2007

Supersizing Daubert Science for Litigation and Its Implications for Legal Practice and Scientific Research

Gary Edmond

Follow this and additional works at: https://digitalcommons.law.villanova.edu/vlr

Part of the Evidence Commons

Recommended Citation
Available at: https://digitalcommons.law.villanova.edu/vlr/vol52/iss4/6

This Symposia is brought to you for free and open access by Villanova University Charles Widger School of Law Digital Repository. It has been accepted for inclusion in Villanova Law Review by an authorized editor of Villanova University Charles Widger School of Law Digital Repository.
SUPERSIZING DAUBERT SCIENCE FOR LITIGATION AND ITS IMPLICATIONS FOR LEGAL PRACTICE AND SCIENTIFIC RESEARCH

GARY EDMOND*

I. INTRODUCTION: SCIENCE FOR LITIGATION AND THE EXCLUSIONARY ETHOS

This essay is about science for litigation, its implications and limitations. Science for litigation is expert evidence specifically developed for litigation—extant, pending or anticipated. Although, as we shall see, federal judges have demonstrated a curious tendency to restrict the concept to expert evidence associated with litigation which is underway or pending. Ordinarily, expert evidence developed for, or tailored to, litigation carries an epistemic stigma. Conventionally, the fact that expert evidence is oriented toward a specific goal is thought to impair the independence of the experts and the reliability of their evidence. This often manifests in concerns that evidence developed for litigation is inconsistent with scientific knowledge and the opinions of experts not embroiled in litigation. That is, scientific evidence designed or refined for legal purposes is thought to be in tension with the independent and methodologically rigorous research undertaken by the scientific community. In consequence, science for litigation is widely considered unreliable and for that reason threatening to routine legal processes. The experts responsible for science for litigation are often vilified as partisan and sometimes even condemned as charlatans.

* BA(Hons) University of Wollongong, LLB(Hons) University of Sydney, PhD University of Cambridge. Associate Professor, Faculty of Law, The University of New South Wales, Sydney 2052, Australia. Email: g.edmond@unsw.edu.au. I would like to thank Villanova Law School for the generous invitation as well as David Caudill, Lash LaRue, Penny Pether, Robert Nelson, David Mercer, an anonymous referee, Gretchen Tomanek, Ian Mahoney and the other editors of the Villanova Law Review. A version of this paper was presented at the Norman J. Shachoy Annual Symposium, Villanova Law School and to the Faculty of Law, University of Windsor.

1. When I use the italicized form (e.g., science for litigation), I am referring to the concept of science for litigation rather than evidence which is developed, prepared or organized with some sensitivity to actual or anticipated litigation.

2. “Anticipated” might be interchanged with “foreseeable.” There are many different names for science for litigation including “litigation-generated science,” “junk science,” “partisan science” and so on. For an informative discussion, see David Michaels & Les Boden, Litigation-Generated Science: Why Should We Care?, ENVTL. HEALTH PERSP. (forthcoming).

3. For a variety of reasons, not always principled or convincing, less attention is focused on the lawyers.
This essay reviews these ideas. It aims to challenge some of the commitments guiding contemporary evidence jurisprudence and to refocus attention on expert evidence and scientific knowledge developed in anticipation of litigation. To state the position more starkly, this essay will explain how recent judicial characterizations of expertise bear limited resemblance to modern forms of practice or recent scholarship on science and technology. The practical upshot is that federal judges have developed admissibility tools, like science for litigation, which are not only remote from real world practice but draconian and potentially misleading.

