[351]

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346. *See id.* at 2194-95.

347. *Id.* at 2195.

348. *See* GALISON, *supra* note 18, at 11.

349. Pierre Bourdieu, *The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason*, *in* THE SCIENCE STUDIES READER, *supra* note 18, at 31, 33.

350. Rheinberger, *supra* note 268, at 286.

351. Latour, *supra* note 228, at 284. Latour suggested:

> [It] becomes impossible to understand brain peptides without hooking them up with a scientific community, instruments, practices – all impedimenta that bear very little resemblance to rules of method, theories and neurons.
>
> "But then surely you're talking about politics? You're simply reducing scientific truth to more political interests, and technical efficiency to mere strategical manœuvres?" Here is [a] misunderstanding. If the facts do not occupy the simultaneously marginal and sacred place our worship has reserved for them, then it seems they are immediately reduced to pure local contingency and sterile machinations. Yet science studies are not talking about the social contexts and the interests of power, but about their involvement with collectives and objects . . . .
>
> "But if you are not talking about things-in-themselves or about humans-among-themselves, then you must be talking just about discourse, representation, language, texts, rhetoric." This is [another] misunderstanding. It is true that those who bracket off the external referent – the nature of things – and the speaker – the . . . social context – can talk only about meaning effects and language games . . . . [But] when

Latour's disengagement of science studies from social science is both influential and controversial. For our purposes, Latour highlighted one of the major controversies surrounding social constructivism, which is not really a single perspective but an ongoing debate about the nature of scientific activity and progress. Again, we are not criticizing Professor Farrell for taking a position in that debate and explicating it well; our concern is only with her representation of science studies in interdisciplinary legal scholarship. All of the approaches that might be termed "constructivist" share both a rejection of the traditional view that social and psychological factors lead only to erroneous or bad science as well as a common goal of specifying how social relations and processes are implicated in the production of all scientific knowledge. Variation persists, however, as to the particular social processes that deserve the most attention and as to the role of "nature" or reality, if any, in those processes. For example, is it the role of theoretical expectations and communal presuppositions, experimental techniques and practices, the social and professional interests of scientists, or networks of persuasion and negotiation that best explain how facts are manufactured? And does the natural world make no difference or is it ultimately a firewall against the social construction of scientific knowledge?

C. Implications for Law

Two recent legal studies on the use of court-appointed experts, sensitive to the broad range of social studies of scientific activity, help to illustrate the diversity of that field and the differing practical implications for law that may be drawn. The amicus brief filed in *Joiner* by *The New England Journal of Medicine*,[352] discussed in Part V.B above, was an argument for the expanded use of court-appointed experts as permitted by Federal Rule of Evidence 706. Justice Breyer, in his concurrence in *Joiner*, quoted heavily from that brief and encouraged judges to take advantage of this procedure to assess the quality of expert scientific testimony proffered at trial.[353] The notion is attractive because it suggests neutrality and disinterestedness in the face of interested testimony prepared for one side in a trial. For those who have come to question whether science is ever neutral and disinterested, does this repre-

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> I describe the invention-discovery of brain peptides, I am really talking about the peptides themselves, not simply their representation . . . . Yet rhetoric, textual strategies, writing, staging, semiotics – all these are really at stake, but in a new form that has a simultaneous impact on the nature of things and on the social context, while it is not reducible to one or the other.

LATOUR, *supra* note 81, at 4.

352. *See* Amicus Brief, *supra* note 232 (supporting neither party).

353. *See* General Elec. Co. v. Joiner, 522 U.S. 136, 149-50 (1997) (Breyer, J., concurring).

sent any sort of solution to the quest for stable scientific knowledge for legal processes and institutions?

Professor Deason recently highlighted the potential for bias and undue deference in the use of court-appointed experts.[354] She referred to the "quaint" notion of scientific certainty, sufficiently challenged by the manner in which "scientific conclusions have been abandoned, modified, or transcended in the last century":[355]

> Today there is still an appreciation that the goal of science is objective inquiry, but there is also a growing awareness that science, like other human endeavors, takes place in a social and political context. The answers scientists offer depend in part on the questions they ask, and the questions they ask are not only driven by the current state of development of scientific knowledge but are also related to social norms and personal interests.[356]

An expert, even one not representing or sponsored by a party in a lawsuit, holds views "shaped by the prevailing culture . . . that will affect her research design, her interpretation of the data, or her testimony [and that are] influenced by personal attitudes."[357] Scientists are influenced by preconceptions, gender stereotypes, financial connections, and academic interests. Accordingly, any judicial deference to so-called neutral scientists should be guarded.[358] Nevertheless, Deason did not "contend that influences such as attitude toward gender determine the outcome of experiments or that there is no such thing as objectivity. Reliable, replicable results are still important. The argument is instead that scientific knowledge is limited by cultural interests and attitudes."[359] Deason wavered between a weak constructivist position, because "every scientist is a human being,"[360] and a weak objectivist theory, because scientific knowledge presents a "partial picture" of reality "formed in a particular context of social values."[361]

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354. *See* Deason, *supra* note 17, at 98-99; *see also* Jack B. Weinstein, *Science, and the Challenges of Expert Testimony in the Courtroom*, 77 OR. L. REV. 1005, 1015 (1998) (noting that experts may represent only majority views disadvantageous to clients without economic resources who cannot retain experts to represent minority views).

355. Deason, *supra* note 17, at 99.

356. *Id.* at 100.

357. *Id.* at 101-02.

358. *Id.* at 101-16.

359. *Id.* at 107.

360. *Id.* at 113 (quoting Eliott Marshall, *When Does Intellectual Passion Become Conflict of Interest?*, 257 SCIENCE 620, 621 (1992)).

