Ethnography and the Idealized Accounts of Science in Law

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> [W]hen sociological observers began to enter the places where scientific knowledge is produced, the laboratory, they found many practices that seemed to share more with daily life outside the lab than with the strict edicts governing knowledge in science, such as universality, objectivity, or reproducibility. Measurement . . . might be based on a very unclear . . . consensus. Techniques might be developed in local settings and depend on local materials and practices. . . . The establishment of findings in the laboratory as facts accepted by the wider scientific community might turn out to be in large part a social process . . . of gaining credibility . . . .[1]

An idealized description of scientific activity persists in law and in legal literature. From the *Daubert*[2] four-part test, formulated in 1993—

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1. EMILY MARTIN, FLEXIBLE BODIES: TRACKING IMMUNITY IN AMERICAN CULTURE—FROM THE DAYS OF POLIO TO THE AGE OF AIDS 6 (1994).
2. Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579 (1993).

science is (i) testable, (ii) with a low error rate, (iii) usually published, and (iv) generally accepted[3]—to the new Federal Rule of Evidence 702—scientific testimony is based on sufficient data and is the product of reliable methods[4]—the social embeddedness of science is virtually ignored in favor of a "core" or "bottom-line" summary description. Even when decidedly social aspects of the scientific enterprise—for example, funding bias, fraud, and governing research paradigms—are acknowledged, they are seemingly viewed as fleeting and irrelevant to the project of defining "science."[5] There is little attention paid to the inevitable, *not* anomalous, institutional and rhetorical features of modern science. The purpose of this Article is to explore that deficiency, explain how some science studies scholars investigate social embeddedness, and suggest how judges and attorneys might view science differently in light of such studies.

The distinction between inevitable and anomalous social features of science is not always clear. For example, language itself is necessary to science, but the particular, dominant style of "agentless prose"[6] among scientists is arguably subject to critique and transformation. While rhetoric and persuasion are inevitable features in the production of scientific knowledge, a particular scientific community's research standard for required precision may, following criticism, change.[7] Complicating the distinction, some features of science, like a particular community's research standard in my last example, might be viewed by scientists as subject to change but not anomalous. That is, the precision requirement is viewed as good for science. Thus the institutionalization and professionalization of modern science, to the extent that its *particular*

3. *Id.* at 593–94.

4. FED. R. EVID. 702.

5. *See, e.g.*, Erica Beecher-Monas, *The Heuristics of Intellectual Due Process: A Primer for Triers of Science,* 75 N.Y.U. L. REV. 1563 (2000). Beecher-Monas begins her "primer" with a strong acknowledgment of science as culture-bound, *id.* at 1576, but then the elements of her "heuristic" are reduced to hypothesis, data, inferences, methodology, and probable conclusion, *id.* at 1589–90.

6. *See* David Locke, *Voices of Science*, 67 AM. SCHOLAR 103, 104 (1998) ("That official language of science—policed by the referees and editorial boards of scientific publications—is what English teachers call agentless prose; that is, there is no designated agent for any of the actions it describes."); *see also* TERRY THREADGOLD, FEMINIST POETICS: *POIESIS*, PERFORMANCE, HISTORIES 16–34 (1997). Threadgold traces in the history of science

> the gradual disappearance of the embodied masculine subject of science from the scene of his scientific activities . . . .
>
> The generic organisation of the scientific article and the business of citation now functions, disembodied and desexed . . . [to hide] a profoundly embodied and disciplined process, subject to all kinds of policy, institutional, private and power relationships . . . .

Id. at 21–22.

7. *See infra* notes 30, 38.

"Authority Structure" and "Reward System" is not inevitable, is arguably necessary for science to flourish.[8] Nevertheless, a distinction should be made between inevitable social features—cognitive limitations, communal standards and conventions, shared language, and institutionalization—and those that are not integral to science—fraud,[9] the public's ideology,[10] the effect of business interests,[11] political influence,[12] ethical limitations,[13] or a combination of the foregoing.[14] The latter anomalous or eliminable features of science, or "junk" science, are clearly acknowledged and are part of legal discourse. In my emphasis on the inevitable features of science, I am neither claiming generally that they are good for science, because they make good science possible, nor that they challenge the status of science because scientific knowledge is *merely* a narrative or a social construction. Rather, I think an

8. *See* David Goodstein & James Woodward, *Inside Science,* 68 AM. SCHOLAR 83 (1999). "[T]he basic outlines of the social organization of science emerged almost as soon as science did .... It is difficult to avoid the conclusion that science cannot exist—certainly cannot flourish—without the Reward System and the Authority Structure." *Id.* at 90. The "Reward System" is collectively "[t]he various means by which scientists express admiration and esteem for their colleagues," which system is guided and controlled by the Authority Structure; "[t]he goals of those in the Authority Structure are power and influence." *Id.* at 84.

9. *See* Daniel S. Greenberg, *Turning Science Into Gold,* WASH. POST, Nov. 30, 1999, at A29 (discussing a "review of a series of drug tests [that] concluded that favorable results were puffed up through repetition in various publications, while negative information was played down or ignored").

10. *See* John McCarron, *Science Takes a Back Seat to Ideology,* CHI. TRIB., Apr. 24, 2000, § 1, at 13 (criticizing "democratized science;" public hearing on vaccination endorsed by American Academy of Pediatrics allows suspicious citizens to overturn scientific experts).

11. *See* Eric Pianin, *Dioxin Report by EPA on Hold: Industries Oppose Finding of Cancer Link, Urge Delay,* WASH. POST, Apr. 12, 2001, at A1 (chemical, beef, and poultry industries challenge EPA study as "not based on science"); *see also* Michael Stern, *Raiders of the Lost Genome,* AM. LAWYER, Feb. 2001, at 77 ("[T]he market economy... always makes the question of who owns what more important than who knows what.") (reviewing KEVIN DAVIES, CRACKING THE GENOME (2000)).

12. *See* Pianin, *supra* note 11, at A1 ("The Bush Administration has challenged several Clinton-era environmental and public health rules and initiatives—including a tough new standard for arsenic in drinking water—on the grounds they weren't scientifically sound and would cause economic hardship to industry and local governments.").

13. *See* Rick Weiss, *Bush Administration Order Halts Stem Cell Meeting,* WASH. POST, Apr. 21, 2001, at A2 (discussing ethical and theological concerns over stem cell research).

14. *See* Barron Lerner, *Body Politics: How Public Pressure, Private Interests and Powerful Lobbies Infect the Treatment of Breast Cancer,* WASH. POST, HEALTH MAG., May 22, 2001, at 14.

acknowledgment of and discourse concerning the inevitable social features of science would lead to a more accurate conception of how science works *and*, in particular cases, could lead to novel lines of critical inquiry on the part of lawyers, judges, and juries concerning some scientific controversies, some scientific theories, and some scientists. In short, the inevitable social aspects of science are neither good nor bad in general, but understanding their role in the production of scientific knowledge provides another potential basis to challenge or defend expert scientific testimony in certain cases. Just as a large payment to an expert to testify is not necessarily indicative of bias, but *might* signal either bias or credibility (for example, success, reputation, and authority) in particular cases, evidence concerning cognitive limitations, standards as to what is worth investigating, governing metaphors, dominant theoretical models, and institutional gatekeeping within science might signal innocent bias or, conversely, frameworks establishing credibility, in particular cases.

In Part I, I confirm the idealizations of science in law and their implications for legal scholarship and practice. In Part II, I describe the ethnographic method used by science studies scholars, with reference to my own ethnographic analysis of interviews with three neuroscientists. I conclude Part II by identifying various social aspects of science that comprise a complex picture of scientific activity. In Part III, I discuss the implications of ethnomethodology for trial practice, including deposition analysis, *Daubert*-type hearings, cross-examination techniques, and drafting jury instructions. Part IV addresses anticipated criticisms of my arguments.

I. IDEALIZING SCIENCE

> "Scientists such as Wolpert happily acknowledge that science is a social activity. Every practicing scientist is acutely aware of it. How could one not be? All he wishes to deny is that 'science is merely a social construct with little special validity.'"[15]

Everybody, it seems, is willing to acknowledge the social character of scientific activity.[16] The real issue is whether the social features of

15. David L. Hull, *The Professionalization of Science Studies: Cutting Some Slack*, 15 BIOLOGY & PHIL. 61, 85 (2000) (citation omitted) (quoting Lewis Wolpert, *Response to Fuller*, 24 SOC. STUD. SCI. 745, 745 (1994)).

16. *See* Philip Kitcher, *A Plea for Science Studies, in* A HOUSE BUILT ON SAND: EXPOSING POSTMODERNIST MYTHS ABOUT SCIENCE 36 (Noretta Koertge ed., 1998).

> Although some idealized treatments of science proceed as if inquiry were carried out by subjects who were disembodied, logically omniscient, and alone, everybody knows better.... Those who want to slight the... thesis [that science is done by cognitively limited beings in social groups with

science make a significant difference in terms of scientific reliability and success.[17] While there may be a perceived risk in the fields of the history, philosophy, and sociology of science (i) that attention to the socio-historical aspects of science might eclipse our notions of scientific progress, predictive success, canons of reason and evidence, and the existence of theory-independent entities,[18] or (ii) that emphasizing power relations, boundary work,[19] institutional arrangements, and governing discourse might render scientists' own internal accounts (of their work as pragmatic, approximate, evidence-relational, and model-based) superfluous,[20] I do not believe there is any such danger in law. Given the reliance on science by legal institutions, the risk is instead that realist or rationalist and *internal* scientific perspectives will dominate legal discourse.

Examples of idealizations of science include the *Daubert* four-part test,[21] the 2000 amendment commentary to Federal Rule of Evidence 702,[22] and much of the recent scholarship on law and science to the

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> complicated structures and long histories] surely do not contest these points but, rather, deny that they have any impact on the practice of science.... [However,] individual and group histories and/or social roles make a difference to scientific work.

Id.

17. *Id.*

18. *See id.* at 34–38. Critics of science studies "are broadly right to recognize a persistent danger of overemphasizing the [socio-historical features of science] and ignoring [its realist-rationalist features]." *Id.* at 38.

19. *See* Michael M. J. Fischer, *Eye(I)ing the Sciences and Their Signifiers (Language, Tropes, Autobiographers): InterViewing for a Cultural Studies of Science and Technology*, *in* TECHNOSCIENTIFIC IMAGINARIES: CONVERSATIONS, PROFILES, AND MEMOIRS 43, 60 (George E. Marcus ed., 1995) [hereinafter TECHNOSCIENTIFIC IMAGINARIES] (describing "the ways in which scientific knowledge polices its own boundaries against new ideas or new information that it cannot easily incorporate"); *see also* Charles Kester, *The Language of Law, the Sociology of Science and the Troubles of Translation: Defining the Proper Role for Scientific Evidence of Causation*, 74 NEB. L. REV. 529, 548–50 (1995). Scientists "use boundary work to self-define their community and maintain consensus among the members of that community." *Id.* at 548.

20. *See* Fischer, *supra* note 19, at 59–64 (noting temptation to ignore accounts of science by scientists themselves in favor of a social constructivist, political mediationist, or narrative-theoretical account; each account is valid to a point but "dangerous if allowed to silence the other perspectives") (citing DONNA HARAWAY, PRIMATE VISIONS: GENDER, RACE, AND NATURE IN THE WORLD OF MODERN SCIENCE (1989)).

21. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593–94 (1993) (stating scientific knowledge is testable, with a low error rate, and is usually peer-reviewed and generally accepted).

22. *See* AMENDMENTS TO FED. R. EVID., H.R. DOC. NO. 106-225, at 41 (2000) (stating that scientific knowledge is admissible by expert testimony "if (1) the testimony

extent that authors emphasize core aspects of science as the most significant for admissibility assessments.[23] Core features of science include (i) the scientific hypothesis or theory (or "testable theory"), (ii) scientific data, (iii) reliable testing or methodology, including standards and controls to ensure a low error rate, and (iv) a conclusion, or probable conclusion, with explanatory power. Each of these features of science has an obvious anchor in nature or reality—(i) theories are formulated with reference to perceived reality, (ii) the data is a representation of natural phenomena, (iii) the methodology is how the data is handled, and (iv) the probable conclusion should lead to explanatory power and success. However, each of the core features of science also has an anchor in social structures—(i) theories reflect personal and communal beliefs and values as to what is important or worth studying,[24] which beliefs and values have a history[25] and refer back to earlier research, institutional training, and professionalization of scientists;[26] (ii) observation of data is mediated by cognitive capacity and theoretical presuppositions (hence the term "theory-laden observation," which suggests the researcher is looking for some things but not for others);[27] (iii) methodology also has a social history of experimental conventions, and may vary among fields of research;[28] measurement technology and inscription devices also have a social history related to available resources and theoretical paradigms;[29] and (iv) even conclusions are made with reference to acceptability standards, arising from the history of research and practical demands.[30] Such social aspects are not, or

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is based upon sufficient facts or data, (2) the testimony is the product of reliable principles and methods, and (3) the witness has applied the principles and methods reliably to the facts of the case").

23. *See, e.g.*, Beecher-Monas, *supra* note 5, at 1576, 1589–90; *see also* David S. Caudill & Richard E. Redding, *Junk* Philosophy *of Science?: The Paradox of Expertise and Interdisciplinarity in Federal Courts*, 57 WASH. & LEE L. REV. 685, 741–43 (2000) (discussing a variety of approaches).

24. *See* David Goodstein, *How Science Works, in* REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 67, 70 (2d ed. 2000). "At the most fundamental level, it is impossible to observe nature without having some reason to choose what is worth observing and what is not worth observing." *Id.*

25. *See* Kitcher, *supra* note 16, at 36.

26. *See* Goodstein, *supra* note 24, at 75–77.

27. *See id.* at 70 ("Popper believed all science begins with a prejudice, or perhaps more politely, a theory or hypothesis.").

28. *See id.* ("We don't really know what the scientific method is."); *see also* Gary Edmond, *Judicial Representations of Scientific Evidence*, 63 MOD. L. REV. 216, 220 (2000) ("There is no universal scientific method determining every aspect of scientific practice.").

29. *See* MARTIN, *supra* note 1, at 6.

30. *See* Kitcher, *supra* note 16, at 37 ("[T]he practical demands and the history of research standards also help determine what will count as acceptable solutions, specifying, for example, the precision that an answer must achieve if it is to be applicable.").

should not be, particularly controversial, but they are often not identified and discussed as significant in idealized accounts of science. When the U.S. Supreme Court, in *Daubert*, defined science as involving testable theories, a low error rate, peer review and publication, and general acceptance,[31] those elements became topics of discussion in legal scholarship, bar journals, and continuing legal education. Lawyers and judges, quite naturally, focused their attention on these aspects of science in matters of admissibility of expert testimony.

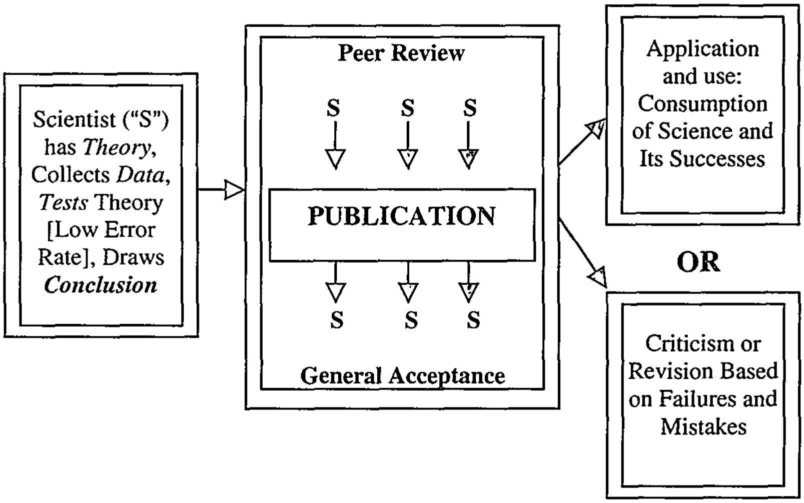
Significantly, the last two features of the *Daubert* four-part test—peer review and publication, and general acceptance[32]—identify social aspects of scientific activity. They are not, however, on the same *level* as testability and low error rate, which are considered to be the markers of valid science.[33] Most of the time, Justice Blackmun opined, valid theories will be the product of the peer review and publication process, and will attain general acceptance—those that are not published or generally accepted should be viewed with suspicion.[34] Social aspects of science are thereby downplayed, and the opportunity is missed for a critical discourse concerning the institutional and rhetorical characteristics of peer review and publication, and general acceptance.

The idealized or core account of science is illustrated in Diagram I. Again, this picture of science, in law, leads judges and lawyers, and therefore juries, to focus on adequacy of data or testing, presence of publications, and level of general acceptance in (i) judicial assessments of reliability, (ii) determining appropriate subjects for deposition and cross-examination questions, and (iii) drafting jury instructions.

Once acknowledged, the social aspects of scientific activity may nevertheless be characterized, in idealized accounts of science, as relatively insignificant or at least as unworthy of serious attention. First, one may distinguish between internal and external factors in scientific activity. Further, one may identify the internal factors as good, that is, productive and positive, and the external factors as bad, or at least as superfluous to genuine science.[35] Indeed, peer review and publication, and

31. *See* Daubert v. Merrell Dow Pharms., Inc., 509 U.S. 579, 593–94 (1993).
32. *Id.*
33. Testability and low error rate are identified, without qualification, as features of science, while peer review and publications, and general acceptance, are factors that may not always be present. *Id.*
34. *Id.*
35. *See* SANDRA HARDING, IS SCIENCE MULTICULTURAL?: POSTCOLONIALISMS, FEMINISMS, AND EPISTEMOLOGIES 2–3 (1998).

DIAGRAM I



general acceptance may be seen as internal to good science; other internal factors might include institutional gatekeeping (including training and professionalization), methodological preferences, experimental conventions, instruments and measurement technologies, models to represent nature, theoretical paradigms, scientific language, negotiation techniques and strategies for conflict resolution and consensus-building, cognitive capacity and perception, and even values like consistency, honesty, rigor, self-criticism, and reproducibility. Each such factor is a social, not natural, structure, but each may be conceived as conducive to natural scientific inquiry.

> A central assumption of [the conventional] theory of scientific knowledge is that the success of modern science is insured by its internal features—experimental method or scientific method more generally, science's standards for maximizing objectivity and rationality, the use of mathematics to express nature's laws . . . .
>
> . . . .
>
> Therefore, when sciences function at their very best, their institutions, cultures, and practices . . . should be understood to provide the necessary conditions for sciences to do their work, but they should not influence the results of research in any culturally distinctive way. Any and all social values and interests that might initially get into the results of scientific research should be firmly weeded out as soon as possible through subsequent critical vigilance.

Id.

External factors might include political interests and pressures, including ethical and policy limitations, economic interests, funding bias, fraud, bad or misleading instruments and models, greed, ambition, rhetoric and persuasion, gender or racial bias, and general cultural values. The problem with internal and external distinctions is that external factors are not always bad or superfluous, and internal factors are not always good or productive. Greed, ambition, persuasion, or economic interests might produce the best science, while institutional gatekeeping, theoretical paradigms, and models may at times lead scientists astray.

Even if the internal and external distinction breaks down, social factors may also be ignored through a distinction between context of discovery and context of justification.[36] The messier aspects of science, for example greed or ambition, may be categorized as belonging to the context of discovery, where new ideas might come from anywhere,[37] even from sloppy techniques and cultural bias. The validity of science is grounded in the context of justification, which is the idealized picture of science: testability, methodology, reproducibility and probable conclusions. This distinction, as well, is problematic, since social aspects pervade the context of justification.[38]

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36. *See* Fischer, *supra* note 19, at 63–64. "Scientists distinguish between discovery (which may be serendipitous) and confirmation/falsification, between the sociology of science and the content of science .... [Such] accounts ... ignore or downplay the sociological and political environments that enable them." *Id.*

37. *See* Goodstein, *supra* note 24, at 70 ("Nobody can say where the theory comes from."); *see also* Ian Hacking, *How Inevitable Are the Results of Successful Science?*, 67 PHIL. SCI. S58, S69 (2000).

> [I]t is patently obvious that which questions get asked, taken seriously, investigated, funded, reported, analyzed, and so forth is the result of social processes, human interactions, and current interests. Very few detailed questions are asked about the most widespread tropical diseases because there is no money in it for drug companies ....

HACKING, *supra*, at 569.

38. *See* WERNER CALLEBAUT, TAKING THE NATURALISTIC TURN, OR HOW REAL PHILOSOPHY OF SCIENCE IS DONE 211–13 (1993) (transcribing conversations with William Bechtel, Thomas Nickles, and other philosophers of science).

> *Bechtel*: Examining the work of scientists ... I came to realize what proportion of their time was devoted to social activities and how important those were in terms of determining the intellectual content of their work. Such things as which scientist would respond to which other and what experiment someone would do were affected....
>
> ....
>
> Even [conforming to a prescribed style in a scientific text] is an important social constraint: you realize that scientists are writing in a particular way

Finally, another "defense mechanism" to critical discussion of social factors is the explanation of errors by reference to social influences, and the explanation of success by reference to nature.[39] For example, funding bias and political pressure may be viewed as mistakes to eliminate, leaving the impression that social influences can be avoided. All of these characterizations of the social aspects of science—external influences, internal supports, context of discovery, and avoidable errors—function to downplay the inevitable institutional and rhetorical character of scientific theory and practice.