Even though expert evidence is always selected, adapted, translated and simplified for use in legal contexts, this essay will not pretend that we should disregard these processes. Instead, it endeavors to explain why origins and orientations should be the starting point of any assessment rather than some banal conclusion. In developing this position, this essay explains how the kinds of scientific research which are normally considered to be independent and reliable have much in common with the expert evidence often pejoratively associated with plaintiffs and partisan experts. For, a large proportion of modern scientific research, especially research related to pharmaceuticals, chemicals, weapons, the environment and public health, is sponsored by for-profit corporations and oriented to their commercial interests. Modern corporations are acutely sensitive to regulatory requirements and the possibility of being sued for the effects of their products and activities. While the ideas motivating judicial recourse to science for litigation are faulty, by applying the concept more consistently (i.e. more symmetrically) to the evidence sponsored and developed by civil defendants and even the state’s forensic scientific evidence, we can begin to identify limitations and reflect on how we might begin to reform practice.

The essay also suggests, although these ideas are developed in greater detail elsewhere, that in recent decades judges have imposed more onerous admissibility standards in response to a range of converging social, logistical and ideological pressures. The flawed models of science and expertise guiding legal practice and federal jurisprudence have not, after all, sprung from nowhere. It is important, therefore, to approach recent changes in admissibility jurisprudence with sensitivity to a range of broader socio-legal problematic. These problems include widespread anxiety about too much litigation; legal irrationality; insurance crises; the prevalence of unreliable expertise and “junk science”; the capabilities of


5. See JOSEPH GUSFIELD, THE CULTURE OF PUBLIC PROBLEMS (1976). Whether these are actually problems remains controversial, although almost all of the scholarly research suggests popular impressions are grossly exaggerated.
lay juries; and the debilitating impact of litigation on research and innovation, especially investment in pharmaceuticals and manufactured goods.

So, with this in mind, we move to consider science for litigation and some of its practical limitations and political implications. It will soon become clear that, as it stands, the concept performs important ideological work. Science for litigation systematically disadvantages civil plaintiffs. It reinforces the threat posed by socio-legal problems and the perceived need for continuing judicial vigilance. Focusing attention on the expert evidence adduced by plaintiffs also elides the activities and strategic orientations of research sponsored by for-profit corporations.

A. Background: Daubert and the Exclusionary Ethos

In order to orient this discussion, it is important to describe the exclusionary ethos emerging in the federal circuits during the 1980s and consolidated by the Supreme Court’s Daubert, Joiner and Kumho decisions in the 1990s. By now most readers will be conversant with the Supreme Court’s Daubert Trilogy so this introduction will be appropriately succinct.

In Daubert v. Merrell Dow Pharmaceuticals, Inc., the Supreme Court heard an appeal on the admissibility standard for scientific evidence in federal courts according to the Federal Rules of Evidence. Rule 702 of the Federal Rules of Evidence governs the admissibility of “scientific, technical, or other specialized knowledge.” Justice Blackmun, author of the majority opinion, advocated a “relevance and reliability” approach. He explained that the Federal Rules of Evidence required “scientific knowl-

11. See id.
12. At the time of the Daubert appeal, Rule 702 stated: “If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise.” Fed. R. Evid. 702 (1975). It was amended in 2000 as follows:
If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise, if (1) the testimony 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. Fed. R. Evid. 702 (2000). Other rules, such as Rules 102, 401, 402, 703, 704 and 705, may also have a bearing on admissibility decision-making.
edge" to "be derived by the scientific method." To help federal judges identify reliable scientific knowledge, Blackmun provided the following criteria (hereafter "the Daubert criteria"):

[1] Ordinarily, a key question to be answered in determining whether a theory or technique is scientific knowledge that will assist the trier of fact will be whether it can be (and has been) tested. "Scientific methodology today is based on generating hypotheses and testing them to see if they can be falsified; indeed, this methodology is what distinguishes science from other fields of human inquiry." Green 645. See also C. Hempel, Philosophy of Natural Science 49 (1966) ("[T]he statements constituting a scientific explanation must be capable of empirical test"); K. Popper, Conjectures and Refutations: The Growth of Scientific Knowledge 37 (5th ed. 1989) ("[T]he criterion of the scientific status of a theory is its falsifiability, or refutability, or testability") (emphasis deleted).