361. *Id.*

A bolder constructivist view of science is evident in Browne, Keeley, and Hier's recommendation for the broader use of court-appointed experts.[362] They took the view that the scientific enterprise is not simply externally influenced by social forces, but that the "social" is "imminent in the process itself, by which we mean that scientists are not disinterested agents but rather are immersed in a web of relations that play an important role in determining the character of truths that emerge from their interactions."[363] Facts and theories are determined in part by "rhetorical struggles and scrambles for resources and avid quests for collegial acceptance" of scientific claims.[364] Scientists take proactive steps to turn their claims into unquestioned facts, "making 'allies' out of facts, people, money, theories, and organizations in the process."[365] The elites formed within scientific disciplines subtly suppress dissent and enforce disciplinary boundaries, such that when knowledge is presented in courts of law, for example, it might seem irrefutable to a judge or jury.[366] In their view, a court-appointed expert might somehow keep a marginalized viewpoint alive by introducing a contrary voice.[367] That is not the conclusion we would have expected from social constructivists – we were thinking something like "court-appointed experts will likely be leaders in the field who will further the hegemony of mainstream science." Justice Breyer in *Joiner*, after all, was enamored of the suggestion that "reputable experts could be recommended to courts by established scientific organizations, such as the National Academy of Sciences or the American Association for the Advancement of Science."[368] That hardly seems a "strategy for overcoming the prescription against [a] new and different idea or perspective"[369] or an acknowledgment that "expertise is inescapably human,"[370] unless one presumes that such organizations are not coterminous with "elites," a presumption difficult to sustain.

The use of court-appointed expert panels or science courts, proposed by a variety of scholars,[371] does generally represent the view that there is a majoritarian "correct" science. Thus, it is strongly positivist in orientation. Under this framework,

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362. *See generally* Browne et al., *supra* note 17.

363. *Id.* at 50.

364. *Id.* at 51.

365. *Id.* at 56.

366. *See id.* at 57-70 (discussing role of elites as scientific gatekeepers).

367. *Id.* at 70.

368. General Elec. Co. v. Joiner, 522 U.S. 136, 150 (1997) (Breyer, J., concurring).

369. Browne et al., *supra* note 17, at 65.

370. *Id.*

371. *See generally* Arthur Kantrowitz, *Proposal for an Institution for Scientific Judgment*, 156 SCIENCE 763 (1967); Kapsa & Meyer, *supra* note 220, at 322.

> the results of scientific research are positive in character, just as other materials are classified as fact. In this sense, the results of science are indeed similar to factual reports about the time of day, weather conditions, stop light color, or the existence of a signature on a contract.... Neither have a normative component.[372]

The most influential proposal for court-appointed experts will surely be that of Professors Walker and Monahan, who discussed the recent use of a National Science Panel appointed by a federal trial judge in the breast implant litigation and provided a legal framework for the appointment and use of expert science panels.[373] They proposed that science panel conclusions be treated as law-like precedent[374] with respect to generalizable research results (e.g., that silicone breast implants do not cause disease) and that the trial judge make "conclusions of law" based on panel findings. The law-like conclusion (e.g., general causation) would be provided in the jury instructions, with the jury adjudicating case-specific issues (e.g., specific causation).

The science panel paradigm fails to accommodate (1) the notion that both majority and minority views may be good science and (2) the social constructedness of science, which is particularly problematic in the social sciences. In most cases, deference to expert panel findings on scientific questions will produce greater accuracy than the decisions made by judges or jurors who lack scientific expertise[375] and are informed through the testimony of less well-trained "experts." But this comes at a cost – reducing the variability and dynamism across cases severely limits the opportunities for the adversarial testing of diverse scientific evidence and experts across cases and over time. Relying on expert panels not only limits the potential for law to construct and inform science,[376] but also entrenches in law the established power structures and majoritarian biases in science. When judges and the expert panels maintain control over scientific knowledge in the courts, elite scientific bodies become the gatekeepers of scientific evidence. Science panel

372. Laurens Walker & John Monahan, *Scientific Authority: The Breast-Implant Litigation and Beyond*, 86 VA. L. REV. 801, 817-18 (2000). *See generally* MARCIA ANGELL, SCIENCE ON TRIAL (1996).

373. *See* Walker & Monahan, *supra* note 372, at 821, 828 (discussing use of expert scientific panels).

374. *Id.*; *accord* Michael J. Saks, *The Aftermath of* Daubert: *An Evolving Jurisprudence of Expert Evidence*, 40 JURIMETRICS 229, 232-33 (2000). *But see* Alan J. Tompkins & Joe S. Cecil, *Treating Social Science Like Law: An Assessment of Monahan and Walker's Social Authority Proposal*, 2 SHEPHARD'S EXPERT & SCI. EVID. Q. 343, 378-84 (1994) (arguing that it is problematic to treat research findings as legal authority because stare decisis prevents that authority from keeping pace with changing research findings).

375. *See* Scott Brewer, *Scientific Expert Testimony and Intellectual Due Process*, 107 YALE L.J. 1535, 1535 (1998).

376. *See generally* Redding, *supra* note 69 (discussing how law shapes scientific discourse).

evaluations of scientific evidence is similar to the journal peer-review process, which is a process not without substantial biases and flaws. Studies have shown that reviewers' publication recommendations are substantially influenced by their own theoretical and methodological biases and there often can be considerable disagreement between reviewers.[377] Therefore, whether a journal accepts a paper for publication may in part depend on who the reviewers happen to be.

Without regard to Deason's caution about court-appointed experts or their utility as seen by Browne, Keeley, and Hiers, both accounts rest on a dualistic account of the history, philosophy, and sociology of science. The authors presented the "new" view, which is not the same in the two articles, as a single modern perspective. They downplayed disagreements within the field of the history, philosophy, and sociology of science to make the argument clear and to stabilize the interdisciplinary borrowing. Again, the "fairy tale" view of science as objective and its history as a linear progression toward truth apparently has been replaced by a new vision of science.[378] However, if a judge decided to read up on this new view of science by seriously engaging the works cited in the two studies above, he or she might conclude that the praised insights are not there. Rather, he or she would find debates and dead ends in a disciplinary field that is as unsettled as law or politics.

In terms of interdisciplinarity, what insights can we import into law from the dynamic field of the history, philosophy, and sociology of science? As discussed in the next section, while courts have strong social constructivist views of science, some have developed a weak constructivist approach – a "pragmatic constructivism" – in response to the complex debates surrounding the nature and philosophy of science.

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377. *See generally* Fiona Godlee et al., *Effect on the Quality of Peer Review of Blinding Reviewers and Asking Them to Sign Their Reports*, 280 JAMA 237 (1998); Redding, *supra* note 69, at 594 (discussing scientists' biases).

378. Deason, for example, stated:

> The scientific view of the world has changed .... Certainly there is no general belief that scientific professionals are "disinterested." Today there is still an appreciation that the goal of science is objective inquiry, but there is also a growing awareness that science ... takes place in a social and political context.
>
> ....
>
> [Kuhn] revolutionized how we view science .... We now speak of paradigms.