Because the narrative above identifies at least twenty social aspects of science in addition to the peer-reviewed publication process, another diagram may be helpful to show how our picture of science can be expanded beyond the previous *core* or idealized picture. Diagram II is also a representation of how most of these social factors can be explained away or rendered superfluous. The "external influences" depicted above the idealized account of science include the "context of discovery" (where greed or ambition, or anything—for example,

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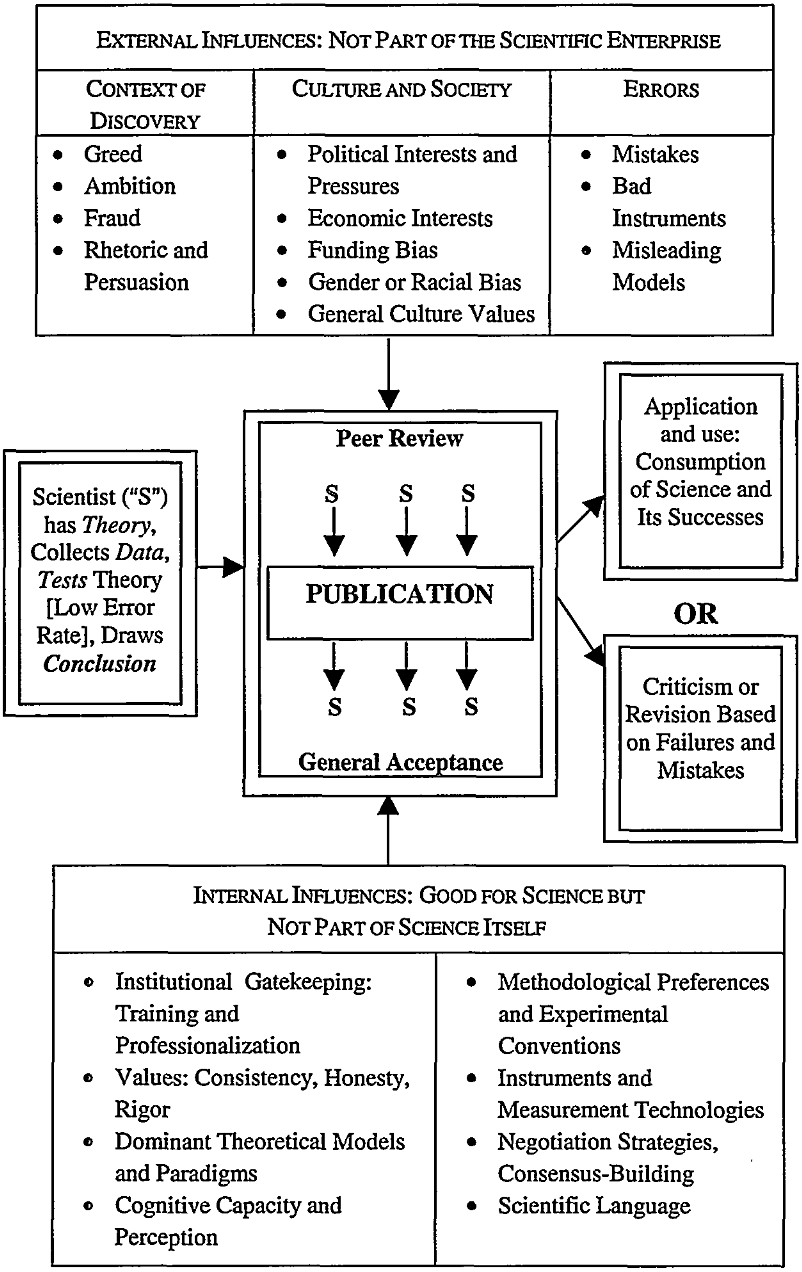
> because that's the only style that's acceptable to get their ideas across. They're fitting into a channel . . . . Also, I came to realize that there is a dynamic that involves who else is in the community and that this influences how one scientist uses words to establish something.
>
> . . . .
>
> *Callebaut*: Back to justification. You [Nickles] make a daring claim . . . that in a sense all justification—and hence all rationality—is at bottom social.
>
> *Nickles*: It sounds daring, but in a way it's trivial. At bottom what else is there? Justification comes down to addressing human critics . . . . [I]t is ultimately a matter of *what the critical community lets you get away with.*
>
> . . . .
>
> The important philosophical implication is that justification as it really operates in ongoing inquiry is quite *local.* The arguments and moves that make a difference, that *cause* investigators to behave one way rather than another, are typically quite local.

Id.; *see also* STEVEN SHAPIN, THE SCIENTIFIC REVOLUTION 10 (1996) ("There is as much 'society' inside the scientist's laboratory, and internal to the development of scientific knowledge, as there is 'outside.'").

39. *See* JONATHAN POTTER, REPRESENTING REALITY: DISCOURSE, RHETORIC AND SOCIAL CONSTRUCTION 19 (1996).

> False belief could be directly explained through a "social fact" (personality, prejudice and so on) disrupting the proper operation of scientific norms. True belief . . . arises directly from a careful investigation of how the world is. Put simply, in this view of science, the facts themselves determine truth, while error is explained by processes of a psychological or sociological nature. The consequence of this is that with true belief there was nothing to explain save for how the conditions for proper scientific inquiry came about and how those conditions are undermined.
>
> In effect . . . the [sociology of error] tradition . . . bracketed off the study of facts themselves and contented itself with examining their sociological context. A full sociological analysis of the *content* of science—of scientific ideas, theories, methods and so on—was reserved only for falsehoods.

DIAGRAM II


EXTERNAL INFLUENCES: NOT PART OF THE SCIENTIFIC ENTERPRISE
CONTEXT OF DISCOVERY
Greed
Ambition
Fraud
Rhetoric and Persuasion
CULTURE AND SOCIETY
Political Interests and Pressures
Economic Interests
Funding Bias
Gender or Racial Bias
General Culture Values
ERRORS
Mistakes
Bad Instruments
Misleading Models
Scientist ("S") has *Theory*, Collects *Data*, *Tests* Theory [Low Error Rate], Draws *Conclusion*
Peer Review
S S S
PUBLICATION
S S S
General Acceptance
Application and use: Consumption of Science and Its Successes
OR
Criticism or Revision Based on Failures and Mistakes
INTERNAL INFLUENCES: GOOD FOR SCIENCE BUT NOT PART OF SCIENCE ITSELF
Institutional Gatekeeping: Training and Professionalization
Values: Consistency, Honesty, Rigor
Dominant Theoretical Models and Paradigms
Cognitive Capacity and Perception
Methodological Preferences and Experimental Conventions
Instruments and Measurement Technologies
Negotiation Strategies, Consensus-Building
Scientific Language

religion—might accidentally lead to good, justifiable science). Other examples of external influences include cultural or social influences, as well as erroneous or junk science. All of these influences may be viewed as eliminable, once identified, by setting a boundary between them and the *core* activities. Indeed, the *core*, expressed earlier as the idealized picture of science, becomes the "context of justification," the arena wherein fraud can be caught, mistakes can be corrected, and biased or interested science can be falsified. The "internal supports"—the social aspects of science depicted below the *core* activities on the diagram—are harder to get rid of, but may be dismissed as obvious (that is, language and perception are *givens*) and in any event as necessary but benign and unproblematic. If something does go wrong internally, such as an insufficient model, a biased laboratory or an inaccurate measurement device, then it becomes an external influence to be moved to the top of the diagram and then eliminated by careful *core* activities.

In the remainder of this Article, I want to challenge the "dismissive" arrangement of influences in Diagram II by suggesting that many of these identifiable social aspects of science should remain in play in our legal discourse concerning scientific expertise. This is not to say that every social aspect identified is present or significant in every scientific activity, but rather that many social factors are characteristic of science generally, and therefore are as much a part of science as data or an experimental test. After discussing how sociologists of science identify and investigate the significance of the social aspects of science, I suggest how lawyers and judges might do the same.

II. ETHNOGRAPHY AND SCIENCE

> [T]he fact that science is political and deeply embedded in [cultural] events is not simply the now-clichéd, albeit important conclusion of social scientists and historians studying scientists, but is part of the condition of doing science . . . . Some scientists . . . acknowledge their social embeddedness not at all or only in the most indirect and subtle ways; for others it is diversely and strongly expressed.[40]

Ethnomethodology[41] has become an established, if varied, mode of

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40. George E. Marcus, *Introduction, in* TECHNOSCIENTIFIC IMAGINARIES, *supra* note 19, at 1, 7.

41. *See* MICHAEL MOERMAN, TALKING CULTURE: ETHNOGRAPHY AND CONVERSATION ANALYSIS ix (1988) ("The term 'ethnometholodogy' . . . is the proper name for viewing 'the objective reality of social facts as an ongoing accomplishment of the concerted activities of daily life,'" and for "discovering the formal properties of common-place . . . actions 'from within' actual settings, as ongoing accomplishments of those settings." (quoting HAROLD GARFINKEL, STUDIES IN ETHNOMETHODOLOGY vii–viii (1967))). Moerman distinguishes "conversation analysis"—the study of "the

analysis in science studies. In anthropology, "ethnography usually requires learning the language, developing key informants, and spending at least one to two years of more-or-less continuous participant-observation in a community, organization, or social movement."[42] The method has been borrowed in other disciplines such as ethnomusicology (wherein the local music of an ethnic community is studied), and in comparative law, for example by Leopold Pospisil in *The Ethnology of Law*,[43] which focused on the legal system of the Kapauku Papuans of West New Guinea. In science studies, however,

> "ethnography" has historically applied loosely to any kind of fieldwork-based method, including short-term observational studies. Thus, in science studies circles the term has a considerably looser usage than in anthropology. . . . For this reason, the term "laboratory studies" is preferable for the first wave of ethnographic studies [which] addressed questions about theoretical issues in the sociology and philosophy of knowledge.[44]

Following scientists through society, or looking over their shoulders, is a means to study the fact-making process, to view the "interactions among scientific, governmental, industrial, religious, and other domains of society," and to identify rituals, values, and material culture such as buildings, machines, and equipment that characterize the scientific enterprise.[45] The anthropological model is justified by viewing science as a subculture with its own language(s), conventions, institutional hierarchies, values, and structures for training and professionalization of its members.[46]

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organization of everyday talk"—from ethnography —"understanding . . . how people make sense of their lives," but says that conversation analysis is within the intellectual tradition of ethnomethodology and must be coupled with ethnography. *Id.* at x.

42. DAVID J. HESS, SCIENCE STUDIES: AN ADVANCED INTRODUCTION 134 (1997).
43. LEOPOLD J. POSPISIL, THE ETHNOLOGY OF LAW (2d ed. 1978).
44. HESS, *supra* note 42, at 134.
45. *See id.* at 135.
46. *See* BRUNO LATOUR, WE HAVE NEVER BEEN MODERN 101–02 (1993).

> [The ethnologist] sets out to analyze one tribe . . . for example, scientific researchers or engineers . . . . Her tribe of scientists claims that in the end they are completely separating their knowledge of the world from the necessities of politics and morality. In the observer's eyes, however, this separation is never very visible . . . . Her informers claim that they have access to Nature, but the ethnographer sees perfectly well that they have access only to a vision, a representation of Nature that she herself cannot distinguish neatly from politics and social interests.

Id. (citations omitted).

Reasons for engaging in ethnographic research, which usually involves interviewing scientists as "informants" and analyzing the transcripts, vary in science studies.[47] Some want to discern the values at work in science, particularly to reveal unattractive values or to suggest social values to which scientists should be committed.[48] Others want to degrade scientific knowledge, to demonstrate that science is just another cultural activity with no more claim to truth than political theory or religion.[49] My own sense is that ethnographic research is useful in the effort to define science or describe how it really works.[50] Whatever the purpose, the ethnography of science is a growing subdiscipline of science studies that is no longer haphazard or undertheorized. Examples of ethnographic research are published,[51] and materials are available that identify interviewing skills and common blunders,[52] as well as the importance of indexicality ("the meaning of a word or utterance is dependent on its context of use"), reflexivity ("descriptions are not just *about* something but they are also *doing* something"), and the documentary method of interpretation (people understand "events and actions . . . in terms of background expectancies, models, and ideas") in conversation analysis.[53] Because of my concern that idealized accounts

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47. See generally Marcus, *supra* note 40, and the ethnographic conversations and commentaries collected in TECHNOSCIENTIFIC IMAGINARIES, *supra* note 19.