[2] Another pertinent consideration is whether the theory or technique has been subjected to peer review and publication. Publication (which is but one element of peer review) is not a sine qua non of admissibility; it does not necessarily correlate with reliability, see S. Jasanoff, The Fifth Branch: Science Advisors as Policymakers 61-76 (1990), and in some instances well-grounded but innovative theories will not have been published, see Horrobin, The Philosophical Basis of Peer Review and the Suppression of Innovation, 263 JAMA 1438 (1990). Some propositions, moreover, are too particular, too new, or of too limited interest to be published. But submission to the scrutiny of the scientific community is a component of "good science," in part because it increases the likelihood that substantive flaws in methodology will be detected. See J. Ziman, Reliable Knowledge: An Exploration of the Grounds for Belief in Science 130-33 (1978); Relman & Angell, How Good Is Peer Review?, 321 New Eng. J. Med. 827 (1989). The fact of publication (or lack thereof) in a peer-reviewed journal thus will be a relevant, though not dispositive, consideration in assessing the scientific validity of a particular technique or methodology on which an opinion is premised.

[3] Additionally, in the case of a particular scientific technique, the court ordinarily should consider the known or potential rate of error, see, e.g., United States v. Smith, 869 F.2d 348, 353-54 (7th Cir. 1989) (surveying studies of the error rate of spectrographic voice identification technique), and the existence and maintenance of standards controlling the technique's operation, see United States v. Williams, 583 F.2d 1194, 1198 (2d Cir. 1978) (not-

14. Id. at 590.
ing professional organization’s standard governing spectrographic analysis), cert. denied, 439 U.S. 1117 (1979).

[4] Finally, “general acceptance” can yet have a bearing on the inquiry. A “reliability assessment does not require, although it does permit, explicit identification of a relevant scientific community and an express determination of a particular degree of acceptance within that community.” United States v. Downing, 753 F.2d 1224, 1238 (3d Cir. 1985). See also 3 Weinstein & Berger ¶ 702[03], pp. 702-41 to 702-42. Widespread acceptance can be an important factor in ruling particular evidence admissible, and “a known technique which has been able to attract only minimal support within the community,” Downing, 753 F.2d at 1238, may properly be viewed with skepticism.15

According to the majority, these criteria were to be applied flexibly: “The inquiry envisioned by Rule 702 is, we emphasize, a flexible one.”16

Two of the justices, Chief Justice Rehnquist and Justice Stevens, dissented on the ground that the majority placed excessive reliance on definitions of science drawn from non-legal writings (e.g., the work of the philosophers Hempel and Popper referred to in [1])—effectively requiring judges to “become amateur scientists.”17 Though confident about the capabilities of federal judges, Rehnquist (and Stevens) expressed pragmatic reservations.18 He was unsure what “falsifiability” meant and how it might be applied by legally-trained judges:

The Court [i.e. the majority] speaks of its confidence that federal judges can make a “preliminary assessment of whether the reasoning or methodology underlying the testimony is scientifically valid and of whether that reasoning or methodology properly can be applied to the facts in issue.” . . . The Court then states that a “key question” to be answered in deciding whether something is “scientific knowledge” “will be whether it can be (and has been) tested.” [see [1] above] Following this sentence are three quotations from treatises, which not only speak of empirical testing, but one of which states that the “‘criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.’”

I defer to no one in my confidence in federal judges; but I am at a loss to know what is meant when it is said that the scientific status of a theory depends on its “falsifiability,” and I suspect some of them will be, too.