Deason, *supra* note 17, at 99-100, 116. Browne, Keeley, & Hiers, on the other hand, readily conceded that the unified perspective they presented differed from contemporary positivist and realist accounts of science. The authors smoothly combined Rouse, Fleck, Yonay, Gieryn, Fuchs & Pfeffer into a compelling alternative account of science without highlighting the diversity of perspectives in science studies. *See* Browne et al., *supra* note 17, at 51-65.

VII. Pragmatic Legal Constructivism

A. The Status of Social Constructivism in Law

> In any truly public battle, those arguing for constructivism in general will lose to those arguing for reality in general.[379]

The journal *Science Communication* recently published one of the most important studies of how social constructivism is represented in the discipline of law.[380] Its authors, Gary Edmund and David Mercer, are scholars in law and in the sociology of scientific knowledge. They argued that, contrary to the impression given by the citations in *Daubert* and by numerous legal scholars, social constructivism has not been accommodated in American legal discourse.[381] Rather, the insights available from the sociology of scientific knowledge have been misappropriated and thereby excluded from legal theorizing about the nature of scientific activity.[382]

Professor Jasanoff, therefore, risked overstatement when she identified a post-*Daubert* "tolerance of multiple viewpoints about how science works."[383] One of the important amicus briefs filed by the Carnegie Commission on Science, Technology, and Government in *Daubert* cited a "small body of work in the social studies of science," including some of Jasanoff's writing on law and science, but only made references to: (1) the extremism of her constructivist perspective, (2) her view that science is provisional (a view shared by non-constructivist briefs), (3) her acknowledgment of the limitations of peer

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379. George Levine, *What Is Science Studies For and Who Cares?* 46-47 SOC. TEXT 1, 126 (1996).

380. *See* Gary Edmond & David Mercer, *Representing the Sociology of Scientific Knowledge and Law*, 19 SCI. COMM. 307 (1998).

381. *Id.* at 308-09. For example, FEDERAL JUDICIAL CENTER, REFERENCE MANUAL ON SCIENTIFIC EVIDENCE (1994)), which now sits on the bench of every federal judge in the United States, contains no hint of the constructivist view. Nor do the highly influential volumes FAIGMAN ET AL., MODERN SCIENTIFIC EVIDENCE, *supra* note 4, and ANDRE A. MOENSSENS ET AL., SCIENTIFIC EVIDENCE IN CIVIL AND CRIMINAL TRIALS (4th ed. 1995). Rather, these treatises all have a strongly positivist orientation, deferring to empirical scientific methodology as the *sine qua non* of good (and thus legally admissible) science. As Professor Levine pointed out in his review of *Modern Scientific Evidence*: "If I were to sum up their viewpoint, I would say the author-editors disapprove of testimony deriving from any field that lacks a body of literature ruling out plausible rival hypotheses in well-designed experiments that allow for the determination of a precise error rate." Murray Levine, *"The Legal Culture Must Assimilate the Scientific Culture" and Vice Versa?*, 21 LAW & POL'Y 77, 83 (1999). Thus, scientific innovation may be discouraged. *See id.* at 87 (noting that "if judges use Shari Diamond's description of what constitutes a good survey, those offering survey data as part of expert testimony will have to meet the best standards in the field").

382. *See* Edmond & Mercer, *supra* note 380, at 308-9.

383. *Id.* at 308 (citing Sheila Jasanoff, *Beyond Epistemology: Relativism and Engagement in the Politics of Science*, 26 SOC. STUD. SCI. 393, 408 (1996)).

review as a standard for good science (a view which does not rely on constructivist accounts), and (4) her view that science is dynamic and facts indeterminate.[384] The latter was not used in the brief to draw a constructivist conclusion, but rather to conclude that courts should "concern themselves with acceptable scientific methodology" rather than choose between hypotheses.[385] Justice Blackmun cited Jasanoff in *Daubert*, but only for the proposition that publication "does not necessarily correlate with reliability"[386] – nowhere "else in the judgment is Jasanoff's work, or any other constructivist perspective, referred to."[387] Not only was constructivism not accommodated, but the Court cited Jasanoff to support "the anti-constructivist perspective of Popper's falsification."[388] According to Edmond and Mercer, by the time *Daubert* was on remand to the Ninth Circuit Court of Appeals any constructivist component of Justice Blackmun's opinion was lost. Instead, the criterion of general acceptance (peer review and publication) along with a new preference for research independent of litigation dominated the court of appeals' analysis.[389]

Edmond and Mercer also surveyed legal scholarship to reinforce their thesis that constructivism has not been accommodated in law and science discourse.[390]

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384. *See id.* at 310-12.

385. *Id.* (quoting Jasanoff, *supra* note 383, at 408).

386. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593 (1993).

387. Edmond & Mercer, *supra* note 380, at 312.

388. *Id.*

389. *See id.* at 313-14 (discussing Ninth Circuit Court of Appeals's application of *Daubert* standard).

390. *See id.* at 314-18. The courts' pragmatism does, however, implicitly acknowledge at least one aspect of the constructivist argument: There is no preinterpretive science, particularly when it comes to science used in the adversary process of litigation. Science loses its claim to value neutrality or objectivism when put in the service of helping to resolve legal or policy disputes. Science may describe reality, but science cannot prescribe how empirical reality can or should inform legal policy. Rather, science in legal disputes is used as part and parcel of a larger narrative molded by the litigants and factfinders.

> Changing the narrative can require shifts in evidence . . . [w]hat is deemed as "evidence" or scientific is susceptible to redefinition in line with the entire narrative. The evidence influences the narrative but the narrative simultaneously structures the status of the evidence. . . . The various narrative accounts are often created alongside relevant scientific knowledge. The narratives are designed to accommodate, favorably frame and utilize the scientific evidence. In turn they provide a framework which gives the evidence meaning.

Gary Edmond, *Science, Law and Narrative: Helping the 'Facts' to Speak for Themselves*, 23 S. ILL. U. L.J. 555, 580-83 (1999).