48. *See, e.g.*, Langdon Winner, *Social Constructivism: Opening the Black Box and Finding It Empty*, 16 SCI. AS CULTURE 427, 443–49 (1993) (arguing that social studies of science and technology should not be neutral reports but morally and politically evaluative).

49. Jonathan Potter, for example, describes the social constructivist "argument . . . that there is nothing epistemologically special about scientific work. Scientific knowledge production does not have principled differences from knowledge in legal or everyday settings." POTTER, *supra* note 39, at 35. Ethnomethodology or conversation analysis offers critics of science a tool for studying "the methods people use for producing and understanding factual descriptions." *Id.* at 42.

50. *See* Marcus, *supra* note 40, at 7.

> [B]eginning to ask how scientists have faith in their own activity, or in what ways their perceptions of what they are doing are changing, given some form of distinctive consciousness about the social and cultural construction of their activity, generates a completely transformed and vast field of inquiry on which a distinctly *cultural* studies of science might establish itself. The reflexivity brings a range of new factors *explicitly* into the production of science, and in this sense, makes it more directly cultural, or blended with concerns that were thought to be external to scientific activity.

51. *See* sources cited *supra* note 47.

52. *See, e.g.*, CHARLES L. BRIGGS, LEARNING HOW TO ASK: A SOCIOLINGUISTIC APPRAISAL OF THE ROLE OF THE INTERVIEW IN SOCIAL SCIENCE RESEARCH (1986).

53. *See* POTTER, *supra* note 39, at 43, 47, 49. A useful example of indexicality and problems of interpretation appears in Michael Moerman & Harvey Sacks, *On "Understanding" in the Analysis of Natural Conversation*, *in* MOERMAN, *supra* note 41, at app. B.

> Roger says: "Ken face it, you're a poor little rich kid." Ken then says: "Yes, Mommy. Thank you." Roger then says: "Face the music." We are sure that

of science in law tend toward "stereotypical images of scientists as cut off from society and culture, concerned only with the micro-worlds of labs and their professional networks,"[54] and because of the suggestion in science studies that "the insight concerning the social constructedness and embeddedness of scientific activity . . . is also shared to varying degrees by scientists and scientific institutions themselves,"[55] I decided to interview three neuroscientists concerning their laboratory research. My goal was to explore, in conversation, aspects of scientific activity that are not captured in idealizations of science. I chose neuroscientists as informants both because their field is dynamic and revolutionary within the disciplines of psychiatry and psychology[56] and because neuroscientists often collapse social existence into the natural, hence the term "eliminative materialism."[57] My informants included "X," a very successful middle-aged professor, who is a recipient of numerous grants, is engaged in commercial consulting, and is the author of numerous publications and

> you, like those present, [understood] . . . Ken's "Mommy" as a deliberate and consequential misidentification of Roger, and not as an error, or as a correct identification of someone not present—Ken's mother.

MOERMAN, *supra* note 41, at 185.

54. *See* Marcus, *supra* note 40, at 7.

55. *See id.*

56. *See* Joseph Dumit, *Twenty-First-Century PET: Looking for Mind and Morality Through the Eye of Technology, in* TECHNOSCIENTIFIC IMAGINARIES, *supra* note 19, at 87 (ethnographic analysis of scientists working in PET (positron emission tomography)).

> DUMIT: Nancy Andreasen, she has written about the biological revolution in psychiatry. You were in medical school during this time. Did you also get the other side of psychiatry?
>
> [JOSEPH] WU: Oh, very much so. I would say that most of the psychiatrists in this [U.C.-Irvine] department are still analytically, dynamically focused. I would say that biologically oriented psychiatrists still make up a minority of the faculty, maybe thirty to forty percent.

Id. at 114–16.

57. *See id.* at 112–13.

> DUMIT: This [Washington University] is one of the centers of biological psychology.
>
> [MICHEL] TER-POGOSSIAN: Yes, indeed, there are few followers of Freud . . . . But I don't know what the human mind is. Don't misunderstand me, I'm not being difficult about that. But it is probably related to the brain. If you remove the brain, there is not much mind left.
>
> DUMIT: Right. I haven't met anybody involved with PET who is not at least that, the eliminative materialist, as it's called.
>
> TER-POGOSSIAN: Is that what it is?
>
> DUMIT: Without the brain, you are nothing. At least that much is material.
>
> TER-POGOSSIAN: Beyond that, it really starts getting difficult.

Id.

conference papers; "Y," a retiring professor at the end of a successful career; and "Z," a relatively young but highly credentialed professor. Even though I asked similar questions of each informant concerning educational background, current research, whether science is ever neutral, and new ideas in psychology, the particular socio-cultural aspects of science discussed by each varied somewhat. I should emphasize that my informants did not believe that they were engaged professionally in an enterprise that is *primarily* rhetorical, philosophical, or driven by social structures. Obviously, they saw their work as grounded in natural phenomena. Each therefore reacted against, in different ways, any suggestion that science is *only* a cultural discourse or belief system. Informant X was not enamored with the idea that the objects of science are quasi-objects or socio-natural hybrids, and he was dismissive of philosophy of science in general.[58] Informants Y and Z viewed the social aspects of science as *influences*, which can be bad if research is restricted but good if they constitute useful structures in which scientists can work.[59]

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58. *See* Interview with X, Professor of Psychology, Neuroscience Program 7–8 (Mar. 7, 2001) (unpublished transcript on file with author).

> [CAUDILL]: [A] lot of ethnographic scholars in science studies are saying that what they're finding is a breakdown in any sort of Cartesian separation between subject and object. . . . Also, your comments about every time you're dealing with an object in the lab, the brain activity regarding smell, that's not different from a person deciding to sit in a different place in the bus [in which an odor was placed in one seat]; . . . was that a social activity, not sitting [in a particular seat] on the bus, or was that a natural object . . . in the brain? The answer is it's both and neither, it is a quasi-object, it's something between the two. . . .
>
> . . . .
>
> [X]: I just cannot believe that. . . . I find that I'm reminded, my old advisor who said that he really liked philosophy until he reached adolescence and then he went on to other pursuits. I can't believe that is a viable argument and it seems to me specious. . . . What other world is there besides the natural world?

Id.

59. *See* Interview with Y, Professor of Psychology, Neuroscience Program 15–16 (Mar. 20, 2001) (unpublished transcript on file with author).

> [CAUDILL]: [S]ome scholars say that science is never neutral, it's not an activity that takes place in isolation, but there's always particular institutional settings, that there's always value choices as to what's important, there's always commitments as to what you're looking for because your discipline has a history and it's pulling for certain things . . . .
>
> . . . .
>
> [Y]: You know it seems like to a certain extent you almost have to have that kind of structure . . . . But the problem is of course if you have too much structure you really scare people off or else you really kind of prevent them developing the potential that they have.

Id.; *see also* Interview with Z, Assistant Professor of Psychology, Neuroscience Program 12–14 (Mar. 30, 2001) (unpublished transcript on file with author).

> [CAUDILL]: [S]cience studies scholars suggest that science is never neutral, it

Nevertheless, in our conversations it was clear that the social aspects of scientific activity are integral to the enterprise. Because I was interested in identifying social aspects of science, I began with a catalogue—a list or menu derived from scholarly literature in science studies—of potential social "factors." In my interviews, I chose not to inquire about those social factors which most scientists consider anomalous or eliminable, including greed, ambition, fraud, obvious funding bias or corporate agendas, bad measuring instruments, and mistakes or identifiable human error. I also did not focus on cultural variations in scientific research, or public understanding of and influence upon science, each of which has been the subject of critical studies of science.[60] Finally, I wanted to distinguish between aspects of science that are, for all practical purposes, inevitable, and those that can be conceived of as problematic or controversial, such as the cultural construction of science in terms of class, race and ethnicity, or gender and sexuality.[61] Focusing on the former, I was left with the following catalogue:

> always involves particular institutional settings with their own language and standards of persuasion, it's always got value choices as to what's important . . . .
> [Z]: Well, I think it's a double-edged sword . . . . [Y]es, science is not neutral. . . . [T]here's nobody handing out lots of money to just let us go free in the laboratory. . . . I know that that was practically the environment . . . in the late 60's to the early 70's . . . . I think that was extremely good for science. And I think it was bad because it created this public perception . . . . [W]hy was the taxpayer paying for *that*? . . .
> . . . .
> On the other hand, I think that it really causes scientists to think long and hard about what it is they plan to do.

60. Regarding cultural variations, see SHARON TRAWEEK, BEAMTIMES AND LIFETIMES: THE WORLD OF HIGH ENERGY PHYSICISTS 126–64 (1988) (comparing Japanese and Western physicists, and exploring the roles of national and gender cultures in shaping scientific institutions and practices); *see also* HESS, *supra* note 42, at 134–35. "Sharon Traweek's ethnographic studies of physicists, based on over a decade of ethnographic fieldwork, are often regarded as a landmark for the beginning of the second wave of ethnography [(the first wave was produced by Europeans trained in sociology and philosophy)]." *Id.* at 135. Regarding public influence on science, see STEVEN EPSTEIN, IMPURE SCIENCE: AIDS, ACTIVISM, AND THE POLITICS OF KNOWLEDGE 26–41 (1996) (demonstrating the impact of AIDS activists on medical research and funding).

61. *See generally* DAVID J. HESS, SCIENCE AND TECHNOLOGY IN A MULTICULTURAL WORLD: THE CULTURAL POLITICS OF FACTS AND ARTIFACTS (1995); *see also* HARDING, *supra* note 35, at 2–3.

(1) Cognitive capacity and limitations, including perception[62]
(2) Standards as to what is interesting or what is worth doing,[63] including
  (a) the effects of funding and patronage
  (b) general cultural values (for example, honesty)
  (c) specific ethical conceptions of, for example, unacceptable research
  (d) government policy and political interests (which overlap with (a), (b), and (c) above)
(3) Language,[64] including

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62. *See* RONALD N. GIERE, SCIENCE WITHOUT LAWS 48–53 (1999) (discussing "the biological and psychological mechanisms underlying the cognitive capacities of individual scientists," *id.* at 49, and the need to look to cognitive science to explore those mechanisms); *see also* Nancy J. Nersessian, *Opening the Black Box: Cognitive Science and the History of Science*, 10 OSIRIS 194–211 (1995) (discussing cognitive history of science, cognitive science, and the investigation of creativity, conceptual innovations, technological innovations, communicative practices, and the role of training in science).

63. *See* Kitcher, *supra* note 16, at 36 ("The social structures in which science is embedded affect the kinds of questions that are taken to be most significant and, sometimes, the answers that are proposed and accepted.").

> [T]he kinds of problems singled out as important depend in part on the history of the field and on the wider interests of members of society. . . . [S]ome problems are especially significant . . . partly because of the history of research [in the field], partly because of what it is . . . possible to do, and partly because of the practical consequences of certain forms of inquiry when applied to the problems of certain kinds of societies.