Through pragmatic constructivism, courts implicitly acknowledge their inability to separate "science" from the overall legal and policy narrative within which it is embedded. Federal courts, therefore, might not "apply" or "interpret" the *Daubert* guidelines; rather, they "perform"

> Predominantly, Jasanoff's work is cited [only] as a critique of peer review and as a source for evidence on such problems as scientific fraud, judges inappropriately constructing science in courts . . . , and the power of courts to expose junk science . . . .[391]

Jasanoff's insight that courts participate in the social construction of scientific truth is generally misunderstood by courts and legal scholars or seen as problematical.[392] While a minority of commentators seem to appreciate Jasanoff's constructivist arguments, "awareness" does not usually "equate with alignment or accommodation."[393] Edmond and Mercer concluded that constructivism is selectively misappropriated rather than accommodated. To explain the court's reluctance to accommodate constructivism, Edmond and Mercer pointed to the legal system's own "self-image suggesting that legal practice is similar to ideal images of science, [and] that its practices are based on the objective discovery of facts and impersonal application of rules."[394]

Consider, however, Professor Farrell's view that lawyers and judges are well aware of the way in which law constructs its own knowledge.[395] Rather

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Daubert, much in the same way actors act out a play. *See* Denis J. Brion, *Performing the Constitution*, 49 WASH. & LEE L. REV. 293, 294 (1992) (looking at Constitution as performance document to be acted out in different ways at different times). In this conception, the performance shows us that "science" as used in law is not something inherent in positivism nor is it a social construction – "[it] is the product of the performance of the text." *Id.* Since no two performances are the same, the result is a pluralistic interpretation of science in the courts which serves to protect against scientific hegemony. Here, Quine's principle of the "web of belief" is significant – that the truth of any one proposition is dependent upon its relationship to other propositions; a change in any one part of the web of belief can change the entire belief system or constituent propositions. *See* Michael A. Livingston, *Postmodernism Meets Practical Reason*, 107 YALE L.J. 1125, 1133 n.32 (1998) (presenting Quine's description of science as web of belief). No two performances of *Daubert*, as applied to a particular body of scientific evidence, will be exactly the same – the web of belief and constituent propositions fluctuate.

391. Edmond & Mercer, *supra* note 380, at 314-15.

392. *Id.* at 315.

393. *Id.* at 318.

394. *Id.* at 319. *See generally* Howard Schweber, *The "Science" of Legal Science: The Model of the Natural Sciences in Nineteenth-Century American Legal Education*, 17 L. & HIS. REV. 421 (1998) (discussing "version of American legal science that looked to the natural science for its metaphors, its methods, and its legitimating claims").

395. *See* Farrell, *supra* note 17, at 2197. Farrell stated:

> The legal community may be especially self-conscious about the way in which it socially constructs facts and the reasons it does so. Lawyers, judges, and legal scholars identify and evaluate endlessly the standards and procedures they use to give legal effect to factual propositions. They are conscious of the social institutions (courts, judges, administrative tribunals, and legislatures) and the processes (evidentiary hearings, judicial notice, precedent, etc.) through which the legal facts are produced.

Id.

than seeing an affinity, as Edmond and Mercer suggested, "between legalistic discourses and scientific discourses in their shared appeal to simplistic empiricist accounts of knowledge creation,"[396] Farrell highlighted the social constructivism that is readily embraced among lawyers, judges, and legislators for law, but not science.[397] This might explain why Edmond and Mercer say Farrell did not embrace constructivism.[398] However, Farrell certainly embraced the view that courts produce or co-produce scientific knowledge alongside some scientific evidence.

While we agree with Edmond and Mercer that judicial and legal scholarly literature do not include genuinely constructivist accounts of science, we find a weak or "pragmatic" form of "legal construction of science" in *Daubert* and in many lower federal court opinions applying *Daubert*. Briefly, the search for scientific truth by scientists, whatever its level of certitude and utility outside of law, is set aside in favor of a pragmatic view of scientific truth as useful in a legal controversy. Justice Blackmun acknowledged that it is not "cosmic understanding" but a "particularized resolution" that the law needs. Thus, even if "authentic insights and innovations" are occasionally missed,[399] lower court federal judges are permitted to construct a little scientific knowledge in court.

In his dissent in *Joiner*, Justice Stevens criticized the majority for its restrictive interpretation of *Daubert*. The Court found no abuse of discretion in the trial judge's exclusion of expert testimony, which Justice Stevens would have considered permissible scientific evidence:

> It is not intrinsically "unscientific" for experienced professionals to arrive at a conclusion by weighing all available scientific evidence – this is not the sort of "junk science" with which *Daubert* was concerned. After all, . . . the [EPA] uses the same methodology to assess risks . . . .[400]

Justice Stevens then highlighted the pragmatic approach to scientific knowledge at trial by emphasizing that it would *not* "have been an abuse of discretion to admit the expert testimony."[401] In other words, the "gatekeeping" standards are flexible and are not tied to extra-legal scientific standards.

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396. Edmond & Mercer, *supra* note 380, at 319.

397. Lawyers, judges, and legal scholars "recognize that the legitimacy or validity of legal facts depends upon whether the processes through which they are found conform with standards agreed upon, formally and informally, within the legal community – broadly defined to include its official and unofficial political institutions." Farrell, *supra* note 17, at 2197.

398. *See* Edmond & Mercer, *supra* note 380, at 318.

399. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 597 (1993).

400. General Elec. Co. v. Joiner, 522 U.S. 136, 153 (1997) (Stevens, J., concurring in part and dissenting in part).

401. *Id.* at 155 (Stevens, J., concurring in part and dissenting in part).

Again in *United States v. Scheffer*,[402] Justice Stevens dissented when the Court confirmed the constitutionality of a military rule of evidence prohibiting polygraph evidence in court-martial proceedings by finding that there is no consensus regarding the reliability of such evidence.[403] Because "a host of studies . . . place the reliability of polygraph tests at 85% to 90%," Justice Stevens argued that its usefulness ought to be tested in adversary proceedings.[404] Thus, in *Kumho Tire Co. v. Carmichael*, Justice Stevens concurred with the majority's explication of the flexibility of the *Daubert* gatekeeping role for federal judges, who must have "considerable leeway" and "latitude" when assessing an expert's reliability.[405]

In sum, the form of constructivism that has developed in judicial law-and-science discourse is not "full-blown" social constructivism, which calls into question scientific objectivity, but legal-social constructivism that "brackets" scientific objectivity as too complex for courts to depend upon. Thus, without taking a position on social constructivism, or by taking a wishful position that science is relatively objective but not always accessible to law, some federal courts have adopted a pragmatic constructivism to avoid questioning the nature of scientific discovery and progress. Whether such courts have successfully avoided a philosophical stance depends in part on one's view of pragmatism generally. Pragmatists think they avoid over-daring theories, while critics think that pragmatism is often a guise for undisclosed or even unwitting theoretical commitments. We tend toward the latter view, thus we argued that the philosophy of science is unavoidable in judicial applications of *Daubert* guidelines to determine the admissibility of scientific evidence. On the other hand, there is no difference in practice between pragmatic constructivists who are (or think they are) avoiding theory and those who adopt (or unwittingly adopt) that "constructivist" position (1) because science is socially constructed or (2) because the best science is not always accessible to law.

B. Law's Alternative: Pragmatic Socio-Legal Constructivism

> Scientific conclusions are subject to perpetual revision. Law, on the other hand, must resolve disputes finally and quickly.[406]

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402. 523 U.S. 303 (1998).

403. United States v. Scheffer, 523 U.S. 303, 320-39 (1998) (Stevens, J., dissenting).

404. *Id.* at 333, 335 (Stevens, J., dissenting).

405. *See* Kumho Tire Co. v. Carmichael, 526 U.S. 137, 152 (1999); *id.* at 159 (Stevens, J., concurring in part and dissenting in part).

406. *Daubert*, 509 U.S. at 597.

> [T]he law works not by identifying and then hewing to some overarching set of principles, or logical calculus, or authoritative revelation, but by deploying a set of ramshackle and heterogeneous resources in an effort to reach political resolutions of disputes that must be framed . . . . By the standards applied to determinate and principled procedures, the law fails miserably . . . but by the pragmatist standard . . . the law gets passing and even high marks because it *works*.[407]

In Part IV above, we identified a "liberal" philosophical perspective based on (1) viewing the *Daubert* factors as flexible, (2) keeping a strong distinction between conclusions and methodology, and (3) acknowledging the possibility of conflicting, admissible scientific testimony, which roughly ends up emphasizing scientific uncertainty and the utility of novel science in the same way that progressive philosophies of science do. We contrasted that with a "conservative" philosophical perspective based on (1) viewing the *Daubert* factors as guidelines, (2) collapsing the conclusions/methodology distinction, and (3) downplaying the likelihood that two conflicting scientific theories can both be good science, which roughly ends up emphasizing the stability of mainstream scientific knowledge in accordance with traditional philosophies of science. While the pragmatic constructivist approach may seem to reflect the liberal perspective, it is oddly consistent with a traditional view of science as a producer of stable knowledge on its own turf, in its own time. Because the law's need for science is unpredictable, so is the science that may be available when law calls.

Chief Judge Posner, in *Rosen v. Ciba-Geigy Corp.*,[408] explained that "the courtroom is not the place for scientific guesswork, even of the inspired sort. 'Law lags science,' it does not lead it."[409] It bears mentioning that *Rosen* does not interpret *Daubert* to admit novel scientific testimony.[410] To the contrary, the proposition that law lags science tends to restrict science to what is

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407. Stanley Fish, *Almost Pragmatism: The Jurisprudence of Richard Posner, Richard Rorty, and Ronald Dworkin, in* THERE'S NO SUCH THING AS FREE SPEECH AND IT'S A GOOD THING, TOO 200, 209 (1994).

408. 78 F.3d 316 (7th Cir. 1996).

409. Rosen v. Ciba-Geigy Corp., 78 F.3d 316, 319 (7th Cir. 1996). Similarly, Professor Milich emphasized that law must not outpace science. *See* Milich, *supra* note 240, at 915 (stating "law's faith in science must not race ahead of science itself"). But law can usefully inform scientific research. *See generally* Redding, *supra* note 69 (discussing how common-sense psychology inherent in law can inform social science, and ways in which law has informed and actually outpaced scientific progress in certain areas of social science).

410. *See Rosen*, 78 F.3d at 320 ("The district court was within its discretion in concluding that the scientific evidence of causation that the plaintiff offered was not admissible . . . .").

"generally accepted."[411] When scientific controversies arise, however, the so-called "default" position is not exactly deferential to the mainstream scientific community. Rather, some courts may reject scientific propositions supported by substantial evidence if that evidence is not convincingly presented at trial. Alternatively, professional opinions and "shaky" science "not yet accepted as canonical" can be admitted.[412] The level of reliability required in science may exceed that required by law, and a mass of less-than-reliable scientific evidence may produce a rather reliable sum total of evidence for law.[413] Conversely, scientifically reliable research studies may produce statistically "significant" (i.e., reliable) findings that have little or no practical or social significance. This significance is determined not by whether there are statistically reliable differences but by whether such differences are sizeable enough that they actually matter in the real world. The degree to which courts deem research to be "valid" or "reliable" depends upon the purpose and circumstances for which it is used. Thus, validity and reliability are conditional and contingent, not fixed and unitary:

> *[A]ll* researchers – whether they work with numbers or words, in the laboratory or in the field – must grapple with issues of generalizability, validity, replicability, ethics, audience, and their own subjectivity or bias. Moreover, all researchers must engage questions of authority and interpretation. Whether numbers or words, data do not speak for themselves. They acquire meaning only within the framework(s) of theory and interpretation imposed by researchers.[414]

While critical self-reflection concerning values and inter-subjectivity is common in qualitative research endeavors, it is rare in quantitative research studies deemed "scientific."[415] Yet, the stories of lived experiences found in qualitative research are precisely the kind of storytelling long approved in the tribunals of law. When courts construct a set of facts for the adjudication of a particular case, those facts may or may not correspond to an objective, veridical reality. Pragmatic constructivism, consistent with pragmatism generally, also seems to view truth as relative to its perceived consequences. This potentially limits the tendency for courts to assume, via the veil of "objective"

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411. *See id.* at 319 ("But the courtroom is not the place for scientific guesswork, even of the inspired sort.").

412. *See id.* at 318 (finding evidence admissible even if based on novel methods); *id.* at 319 ("There may be evidence to back up [the] claim, but none was presented to the district court.").

413. *Id.* at 318.

414. Jeanne Marecek et al., *Working Between Worlds: Qualitative Methods and Social Psychology*, 53 J. SOC. ISSUES 631, 632 (1997).