Id. at 37; *see also* Pierre Bourdieu, *The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason, in* THE SCIENCE STUDIES READER 31 (Mario Biagioli ed., 1999).

> [I]t is pointless to distinguish between strictly scientific determinations and strictly social determinations of practices that are essentially *overdetermined*. . . . Fred Reif shows . . . how artificial and indeed impossible it is to distinguish between intrinsic and extrinsic interest . . . "A scientist strives to do research which he considers important. But *intrinsic satisfaction and interest are not his only reasons*. . . . The scientist wants his work *to be not only interesting to himself but also important to others*." What is regarded as important and interesting is what is likely to be recognized by others as important and interesting . . . .

Id. at 32–33 (quoting Fred Reif, *The Competitive World of the Pure Scientist*, 134 SCIENCE 1957–62 (1961)).

"The sciences depend upon their 'context' not only for funding and material resources; recruits and auxiliary personnel; institutional location in universities, academies, corporations, bureaucracies, or foundations; social norms, cultural forms, and bodily disciplines; but also for much of what is ultimately at issue or at stake in scientific practices." Joseph Rouse, *Understanding Scientific Practices: Cultural Studies of Science as a Philosophical Program, in* THE SCIENCE STUDIES READER, *supra*, at 442, 445.

64. *See* Timothy Lenoir, *Inscription Practices and Materialities of Communication, in* INSCRIBING SCIENCE: SCIENTIFIC TEXTS AND THE MATERIALITY OF COMMUNICATION 1, 1–19 (Timothy Lenoir ed., 1998).

> Considerations about language, whether Kuhn-inspired in quantitative linkages between scientific publications or concerns about Wittgensteinian language

(a) scientific and discursive regimes
(b) governing metaphors
(c) conventions of rhetoric and persuasion
(4) Evolution and constraints of measurement instruments and technology[65]
(5) Observation variables,[66] including
(a) dominant theoretical paradigms
(b) models and maps that function as representations of nature
(c) expectations, including their effect on interpretation of data
(6) Institutional gatekeeping, including
(a) training
(b) professionalization

> games and forms of life, have always been part of science studies in one form or another. . . .
>
> . . . .
>
> . . . [S]cholars from the side of literature studies have begun to focus on the role of rhetorical practice and techniques of persuasion in scientific texts, on narrative structures and metaphor in the internal structure of scientific work, and on the semiosis among scientific narratives and grand cultural narratives, represented in literature, museum exhibits, and popular culture, as means for the construction and stabilization of scientific artifacts.

Id. at 1, 3; *see also* JOSEPH ROUSE, ENGAGING SCIENCE: HOW TO UNDERSTAND ITS PRACTICES PHILOSOPHICALLY 158–65 (1996) (discussing the turn to narrative in science studies).

65. *See* DAVID HESS, SCIENCE AND TECHNOLOGY IN A MULTICULTURAL WORLD: THE CULTURAL POLITICS OF FACTS AND ARTIFACTS 3 (1995) ("Even apparently transparent observations, such as machine inscriptions of data, are social because machine design is the product of a history that involves social negotiation, as are decisions over calibration and how to interpret machine inscriptions.").

66. *Id.*

> What people expect to observe, are able to observe, and want to observe are all shaped in part by their theories and assumptions, which in turn are outcomes of discussions and controversies in which social negotiation is critical. . . . (However, . . . this claim does not mean that observations have nothing to do with reality: observations are simultaneously socially shaped and representative of a "real" material or social world.)

Id.; *see also* Donna J. Haraway, *Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective, in* THE SCIENCE STUDIES READER, *supra* note 63, at 172, 177.

> [A]ll eyes, including our own organic ones, are active perceptual systems, building in translations and specific *ways* of seeing . . . . There is no unmediated photograph or passive camera obscura in scientific accounts of bodies and machines; there are only highly specific visual possibilities, each with a wonderfully detailed, active, partial way of organizing worlds.

Haraway, *supra* at 177.

(c) symbolic capital[67] (social networks, Rolodex, positions held, previous accomplishments, etc., that lend credibility)
(d) consensus-building and negotiation techniques
(7) Methodological *variation* and experimental conventions[68]

The above catalogue, intended to capture ordinary social aspects of science as opposed to eliminable problems or negative influences, offers both a complex picture of science to supplement idealizations, which emphasize *core* aspects, *and* a categorization scheme for classifying statements made by scientists in my interviews. I did not classify references to core aspects of science, which were numerous, because I take it to be obvious that science involves theories, data collection, and conclusions that promise some level of objectivity, precision, and prediction. Again, my informants did not address each social category in our conversations; I began each interview with general questions about the informant's background and current research interests, and when I later asked about social aspects of their work I was intentionally ambiguous. In the following summaries of each interview, therefore, variable responses were to be expected.[69]

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67. *See* HESS, *supra* note 42, at 118.

> Some researchers have found Bourdieu's [concept] of symbolic capital . . . to be particularly useful. One might think of symbolic capital as status viewed through a political economy lens. Symbolic capital can be saved and spent, hoarded and wasted, accumulated and invested, and transformed into financial capital. In terms of science, symbolic capital might be operationalized as a scientist's CV and rolodex, that is, a set of career achievements and a network . . . similar to the concepts of reputation and recognition in the sociology of science.

Id.; *see also* Bourdieu, *supra* note 63, at 33.

> The struggle for scientific authority, a particular kind of *social capital* which gives power over the constitutive mechanisms of the field . . . owes its specificity to the fact that the producers tend to have no possible clients other than their competitors . . . . This means that in a highly autonomous scientific field, a particular producer cannot expect recognition of the value of his products ("reputation," "prestige," "authority," "competence," etc.) from anyone except other producers who, being his competitors too, are those least inclined to grant recognition without discussion and scrutiny.

Bourdieu, *supra* note 63, at 33.

68. *See* HESS, *supra* note 61, at 3.

> Decisions on appropriate methods, criteria for establishing replication, statistical measures, quantitative versus qualitative measurement, and so on are shaped by rhetoric, network politics, disciplinary cultures, personal reputations, gender socialization patterns, and so on. There is no single Scientific Method to which all scientists can refer; instead, laboratory procedures are opportunistic and contingent on social factors.

Id.

69. Interview with X, *supra* note 58; Interview with Y, *supra* note 59; Interview with Z, *supra* note 59.

INTERVIEW WITH X:

(1) *Cognitive Capacity and Limitations*: "I also recognize the fact that my experience is different from many other humans . . . . Males tend to not think about odors in the same way as females."

(2) *Standards as to What Is Interesing and Important*

   (a) *Funding*: "[I]t's important to have an income. In order to have an income you have to stay sort of within the mainstream of science."

(3) *Language*:

> [Our] language is going to change how you think about [mind and body], and this happens in the law when it comes to the insanity defense; our ideas about human behavior are so inherently dualistic ... I think changing the way people talk about this and reducing this dualistic language in terms of the descriptions of this physical world will actually be ... useful . . . .

(4) *Instruments*:

> It would be absurd to imagine that you didn't have constraints on the outcome you were expecting. By simply choosing a measurement instrument, a device to measure, you are generating hypotheses [that constrain] what's going to happen. We all know that you can measure one thing and have multiple things happening and then ignore those others.

(5) *Observation*:

> I think one of the more interesting things in science are those people who are at the edge ... and how they are either embraced or pushed out of regular scientific journals.... [Y]ou have a lot of expectations out there.... There are an infinite number of solutions so I have to constrain my solutions.

(6) *Institutional Gatekeeping*: "[W]hen I asked my advisor if we couldn't do some smell experiments in the lab he said, no, we can't, it's just too difficult to control odors and stimulate and so we're not doing that."

(7) *Methodological Variation*:

> [W]e lost evolutionary psychology.... It's actually impossible methodologically to compare a goldfish with a dog and people said of course and so it was dropped .... [T]hen suddenly in literally the last 5 years, you have had this re-emergence of people who want to say things about the comparative nature of animals .... I think that ... we're re-couching these things, they come and go as fashions, and that's troubling ....

INTERVIEW WITH Y:

(1) *Cognitive Capacity and Limitations*: [no significant remarks]

(2) *Standards as to What Is Interesting and Important*

(a) *Funding*:

> [T]here were pharmaceutical firms that were very interested especially in following this . . . . [T]hen [the researchers in England with whom Y was working] approached a couple of big firms that are, I guess a number of pharmaceutical companies get together and they have a granting agency that . . . support[s] this kind of research . . . . [T]he idea was there would [be] money to last for three years. Well, the money didn't go that far . . . . So they raised [money by going public].

(c) *Ethical Policy Constraints*: "[O]ur new administration put[s] such constraints on using fetal tissue for research. . . . That's an ethical issue that is so important . . . . [S]ocial values are getting in the way of scientific progress . . . in the [United] [S]tates . . . ."

(3) *Language*: [Y agreed that there's an aspect of persuasiveness in science, convincing others that one's work is valuable to get funding.]

(4) *Instruments*: [no significant remarks]

(5) *Observation*

(b) *Models*:

> You can create a stroke in rats by cutting off the carotid arteries and having them on a respirator for about ten minutes, but they lose about 60% of the animals, but they have the same kind of damage you have with a person who's had a heart attack. . . . So they [think] if [they] could directly damage those cells and not lose so many animals . . . that would be a good kind of animal model . . . .

(6) *Institutional Gatekeeping*: "So we did one study that for the longest [time just] sat there and nobody paid any attention to it, and just within about the last two or three years, people really started paying attention."

(7) *Methodological Variation*: [no significant remarks]

INTERVIEW WITH Z:

(1) *Cognitive Capacity and Limitations*: "I think it's not really practical or even advisable for a single person to try to cover all the bases [in every discipline]."

(2) *Standards as to What is Interesting and Important*

(a) *Funding*:

> I think a lot of us whenever we make a pitch to the NIH . . . have to indicate that there is some benefit [that] there is social good coming

> out of what we do.... [W]hen you have a constituency to which you're accountable, you've got to show some meaningful progress .... And that is, I think, [an] apparent restriction of thought.

(3) *Language*:

> [O]ne of the first things that I tell students in a class [is that] because of the nature of the material, ... they're going to have to learn a lot of vocabulary.... [I]t's like learning words in the dictionary [to enhance] the ability to be conversant and to communicate in a succinct and accurate way....

(4) *Instruments*: [no significant remarks]
(5) *Observation*
  (a) *Paradigms*:

> [T]here's a considerable amount of friction between [those] doing biological psychology, and people at other areas of the [psychology] department. Now the biological psychologists are in literally separated space .... [T]hose are people who have ... and I'll admit that I'm sort of one of them ... only a very peripheral interest in the overt behavior of the organisms that they study.

(6) *Institutional Gatekeeping*: Funding requests are forwarded "to a group of peers who are expert, to evaluate your work for several different things. One is scientific novelty. Another one is rigor of the proposal—feasibility.... And they take into consideration things like your track record; your biographical sketch is an essential component."
(7) *Methodological Variation*: [no significant remarks]

The value of ethnography, or conversation analysis, is its informality—part anthropological interview with a key informant and part journalistic profile. The setting allows for unanticipated turns and linkages in a way that questionnaires sent to scientists might not.[70] Scientists' own accounts of their work are not likely to emphasize the institutional and rhetorical aspects of their work, except in the case of

70. *See* Roger Smith & Brian Wynne, *Introduction, in* EXPERT EVIDENCE: INTERPRETING SCIENCE IN THE LAW 1 (Roger Smith & Brian Wynne eds., 1989) [hereinafter EXPERT EVIDENCE].