415. *See id.* at 635.

science,[416] "ultimate power to interpret another's experience . . . by virtue of the prestige, status, and authority afforded to [academic researchers] . . . to define the right, the good, the best, the ideal."[417]

Recall that in *In re Paoli R.R. Yard PCB Litigation*, the court emphasized the screening function of the trial judge:

> The evidentiary requirement of reliability is lower than the merits standard of correctness . . . . The grounds for the expert's opinion merely have to be good, they do not have to be perfect. The judge might think that there are good grounds for an expert's conclusion even if the judge thinks that there are better grounds for some alternate conclusion, and even if the judge thinks that a scientist's methodology has some flaws such that if they had been corrected, the scientist would have reached a different result.[418]

Thus, the view that the proper inquiry is "what experts in relevant discipline deem it to be" has been rejected in favor of the gatekeeping role for federal judges.[419] Moreover, review of trial court admissibility determinations remain crucial because "there is a significant risk that district judges will set the threshold too high."[420]

This is not to suggest that federal trial and appeals courts do not tie their evidentiary standards to the *Daubert* definitions of valid scientific methodology. In *Summers v. Missouri Pacific Railroad System*,[421] the Tenth Circuit Court of Appeals affirmed the exclusion of testimony that was not based on "acceptable scientific levels of methodology and criteria" and was based on unvalidated preliminary research.[422] Similarly, in *Belofsky v. General Electric Co.*,[423] a district court judge granted a motion to exclude expert testimony that was not based on adequate scientific methods. Indeed, the court failed to find "any method in [the expert's] madness."[424] Finally, in *Target Market Publishing v. ADVO*,[425] the Seventh Circuit Court of Appeals affirmed the exclusion

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416. Law, of course, offers up the same veil of disinterestedness. *See* Michael D. Weiss, *A Jurisprudence of Blindness: Rawls' Justice and Legal Theory*, 42 DRAKE L. REV. 565, 566 (1993) (discussing veil of ignorance pierced by legal realist and critical legal studies movements).

417. Mark B. Tappan, *Interpretive Psychology: Stories, Circles, and Understanding Lived Experience*, 53 J. SOC. ISSUES 645, 655 (1997).

418. *In re* Paoli R.R. Yard PCB Litig., 35 F.3d 717, 744 (3d Cir. 1994).

419. *Id.* at 747, 748.

420. *Id.* at 750 (discussing risk of too high threshold due to amorphous "reliability standard of Rules 702 and 703").

421. 132 F.3d 599 (10th Cir. 1997).

422. Summers v. Missouri Pac. R.R. Sys., 132 F.3d 599, 604 (10th Cir. 1997).

423. 980 F. Supp. 818 (D.V.I. 1997).

424. Belofsky v. General Elec. Co., 980 F. Supp. 818, 823 (D.V.I. 1997).

425. 136 F.3d 1139 (7th Cir. 1998).

of evidence based on "assumptions that [did] not legitimately support the conclusion."[426]

Nevertheless, in *Cabrera v. Cordis Corp.*,[427] the Ninth Circuit Court of Appeals quoted *Daubert* for the proposition that soundness of methodology, not the correctness of the expert's conclusions, is the test for admissibility [428] (even though the appeals court affirmed the exclusion of testimony as unreliable and based on "underground knowledge, untested and unknown to the scientific community").[429] In *Ruiz-Troche v. Pepsi Cola of Puerto Rico Bottling Co.*, the First Circuit Court of Appeals reversed the trial court's exclusion of expert testimony, confirming that judges need not decide between methodologically sound scientific theories in conflict.[430] Requiring scientific certainty "solicits a level of assurance that science realistically cannot achieve and that *Daubert* does not demand;" such a standard "changes the trial judge's role . . . from that of gate-keeper to that of armed guard."[431] The Fifth Circuit Court of Appeals en banc opinion in *Moore v. Ashland Chemical Inc.*[432] also emphasized scientific uncertainty and the difference "between truth-seeking in the courtroom and in the laboratory."[433] Reliability is not correctness,[434] and "the law cannot wait for future scientific investigation and research."[435] While the majority affirmed the exclusion of evidence as not grounded in science, a strong dissent questioned whether the application of *Daubert* to clinical testimony was too restrictive:

> The *en banc* majority adopts a mechanistic interpretation of the *Daubert* factors that threatens to require the exclusion from evidence of vast numbers of clinical medical opinions, although they are generally accepted as trustworthy by physicians practicing in their fields . . . .[436]

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426. Target Mkt. Publ'g v. ADVO, 136 F.3d 1139, 1144 (7th Cir. 1998).

427. 134 F.3d 1418 (9th Cir. 1998).

428. *See* Cabrera v. Cordis Corp., 134 F.3d 1418, 1421 (9th Cir. 1998) (quoting Daubert v. Merrell Dow Pharms., Inc., 43 F.3d 1311, 1318 (9th Cir. 1995)).

429. *Id.* at 1423.

430. Ruiz-Troche v. Pepsi Cola of P.R. Bottling Co., 161 F.3d 77, 85 (1st Cir. 1998).

431. *Id.* at 86.

432. 151 F.3d 269 (5th Cir. 1998).

433. Moore v. Ashland Chem. Inc., 151 F.3d 269, 275 (5th Cir. 1998) (quoting Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 597 (1993)), *cert. denied*, 526 U.S. 1064 (1999).

434. *Id.* at 276 ("The proponent need not prove to the judge that the expert's testimony is correct, but she must prove by a preponderance of the evidence that the testimony is reliable.").

435. *Id.*

436. *Id.* at 286 (Dennis, C.J., dissenting).

In the dissent's view, "hard" science was not the only science to which parties may appeal.[437]

While we maintain that there is no escape from the inevitability of taking, even unwittingly, a philosophical position on the nature of the scientific enterprise, the particular position taken is rendered almost irrelevant in the pragmatic constructivist mode of analysis. A judge may think that science always operates in an influential social context, or that the best science rises above social determinants in its access to nature or reality. Courts, however, can set aside both positions to focus on legal science, which is constructed in court by experts who bring the best that they have to court that day.

The pragmatism used by courts in interpreting and applying scientific evidence reflects the interpretive community in which courts operate.[438] As Stanley Fish argued, evidence is given meaning through the interpretive community (comprising shared values, biases, beliefs, and assumptions) of which the factfinder is a part: there is no ultimate truth, only local truths.[439] We cannot do objective science because "the mental operations we can perform are limited by the institutions in which we are already embedded."[440] It is not surprising that different courts and jurisdictions may produce differing results – differing pragmatic constructions of the very same scientific evidence, because each interpretive community constructs its own truth from the scientific evidence at hand. In a variety of ways, these constructions can usefully inform empirical science.[441] Experiential data often drive hypothesis formulation in science, with preliminary observations and theories having significant "common-sense" validity before they are confirmed through rigorous scientific testing.

Given the courts' pragmatic approach to constructing a science for the case, we return to the status of social constructivism in law's interdisciplinary borrowings.

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437. *See id.* at 281 n.2 (Dennis, C.J., dissenting) ("Where the expert does not propose to testify to an opinion based on hard scientific methodology, [*Daubert*] indicated that the reliability of his opinion should be assessed according to the methodology of the expert's own discipline.").

438. *See* BRIAN Z. TAMANAHA, REALISTIC SOCIAL-LEGAL THEORY: PRAGMATISM AND A SOCIAL THEORY OF LAW 142-52 (1997) (arguing law represents consensus about need to apply coercion to support particular norm in given situation).

439. *See generally* STANLEY FISH, IS THERE A TEXT IN THIS CLASS? THE AUTHORITY OF INTERPRETIVE COMMUNITIES (1980).

440. *Id.* at 331.

441. *See* Redding, *supra* note 82, at 118-36 (discussing ways in which common sense or received wisdom inherent in law can inform social science research).

C. *Interdisciplinarity and Social Constructivism*

According to Professor Farrell, courts and commentators have retreated to or remained in a positivist framework and have not adopted the view that law and science are both socially-constructed enterprises. Similarly, in accordance with the assessment of Edmond and Mercer, courts and commentators have never really accommodated constructivist views of the scientific enterprise. But post-*Daubert* jurisprudence on the relations between law and science is not so easily characterized either as positivist or constructivist. Such a dualism tends to hide the pragmatic form of constructivism that has been celebrated since *Daubert* loosened the *Frye* link between mainstream science and law. Although courts have not seriously questioned the positivist features of science, the pragmatic constructivism of the courts – constructing a "science for the case" out of proffered expert testimony – is comfortably acknowledged in post-*Daubert* jurisprudence. Farrell is correct that a social constructivist approach to science has not taken hold in law, and Edmond and Mercer are correct that scientific constructivism has been misappropriated and misunderstood. But the positivism that these scholars set up in opposition to constructivist views is not the only alternative. Rather, a pragmatic legal constructivism, which would not satisfy social constructivists, arose from the mildly critical view that science is a dynamic and uncertain enterprise.

Significantly, this alternative position indirectly reflects the controversies within the fields of the history, philosophy, and sociology of science. A lively debate continues in the scholarly literature about the nature of scientific practices. The debate is not between old-fashioned objectivists (though there are a few) and social constructivists who believe that scientific facts are manufactured entirely out of and are explainable wholly by reference to social relations. Instead, it is between those who readily concede the social aspects of science, but cannot quite decide what role "nature" plays in scientific practice. On the one hand, theories affect what one measures and "sees," as do the institutions of science (training, funding, research programs, experimental conventions, instrumentation) and established rhetorical networks of persuasion. On the other hand, "nature" is sometimes wholly constructed, sometimes a resistance against which "reality" is constructed, but is rarely an effect unblemished by perceptions or expectations. Narrative devices may be deeply present in every field of science and all seeing perspectival and all knowledge situated, but the world is partly object, a co-participant in knowledge production, a constraint on perception.[442]

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442. LATOUR, *supra* note 81, at 6. Latour stated:

> Yes, the scientific facts are indeed constructed, but they cannot be reduced to the social dimension because this dimension is populated by objects mobilized to con-

These aphorisms are not the stable insights of a discipline, but problems or anomalies for any account of science. Where does nature end and society begin? Or is that the wrong question altogether – a divisive dualism that eclipses the second dimension (beyond the nature-society pole) that could account for quasi-objects, nonhuman actors, or entities as trajectories that occupy "many states, being impurely social, then purely social, then purely natural, then impurely natural?"[443] These are matters for intense and ongoing disciplinary debate, not the settled insights available for importation into law.

As insights, they are unsettling. While it would be an overstatement to say that courts and most legal commentators engaged those debates, it is likely that a danger sign was sent to the field of law by the unsettled character of the history, philosophy, and sociology of science as suggested by the amicus briefs in *Daubert* and as faintly reflected in *Daubert* itself. If "arguably, there are no certainties in science,"[444] wait until you see the history, philosophy, and sociology of science! If courts have no time for theoretical disputes in science because law "must resolve disputes . . . quickly,"[445] then the theory of theoretical disputes in science is extremely low on the interdisciplinarity priority list.

The problems identified in studies of science, however, will not go away. Science is an historical affair, affected and/or constituted by social relations, and its conclusions are "subject to perpetual revision."[446] Once that insight is acknowledged, a position on science is unavoidable. However, instead of adopting a traditional view of scientific objectivity, courts can simply bracket or ignore the question of scientific certainty or causal precision. Let the scientists testify and then construct certainty and causality for a particular legal, factual controversy. If the judgment turns out to be "incorrect" by later developments in science, correctness was never the issue in the first place.[447]

> struct it . . . . The ozone hole is too social and too narrated to be truly natural; the strategy of industrial firms and heads of state is too full of chemical reactions to be reduced to power and interest; the discourse of the ecosphere is too real and too social to boil down to meaning effects. Is it our fault if the networks are *simultaneously real, like nature, narrated, like discourse, and collective, like society*?

Id.

443. Latour, *supra* note 228, at 286.

444. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 573, 590 (1993).

445. *Id.* at 597.

446. *Id.*

447. But law's legitimacy is drawn into question when it produces questionable, random, or inconsistent outcomes based on the same body of scientific evidence. *See* David L. Faigman, *"Normative Constitutional Fact-Finding": Exploring the Empirical Component of Constitutional Interpretation*, 139 U. PA. L. REV. 541, 604 (1991) ("[P]ersistent misapplication of empirical data undermines the Court's legitimacy."); Veilleux, *supra* note 341, at 1994 ("Science is the American faith . . . . To the extent that the public believes the courts are following

This is pragmatic, legal constructivism. Justice Breyer's crusade for truth[448] and the crusaders against junk science[449] lost the battle for scientific authority in law, but they did not lose to social constructivists. They lost to legal pragmatists, including federal judges whom are grateful not to have to become philosophers of science.