> We all benefited from extensive discussions with practitioners in the science-law area. Many of these discussions took the form of a relatively unstructured interview .... From the viewpoint of the empirical social sciences, our method is too informal to count as 'method'; but, given a deliberate orientation towards qualitative issues, it has served its purpose.

Id. at 11.

autobiographies. Nevertheless, ethnography is obviously a supplement to, and not a replacement of, the sociology of science generally (which establishes the theory that science is a social activity), the philosophy of science, and the history of science, each of which includes critical scholars who offer insights as to how science *really* works in contrast to popular or idealized accounts of scientific progress, including idealized accounts within their own discipline.

In the next section, I turn to the practical literature regarding the use of expert witnesses in trials, to show that the idealized version of scientific activity pervades that discourse among lawyers. My ultimate argument is that a discourse about science as a social enterprise would offer insights for lawyers to use in depositions, in *Daubert*-type hearings, during cross-examination, and in drafting jury instructions.

III. SCIENTIFIC EXPERTISE AND THE SOCIAL ASPECTS OF SCIENCE

> "Of course social factors influence the course of science. The only controversy concerns which social factors are operative in which situations and how powerful these social factors turn out to be."[71]

Expert scientific testimony is presented at trial in the context of a dispute. While the legal dispute is broader than and distinguishable from the scientific dispute, the disagreement between scientific experts becomes a matter for resolution by the judge or jury. Jeremy Green identifies three models of scientific disputes—contextual, ethnosociological, and epistemological explanations—that emphasize "different aspects of the scientific process."[72] Contextual explanations "might refer to differences in disciplinary perspectives, institutional or occupational affiliations, methodological and metaphysical commitments, or social interests."[73] In his study of English workers' compensation cases, Green found contextual explanations "comparatively rare."[74] Ethnosociological explanations, which might "refer to factors extraneous to the scientific content of the dispute—for example, to the competence, neutrality and openness, or the honesty of the participants" were slightly more common.[75]

71. Hull, *supra* note 15, at 74.
72. *See* Jeremy Green, *Industrial Ill Health, Expertise, and the Law, in* EXPERT EVIDENCE, *supra* note 70, at 93, 119.
73. *Id.* at 119.
74. *Id.*
75. *Id.*

> Most common of all were epistemological explanations; expert witnesses and barristers (the latter overwhelmingly so) preferred to explain disputes as caused by the absence of sufficient facts or by 'grey areas.' Disagreement was seen as a result of differences in interpretation, not differences as to what were the facts. . . . [S]ociologial critiques of expertise were regularly translated . . . into personal accusations of bias . . . . Both the preferred explanation—shortage of facts and legitimate differences in interpretation, and the rejected one—bias, are cast in individualistic terms. The contextual dimension, and accounts of commitments that do not entail personal bias, are excluded.[76]

The same tendency to exclude contextual explanations is evident in the discourse concerning scientific expertise among U.S. lawyers. For example, in *Impeachment of Witnesses: The Cross-Examiner's Art*, the chapter on impeaching expert witnesses emphasizes pretrial discovery, especially depositions, of adverse expert witnesses, as well as identifying publications and transcripts of previous testimony, as important to preparation.[77] In addition to questioning the qualifications of an expert and the consistency of the opinion with authoritative treatises,[78] impeachment can be based on "interest, bias, and motivation," that is, by showing that the expert is not "a disinterested professional."[79] While payment for testimony alone is not determinative, the fact that an expert derives a substantial portion of her income from testifying, that the expert always testifies for the same "side," that the expert opinion was prepared for trial and was not based on information and knowledge acquired as part of the expert's regular activities, or that professional conflict and jealousy may be present, can each provide a basis for impeachment.[80]

By reference to Green's models of scientific disputes,[81] the authors of *Impeachment of Witnesses* seem to view scientific disputes as epistemological (involving shortage of facts, differences in interpretation)[82] and ethnosociological (involving competence, neutrality),[83] *and* they

76. *Id.* at 120.
77. *See* ROBERTO ARON ET AL., IMPEACHMENT OF WITNESSES: THE CROSS-EXAMINER'S ART 140–47 (1990).
78. *Id.* at 147–51, 160–65.
79. *Id.* at 151.
80. *Id.* at 151–55.
81. *See supra* text accompanying note 71.
82. *See* ARON ET AL., *supra* note 77, at 158 (providing an example of a doctor who did not personally observe the deceased but relied on observations of a forensic pathologist without a lot of experience); *id.* at 154 (providing an example of a doctor with a new theory that is subject to substantial academic and scientific criticism).
83. *See id.* at 147–51 (discussing how to question qualifications); *id.* at 151–55 (discussing how to establish interest, bias, and motivation to show that the expert is not a "disinterested professional").

tend to cast bias, interest and motivation in individualistic terms. Contextual explanations are thereby eclipsed, and the fields of "disciplinary perspectives, institutional or occupational affiliations, methodological and metaphysical commitments, or social interests" are not mentioned as rich sources for impeachment materials, except as to personal qualifications and accusations of individual bias.[84]

In Malone and Zwier's *Effective Expert Testimony*, discussing cross-examination on financial or other bias, the same individualistic emphasis is apparent; financial or other bias are matters for impeachment.[85] Later in their treatise, the authors distinguish impeachment from "substantive" cross-examination,[86] and the treatise concludes with a chapter on examining reliability using the four *Daubert* factors (testability, error rate, publication, and general acceptance) and others (for example, science prepared for litigation, adequacy to explain important empirical data, basis in sufficient data, consistency, credentials, and derivation from mainstream approaches).[87] That is, when the authors consider more *contextual* matters involving the scientific community and not personal bias, the framework for examination reflects an idealized view of science nearly bereft of, in Green's formulation, the field of "disciplinary perspectives, institutional or occupational affiliations, methodological and metaphysical commitments, or social interests."[88] Indeed, earlier in their treatise, Malone and Zwier caution against "macro" lines of cross-examination that go "after the whole discipline": The "place to challenge an entire field is *in limine*, in a *Daubert*-type challenge."[89] Malone and Zwier have also developed a useful set of rules for deposing expert witnesses, including a recommendation that counsel ask what the expert relied on or decided not to rely on, who in the expert's field agrees or disagrees with the expert, and who did the collection of data (for

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84. Green, *supra* note 72, at 119. Roberto Aron et al. discuss cross-examining a witness who holds a novel theory that is out of step with mainstream science, but only to raise professional conflict and jealousy as a persuasive basis for impeachment. ARON ET AL., *supra* note 77, at 154. They also discuss *lack of* professional affiliations as a basis for questioning qualifications. *See id.* at 18–21.

85. DAVID M. MALONE & PAUL J. ZWIER, EFFECTIVE EXPERT TESTIMONY 164 (2000).

86. *Id.* at 187. Substantive cross-examination, in contrast to impeaching inquires, involves factual issues in the case—why a bracket failed or whether tests were conducted.

87. *See id.* at 223–33.

88. Green, *supra* note 72, at 119. In fairness, the reliability inquiry could potentially lead to evidence regarding disciplinary perspectives (for example, explaining why a novel theory has not been published), but institutional affiliations, methodological commitments, or social interests (for example, funding for certain kinds of research but not others, due to corporate, government, or public influence) do not appear in legal discourse as factors relevant to reliability.

89. MALONE & ZWIER, *supra* note 85, at 193.

example, assistants),[90] each of which could lead to a more contextual approach, but tend not to because of the pervasive emphasis on a particular, individual expert and not the community or discipline.[91]

Professor Edward Imwinkelried's *The Methods of Attacking Scientific Evidence* identifies the potential of bias (i) in favor of a particular instrument or technique in which an expert has a financial interest,[92] (ii) originating in prior knowledge and opinions,[93] (iii) due to prior occupation (for example, law enforcement),[94] (iv) in favor of a particular party or type of party (for example, large corporations, or injured plaintiffs),[95] (v) related to indirect financial interests (for example, affiliation with an organization or university that "receive[s] grants from a party to the lawsuit or from the same industry"),[96] or (vi) in favor of a particular theory or technique, which Imwinkelried calls "doctrinal bias."[97] While Imwinkelried frames these types of bias in the individualistic terms typical of legal discourse concerning impeachment, the contours of a contextual or communal notion of bias begin to emerge. Green's field of "disciplinary perspectives, institutional or occupational affiliations, methodological and metaphysical commitments, or social interests"[98] need not be *translated*, as usual,[99] into personal accusations of bias—Imwinkelreid even refers to prior knowledge, opinion, and occupations as "innocent" or "subconscious" bias that lacks the bad faith associated with crass financial interests.[100] Instead of characterizing bias as the opposite of good science, disciplinary, institutional, occupational, or methodological bias can be seen as part of science. As to the former

90. *See* DAVID M. MALONE & PAUL J. ZWIER, EXPERT RULES: 100 (AND MORE) POINTS YOU NEED TO KNOW ABOUT EXPERT WITNESSES 49–53 (2d ed. 2001).

91. Again, in fairness, the question: "Who in this field agrees with your methodology?," *id.* at 52, seems to implicate a discipline or scientific community, but the only point is that a particular expert may be an "outlier," *id.*, in conflict with other experts. The idea that scientific communities—their language, values, consensus-building techniques, and institutions—represent a force, alongside nature in the production of knowledge, is not suggested.

92. EDWARD J. IMWINKELRIED, THE METHODS OF ATTACKING SCIENTIFIC EVIDENCE 118 (3d ed. 1997).

93. *Id.* at 250.

94. *Id.* at 252.

95. *Id.* at 255.

96. *Id.* at 256.

97. *Id.* at 257.

98. *See* Green, *supra* note 72, at 119.

99. *See supra* text accompanying note 76.

100. IMWINKELRIED, *supra* note 92, at 252.

characterization—bias is bad—consider F. Lee Bailey and Henry B. Rothblatt's *Cross-Examination in Criminal Trials*: "The good forensic pathologist is detached and objective. However, you will encounter some pathologists who will not be quite so professional. . . . You must be aware at the signs indicating that a medical examiner is biased . . . ."[101]

Contrast that warning with Dr. Bernard Diamond's view that "all witnesses, regardless of who engaged them, identify closely with their own opinions and unintentionally introduce as a result a certain degree of bias and deviation from their oath to tell the truth . . . ."[102] Experts are "bound to be biased and partial and strongly motivated toward advocacy of [a] particular prejudiced point of view."[103] Inevitable bias in science is more readily acknowledged among social scientists as "human beings with social identities, beliefs, and values that link them to some causes and parties more than to others."[104] However, the notions that social science "depends upon, benefits from, and is interdependent with extrascientific institutions," and that "financial allocations have overt or covert strings attached [that] shape what gets studied [and] perhaps what is concluded,"[105] are arguably applicable to the hard sciences. For example,

> it is not possible to pursue either a programme of research or a career in [complex areas of medicine] in isolation from a limited set of key institutions; and the high financial costs of maintaining these institutions have led researchers towards an increasingly close relationship with those industries that have a direct interest in the extent and content of knowledge about particular kinds of ill health.[106]

Nevertheless, such "social" analyses of expertise seem rare in the treatises on cross-examination of expert witnesses.