VIII. Conclusion

> One does not come away from [Matthew Fontaine] Maury's work with the sense that Nature has been *represented*; instead, *Sailing Directions* [1854] provokes a feeling of being overwhelmed at the ongoing work required to produce the inscriptions that enable a range of humans and nonhumans to form alliances and consequently predict and control enough socio-technical-natural patterns to accomplish a specific task.[450]

As Justice Blackmun explained in *Daubert*, courts must act quickly to resolve legal disputes, and in cases involving scientific issues, courts cannot wait for scientists to resolve their own controversies. Similarly, when the Court began an abstract inquiry into the characteristics of good science to set standards for admissibility of expert scientific testimony, it needed to act quickly. We might wish that the Court would have surveyed the disputes in the history, philosophy, and sociology of science; acknowledged the diverse positions in the field; and then, after selecting its preference, carefully defended that position against existing criticism. In the rush to judgment, the

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a scientific procedure, they are likely to have more confidence that the procedure produced the right result (credibility) and to have more respect for the procedure itself (legitimacy).") In the Bendectin cases, for example, the outcomes of the jury trials were very inconsistent and some juries awarded sizeable damages against the manufacturer, though subsequent rigorous research consistently failed to establish a causal link between the drug and birth defects. *See* Joseph Sanders, *From Science to Evidence: The Testimony on Causation in the Bendectin Cases*, 46 STAN. L. REV. 1, 4-27 (1993) (presenting discussion of Bendectin trials and lack of evidence of causation). According to Professor Schuck, this helped to further discredit the tort system, forced Bendectin off the market, and cost the company substantial amounts of money. Peter H. Schuck, *Multi-Culturalism Redux: Science, Law, and Politics*, 11 YALE L. & POL'Y REV. 1, 22-23 (1993). "The law's repudiation of bad science in the *Bendectin* litigation thus came tragically late." *Id.* at 23.

448. *See* General Elec. Co. v. Joiner, 522 U.S. 136, 150 (1997) (Breyer, J., concurring) (presenting "ascertainment of truth" as basic objective of Federal Rules of Evidence). Significantly, however, Justice Breyer's majority opinion in *Kumho Tire*, 526 U.S. 137 (1999), is strikingly pragmatic in how "flexible" the *Daubert* factors became.

449. *See* Koukoutchos, *supra* note 17, at 2250-51.

450. T. Hugh Crawford, *Networking the (Non) Human:* Moby-Dick, *Matthew Fontaine Maury, and Bruno Latour*, 5 CONFIGURATIONS 1, 17 (1997).

Court told a story, that is, constructed a narrative about how science operates. Only by reading the briefs does one get the idea that the Court was adopting the "new" view of science that replaced an "old" view, and that the two-view argument was also a narrative constructed to make it easy on the justices. A more accurate representation of the field of science studies would have identified at least three alternatives to traditional views of science as autonomous, namely: (1) "weak" social theories that identify the influences on science of theoretical paradigms, individual interests, and the norms of scientific communities; (2) "stronger" social theories that characterize science as a cultural activity that produces facts in reliance not on nature, but on rhetorical conventions and a "theatre" of experiments; and (3) co-production theories that attempt to avoid ultimate references to either nature or society, since both are in play in any scientific enterprise. Variations persist within these broad frameworks, of course, and the debates are as many-sided and inconclusive as any other philosophical controversy. The United States Supreme Court, therefore, could not enter these debates and, on balance, justifiably pick a winner.

The paradox, of course, is that the Court had to pick a winner, or at least try to pick a winner. The definition of science announced in *Daubert* was ambiguous. Thus, federal courts enjoy a certain philosophical leeway alongside and paralleling their leeway to make reliability determinations. Interestingly, the behavior of federal courts in imposing criteria for science, by virtue of legal authority, is not so different from Bruno Latour's view of the behavior of laboratory scientists. According to Latour, scientists do not need universal laws or theoretical certitude to succeed; they only need to control enough facts to produce convincing, local knowledge.[451] Courts have become Latourian laboratories, controlling enough facts to draw a conclusion that is enforceable against the parties, if not against the tribunals of reason – Latour's term for the general population who act as judges of scientific progress.[452]

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451. *See id.* at 16-17.

> According to a constructivist view of knowledge, learning may better be characterized by parallel constructions relating to specific contexts. This means that parallel and maybe even competing, ways of constructing reality develop and provide the basis for new forms of knowledge. . . . Research-based knowledge, which is often abstract and general in nature, must be transformed to a concrete example that can be used and used in a specific and practical setting.

Glenn Hultman & Cristina Robertson Horberg, *Knowledge Competition and Personal Ambition*, 19 SCI. COMM. 328, 341, 343 (1998) (internal quotations and citations omitted).

452. *See* LATOUR, *supra* note 81, at 18 (discussing Robert Boyle's scientific work). Latour stated:

> Ironically, the key question of the constructivists – are facts thoroughly constructed in the laboratory? . . . – is precisely the question that Boyle raised and resolved.

Some federal courts have adopted a view of science not visible by the current interdisciplinary contributions in the field of law and science. It is a unique position driven by pragmatic concerns, much the same way that those legal scholars, who try to influence the judiciary in their amicus briefs or in legal periodicals, are driven by pragmatic uses of the pedagogical divisions between two views in the field of the history, philosophy, and sociology of science. Social constructivism, it is true, has not yet had its day in court or in the field of legal commentary. But we can identify a pragmatic form of legal constructivism based, in part, on the courts' inability to find an uncontroversial representation of the scientific enterprise. Given the choice of (1) adopting an impoverished philosophy of science from *Daubert*, (2) ignoring the philosophy of science and unwittingly taking a position, or (3) setting aside the problem by isolating a world of *legal* science, the latter seems best. At least we are not pretending that the philosophy of science is conclusive and settled. Rather, we see its complexity and back away, recognizing law's purposes and limits.

> Yes, the facts are indeed constructed in the new installation of the laboratory and though the artificial intermediary of the air pump . . . . But are the facts that have been constructed by man artifactual for that reason? No . . . we know the nature of the facts because we have developed them in circumstances that are under our complete control . . . . Boyle turns a flaw – we produce only matters of fact that are created in laboratories and have only local value – into a decisive advantage: these facts will never be modified, whatever may happen elsewhere in theory, metaphysics, religion, politics or logic.

Id.