Roger Smith and Brian Wynne have attempted to explain the lack of attention to contextual—disciplinary, institutional, occupational, and methodological—models of scientific disputes in law.[107] They identify a persistent "hope that the objectivity of science will provide a firm and authoritative input, giving decisions a factual basis that cannot be questioned. That the science often appears equivocal is put down to

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101. F. LEE BAILEY & HENRY B. ROTHBLATT, CROSS-EXAMINATION IN CRIMINAL TRIALS 241 (1978).

102. Bernard L. Diamond, *The Fallacy of the Impartial Expert, in* READINGS IN LAW AND PSYCHIATRY 217, 218–19 (Richard C. Allen et al. eds., 1975).

103. *Id.* at 221.

104. *See* MARK A. CHESLER ET AL., SOCIAL SCIENCE IN COURT: MOBILIZING EXPERTS IN THE SCHOOL DESEGREGATION CASES 63 (1988).

105. *See id.* at 63–64.

106. Green, *supra* note 72, at 126.

107. *See generally* Smith & Wynne, *supra* note 70.

procedural problems rather than inherent properties of scientific knowledge or methods . . . ."[108]

Legal institutions function under pressures, "under constraints of time and finite resources," that narrow the agenda in challenges of expertise.[109]

> Conditions of pressure therefore have the consequence that legal or expert practitioners tend to perceive and think about the science-law relation at the level of detail . . . . There may be difficulties of interpretation, agreement, efficiency, and such like, at this level, but . . . [t]o accept that there may be more basic or general problems would be to accept potential problems in the construction of authority by the institutions of science and law themselves. . . .
>
> It is therefore hardly surprising that what literature there is on scientific expertise in the law is mostly concerned with detailed and specific matters about knowledge or procedure . . . .[110]

Insights from the sociology of scientific knowledge, for example, "that even the most disciplined and objective observation is never free of theoretical and thus interpretive precommitments,"[111] or that a "proven fact is ultimately a social achievement among scientists,"[112] do not pass easily into law as general propositions. The laws of evidence have *already* constituted "certain kinds of knowledge as expert,"[113] and even though particular scientists are fair game for challenge, the "metaphysics of law is that . . . 'science' is reliable."[114] On the other hand, "legal procedures often actually generate sceptical pressures on scientific expertise. Indeed, such procedures sometimes show that 'established scientific fact' is riddled with suppositions, unstated limiting conditions, and other qualifications or uncertainties."[115] That is, lawyers regularly "deconstruct" scientific claims[116]—the "formal legal process can be

108. *Id.* at 1.
109. *Id.* at 3.
110. *Id.* at 4–5.
111. *See* Brian Wynne, *Establishing the Rules of Laws: Constructing Expert Authority, in* EXPERT EVIDENCE, *supra* note 70, at 23, 23.
112. *Id.* at 28.
113. *See id.* at 32.
114. *See id.* at 54.
115. *Id.* at 32.
116. *Id.*

> [Legal] procedures sometimes show that 'established scientific fact' is riddled with suppositions, unstated limiting conditions, and other qualifications or uncertainties.
>
> For instance, if one side in a case advances an expert claim stripped of those nuances, the other side, if competent, may then proceed to reintroduce them . . . . This 'deconstructs' the other side's scientific claim, showing it to be made up of empirically unwarranted 'collective opinion.'

described as institutionalized pure mistrust."[117] Nevertheless, the usual limits of cross-examination, as well as any imbalance of resources among parties, "restrict the free play of scepticism."[118] Most important, legal institutions refer "to natural scientific authority in constructing [their] own social authority,"[119] and through judicial decision making, "the private intellectual bloodbath of ... court exchanges may be translated into a more orderly account for public consumption."[120] Scientific knowledge, that is, is "open to deconstruction and reconstruction."[121] Finally, to the extent that there is a tension between the view that scientific knowledge or method is superior to and capable of settling the arguments of interested legal parties or advocates, on the one hand, and the view that scientific "knowledge" is "capable of being manipulated to reflect and support any interests which have the resources to 'buy' expertise,"[122] lawyers and judges seem to hold the former view. "Bought" or "manipulated" knowledge is not science, and ideally is to be eliminated prior to its introduction (in a *Daubert*-type hearing) or discredited during cross-examination.

The purpose of this Article is to identify a field of inquiry between the extremes of an idealized view of science, in which social aspects (such as institutional gatekeeping, professionalization, consensus-building techniques, and discursive regimes) are considered irrelevant, and the conception that social factors (like funding, prior occupation, theoretical commitment, and organizational affiliations) signal individual bias and not expertise. The former view emphasizes *core* aspects of science such as theory, data, test, conclusion, and publication and acceptance, while eclipsing any substantial inquiry into the social aspects of science; the latter emphasizes arguably social aspects of science but only as errors to be eliminated. Neither view leads to a complex picture of the scientific enterprise as a social, rhetorical, and institutional practice.

Consider again my catalogue of social aspects of science, as they are presented in Diagram III to contrast with and replace Diagram II, which downplayed social factors as eliminable or as merely background features.

Id. While sociologists of science accept that "scientific conflicts offer the most fruitful examination of scientific knowledge in-the-making, because the adversarial pressure forces the premises and conventions of each side out into the open," legal "contexts could be described as a special case of this general type." *Id.* at 33.

117. *Id.* "Legal processes enshrine scepticism and mistrust: cross-examination has a duty to question as fully as possible the adversary's case in front of the judge or jury." *Id.* at 37.

118. *See id.* at 36.

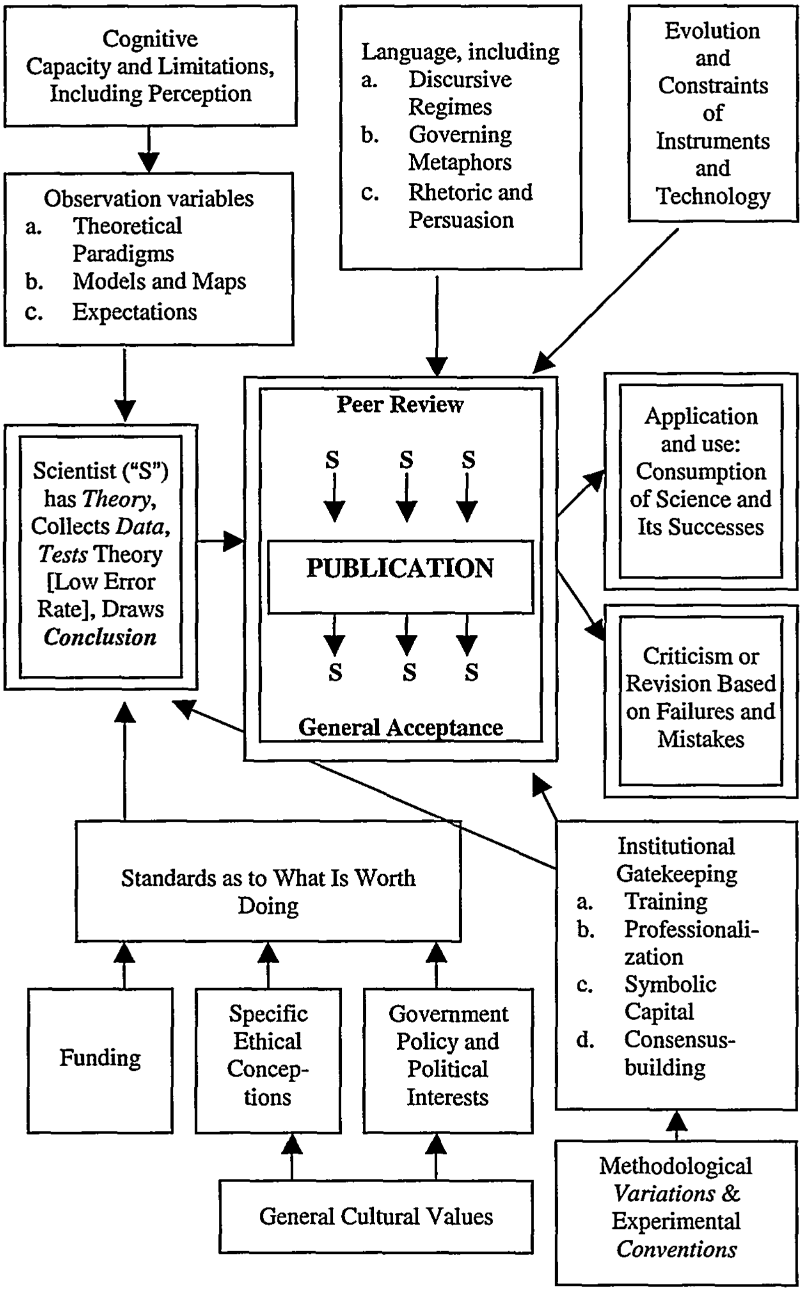
119. *See id.* at 37.

120. *See id.* at 38. "Reconstruction from the ensuing intellectual debris is not the expert's, but the judge's responsibility." *Id.* at 37.

121. *See id.* at 49.

122. *See id.* at 53.

DIAGRAM III


Cognitive Capacity and Limitations, Including Perception
Language, including a. Discursive Regimes b. Governing Metaphors c. Rhetoric and Persuasion
Evolution and Constraints of Instruments and Technology
Observation variables a. Theoretical Paradigms b. Models and Maps c. Expectations
Peer Review
S S S
PUBLICATION
S S S
General Acceptance
Scientist ("S") has *Theory*, Collects *Data*, *Tests* Theory [Low Error Rate], Draws *Conclusion*
Application and use: Consumption of Science and Its Successes
Criticism or Revision Based on Failures and Mistakes
Standards as to What Is Worth Doing
Institutional Gatekeeping a. Training b. Professionalization c. Symbolic Capital d. Consensus-building
Funding
Specific Ethical Conceptions
Government Policy and Political Interests
General Cultural Values
Methodological *Variations* & Experimental *Conventions*

Under an idealized view of science that concedes the presence of the aspects in Diagram III, but considers them usually irrelevant or even admirable, and under the complimentary view that social aspects otherwise signal eliminable bias, the features depicted are not likely to be seen as important to legal evaluations of scientific claims. My argument is that they are important because they are typical structures in science, and could in particular cases be useful avenues of inquiry in pretrial depositions or in cross-examination, and also provide useful insights in drafting jury instructions. Significantly, I am not suggesting that the presence of such aspects is especially, in particular cases, good or bad. One can argue that the gatekeeping mechanisms of science are good for scientific progress, and an attorney whose expert witness has been attacked for belonging to a narrow school of thought could use such an argument to reconstruct integrity.

On the other hand, institutional gatekeeping might signal impeachable bias against novel but unpublished research. Inevitable aspects of science, like language, instrumentation, theoretical commitments, and funding sources are best approached neutrally, as though they may or may not be significant, and as though they may be positive or negative features, in a particular case. Consequently, an inquiry along these lines might be as reconstructive as it is deconstructive. In any event, scientific expertise will be presented as it really is, neither idealized nor as subject to crass manipulation. Social aspects are neither uniformly wonderful nor instances of eliminable bias—they are present, for good or bad, in particular cases involving scientific claims.

David Malone and Paul Zwier, in *Expert Rules: 100 (and More) Points You Need to Know About Expert Witnesses*, suggest attorneys deposing expert witnesses always ask about opinions formed, what was done to reach them, how it was done, why it was done, what assumptions were made, whether there are reliable authorities in the field, and what tasks were not done; other recommended questions include what did the expert review and decide not to rely on, who in the field agrees or disagrees with the expert, the *Daubert* questions (peer review and publication, error rate, general acceptance, and testability and replicability), who selected the materials reviewed, and who are the assistants who collected data.[123] This approach, despite its typical idealization of science and emphasis on individual bias (science is good; it is scientists that are bad), begins to touch on some elements of socialinquiry: (1) asking what the expert decided *not* to do, or *not* to rely on, refers both to conventions in the field and to standards as to what is

123. *See* MALONE & ZWIER, *supra* note 90, at 49–56.

worth looking into; (2) asking who selected the data to review (for example, counsel), and about the assistants who collected data, refers to the effect of expectation on experimental conventions, and to professionalization, respectively. One could just as easily ask about institutional funding for this type of research, policy or ethical constraints, dominant models or theories, and limitations of measurement technology, but a more informal line of questions similar to those used in ethnomethodology (or conversation analysis) might reveal more about the presence of such aspects. That is, asking experts to describe generally what they do, who funds their work, where such work is done, other scientists with whom they work or communicate regularly, what gets in the way of progress in the field, and even failed experiments or theories, will produce a transcript for analysis of social aspects. Again, these conversational answers will not likely reveal intentional bias, but innocent or inevitable bias, including cognitive or technological limitations. Both at a *Daubert*-type hearing before trial, which permits inquiry beyond "the four factors" (since the *Daubert* test is flexible and not written in stone),[124] and at trial during cross-examination, the goal is not only to dismiss or impeach experts, but also to understand the limits of and constraints upon the methodology and results offered. Cross-examination along these lines begins to look like sociological skepticism of scientific certainty—claims that are presented as natural or obvious are seen as the product of consensus-building techniques, gap-filling assumptions, experimental conventions and tentative models of nature.[125]

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124. *See* FED. R. EVID. 702 advisory committee's note.

> *Daubert* set forth a non-exclusive checklist for trial courts to use in assessing the reliability of scientific expert testimony. . . .
>
> . . . .
>
> No attempt has been made to "codify" these specific factors. *Daubert* itself emphasized that the factors were neither exclusive nor dispositive. . . .
>
> . . . .
>
> Courts both before and after *Daubert* have found other factors relevant in determining whether expert testimony is sufficiently reliable . . . .

Id.

125. *See* Wynne, *supra* note 111, at 33–34.

> [S]tudies of scientific controversies have shown how scientific knowledge taken as natural and universal by one school may be exposed as a tissue of selective observations based upon a limited set of localized technical practices and theoretical resources, and accepted inference bridges across gaps in evidence, while partly leaning for credibility upon commitments to adjacent bodies of knowledge which are similarly constructed. The whole edifice is a network of combined social-cognitive commitments. . . . [Sceptics] can cut away the apparently 'natural' warrants in these inference bridges and network

As to jury instructions, Charles Kester has proposed that juries be instructed to consider that all science is socially constructed and relies upon dominant theoretical paradigms, unproven assumptions, and communal interaction.[126] While Kester's proposal relies on his own questionable assumption that scientists understand and concede the social construction of reality,[127] his proposal would make more sense if juries actually had some evidence of exactly how science works as a social enterprise. If experts were deposed or cross-examined on the cognitive and instrumental limitations on their interpretations, the effects of funding and cultural values on their work, the networks of persuasion in which they work, the dominant theories and models to which they ascribe, and the gatekeeping mechanisms of science, an instruction to consider those matters in their deliberation would be meaningful.

IV. SELF-CRITICISMS AND CONCLUSION

Because I have couched my otherwise theoretical, interdisciplinary argument in practical terms, I anticipate the concern of practitioners that careful attention to the social aspects of the scientific enterprise would further complicate an already complex field of inquiry. Given the numerous and lengthy volumes on how to conduct cross-examination of expert witnesses, it is hard to imagine the subject could be further complicated. Nevertheless, considering the social aspects of science can be seen only as an expansion of the flexible *Daubert* inquiry, the various bases for impeachment, and the suggested lines of questioning for cross-examination. On the other hand, the social studies of science on which I have relied have as their goal a more accurate account of the way scientists actually work and of the way scientific knowledge is produced. I think it is more accurate, that is, than either (i) the idealized view of science as exclusively anchored in nature (rather than, alongside nature, in language, rhetoric, interpretation, values, social interests, and institutions) *or* (ii) the deflationary view of scientists as variously greedy, for sale, or

crutches, showing the subtle social, conventional character of the knowledge.
Id.

126. *See* Kester, *supra* note 19, at 545–46. Kester proposes that juries be instructed, in part, as follows: "Although this [qualified witness's] expertise provides the [witness] with specialized knowledge, it may also have the effect of giving the [witness] a vested interest in publishing certain papers, or in seeing a certain outcome, or in maintaining [the witness's] status.... [T]he opinion stated ... was based on certain assumptions." *Id.* at 565–66.

127. Kester claims that "realist accounts of verification and falsificationism have largely been rejected," *id.* at 567, but I remain suspicious. I instead agree with Callebaut, *supra* note 38, at xvi, that a "majority of philosophers ... defend some variety of realism," and that a naturalist turn is evident among historians, philosophers and sociologists of science.

precommitted to their corporate employer, their funding source, or their favorite theory. The *Daubert* opinion itself,[128] though not revolutionary, succeeded in establishing a new regime for evaluation of expert scientific testimony, reflected in the new Federal Rule of Evidence 702, that is more complex than the *Frye* regime,[129] all of which is justifiable on the basis that scientific reliability is a more complex matter than "general acceptance." And while the *Daubert* four-part test is not sufficiently attentive to the social aspects of science, the flexible approach that accompanies most references to the *Daubert* hearing anticipates new factors that will become part of *Daubert*-type analyses.[130] Even without attention to the social aspects of science, the test for reliability becomes more complicated as new situations confront federal courts.

A related concern might be the effect of inquires into social aspects on trial judges, who, despite the promise that they are given "considerable leeway" as to how to conduct *Daubert* hearings, and when to allow or disallow a particular expert's testimony,[131] are regularly reversed for being too lenient in admitting questionable testimony,[132] or for being too restrictive in disallowing potentially useful testimony.[133] Sometimes a judge thinks a sufficient *Daubert* hearing was held, but an appellate panel does not see enough careful and meticulous attention.[134] Sometimes the *Daubert* hearing is misused or misunderstood in the eyes of an appellate panel.[135] Given that it is an abuse of discretion to rely on

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128. 509 U.S. 579 (1993).

129. *See* Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923) (holding that expert opinion is admissible if based on principle or discovery sufficiently established to have gained general acceptance in the field to which it belongs).

130. *See* discussion, *supra* note 125.

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

132. *See, e.g.*, Elcock v. Kmart Corp., 233 F.3d 734, 744–50 (3d Cir. 2000) (holding that the trial judge misapprehended the gatekeeping requirement of Federal Rule of Evidence 702 by admitting testimony of an expert based on an untested, novel method; the case was remanded for a *Daubert* hearing).

133. *See, e.g.*, Jahn v. Equine Services, PSC, 233 F.3d 382, 389 (6th Cir. 2000) (stating that the trial judge "employed a standard of admissibility more stringent than that expressed Federal Rule of Evidence 702").

134. *See, e.g.*, U.S. v. Smithers, 212 F.3d 306, 314, 324 (6th Cir. 2000) (stating that the trial court abused its discretion in excluding expert on eyewitness identification without first conducting a *Daubert* hearing; the dissent argued that the trial judge conducted its *Daubert* analysis properly).

135. *See, e.g.*, Smith v. Ford Motor Co., 215 F.3d 713, 721 (7th Cir. 2000) (stating that the trial "court erred by relying on a single, potentially irrelevant, criterion to

a "forbidden factor" in *Daubert*-type analyses,[136] what is the status of an inquiry into the social aspects of science?

Questions about the effects of funding, persuasion, evolving instruments, observational variables, and institutional gatekeeping would fit into both the flexible *Daubert* hearing and into conventional cross-examination techniques, but they are not a substitute for questions about *core* elements of science—hypothesis, data and methodology—or concerns about intentional bias on the part of a particular scientist. My point is that in some cases, evidence of social aspects might be useful to consider, for negative or positive assessments of the reliability of particular testimony. Judges should welcome such useful, mediating insights, but I concede that these are not the usual factors.

The greatest concern is one that has been identified in social studies of science that engage in "epistemologically symmetrical" analyses. That is, the social aspects of science have been used in traditional analyses to explain errors or unscientific knowledge;[137] as scientific controversies arise, one side is correct because it accurately describes nature, and the other side's mistake is explained, for example, by funding bias, political pressure, a corporate agenda, a faulty theoretical paradigm, or poor training. Relativistic social scientists, in contrast, do not assume that one side is correct, or on the side of nature. Rather, their epistemologically symmetrical analyses assume that social aspects are at work in both sides of a controversy.[138] However,

> an epistemologically symmetrical analysis of a controversy is almost always more useful to the side with less scientific credibility . . . . The side with fewer scientifically or socially credentialed resources is more likely to attempt to enroll the [neutral] researcher, whereas the better-credentialed side views an epistemologically symmetrical analysis as threatening to its cognitive and social authority, and it is more likely to react to the analyst with hostility or suspicion.[139]

In legal-scientific disputes, the risk of introducing the social aspects of science (in depositions, *Daubert* hearings, cross-examination, or jury instructions) is that they will become challenges to mainstream science. In this Article, I have taken a modified "symmetrical" position that social

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determine that plaintiff's proposed experts based their conclusions on methodologies that are not sufficiently reliable to satisfy the requirements of Federal Rule of Evidence 702").

136. *See* Powell v. AT&T Communications, Inc., 938 F.2d 823, 825 (7th Cir. 1991) (holding that the court abuses discretion when relying "on a forbidden factor or failure to consider an essential factor").

137. *See* Pam Scott et al., *Captives of Controversy: The Myth of the Neutral Social Researcher in Contemporary Scientific Controversies*, 15 SCI., TECHNOLOGY, & HUM. VALUES 474, 474–75 (1990).

138. *See id.* at 475.

139. *Id.* at 490.

aspects of science are not signals of eliminable bias, but that they are inevitable aspects of the scientific enterprise, for good or bad. Social aspects, like institutional gatekeeping or dominant theoretical paradigms, can be shown to be positive supports for useful knowledge. They can also be shown to be constraints on scientific progress. My conclusion is neither that social aspects are always useful to novel scientific proposals, nor that they are always useful to mainstream science. My conclusion is that they are present, and that any inquiry into the reliability of a particular expert opinion should include, alongside the *core* aspects of idealized accounts of science and alongside the conventional notion of bias, a careful consideration of the social aspects of the scientific enterprise.