17. See id. at 598-601 (Rehnquist, J., dissenting).
18. See id.
I do not doubt that Rule 702 confides to the judge some gatekeeping responsibility in deciding questions of the admissibility of proffered expert testimony. But I do not think it imposes on them either the obligation or the authority to become amateur scientists in order to perform that role.¹⁹

Six years after Daubert, the Supreme Court heard another appeal on the admissibility of expert evidence. This time the Court addressed the standard for non-scientific forms of expert evidence. This was the other portion of Rule 702 concerned with “technical, or other specialized knowledge.” In Kumho Tire Co. v. Carmichael,²⁰ the Court firmly endorsed the Daubert decision—no dissent this time—explaining that the four criteria might be applicable in all expert evidence admissibility determinations.²¹

Once again the Court emphasized the importance of “flexibility” in the application of the Daubert criteria.²² Unlike the Daubert appeal—which was remanded to the Ninth Circuit Court of Appeals—in Kumho the majority applied the new admissibility standard for “technical, or other specialized knowledge” to the case before them.²³ With the exception of Justice Stevens, the Court excluded the plaintiffs’ engineering evidence because it did not pass the inflexible application of all four of the Daubert criteria.²⁴

Daubert and Kumho Tire consolidated a more widespread tightening of admissibility standards and encouraged trial judges to act uncompromisingly as evidentiary gatekeepers.²⁵ The choice of the gatekeeping metaphor might be considered quite revealing in this context. In General Electric Co. v. Joiner,²⁶ another appeal concerned with expert evidence, the Supreme Court underscored the importance of judicial gatekeeping by insulating the trial judge’s admissibility determinations from appellate review.²⁷ After Joiner, appellate courts were required to identify an abuse of the trial judge’s discretion to activate their power to intervene. The Supreme

¹⁹. Id. at 600-01 (internal citations omitted).
²¹. See id. at 141.
²². See id. at 141-42.
²⁴. The litigation surrounding Bendectin is an example of this broader tightening, as is the recognition by reform proponents like Huber and Foster. See PHANTOM RISK: SCIENTIFIC INFERENCE AND THE LAW 39-40 (Kenneth R. Foster et al. eds., 1993).
²⁶. See id. at 146-47. Interestingly, the Joiner decision tempered the extreme confidence Blackmun and the Daubert majority had placed in “the scientific method.” Blackmun had suggested that method was so important that judges could focus on methodology rather than experts’ conclusions. See Daubert v. Merrell Dow Pharm. Inc., 509 U.S. 579, 590 (1993).
Court explained that there was to be no "fresh" or "hard look" at the exclusion of plaintiff's expert evidence unless the trial judge's decision was palpably mistaken.\textsuperscript{28} In \textit{Joiner} the Supreme Court also invoked all of the \textit{Daubert} criteria to exclude the plaintiffs' expert evidence.\textsuperscript{29}

Together, \textit{Daubert} and its progeny have exerted a stultifying effect on tort and product liability suits filed in federal courts and beyond.\textsuperscript{30} Trial judges are encouraged to act as vigilant gatekeepers, and appellate courts are prevented from interfering unless trial judges clearly abuse their wide discretions. In consequence, plaintiffs frequently struggle to have their expert evidence admitted. The changes to practice have been so profound that it has become normal to have pre-trial admissibility hearings for expert evidence (now called \textit{Daubert} hearings) upon which the fate of civil actions often depends. Plaintiffs who are unable to introduce expert evidence are often left without a viable cause of action.

Against this backdrop, we consider \textit{science for litigation}. Here, we can observe how judges (and others) have enlarged the practically demanding \textit{Daubert} criteria with supplementary concerns about the purpose and origins of expert evidence.

\section*{II. \textit{Science for Litigation: The Locus Classicus}}

Notwithstanding the emphasis on "flexibility" in the \textit{Daubert} and \textit{Kumho Tire} judgments, in practice federal judges have scrupulously followed the Supreme Court's \textit{example}. Federal trial judges customarily approach (and apply) the \textit{Daubert} criteria as an inflexible checklist. Every day in courts across America the \textit{Daubert} criteria are used to exclude plaintiffs' expert evidence. For a number of reasons this is unfortunate. First, because the models of science in \textit{Daubert} have little to tell us about modern expertise and its diverse forms. Second, when the criteria are combined, they tend to produce a very demanding admissibility threshold.\textsuperscript{31} We might call the combination and inflexible application of all four criteria hard \textit{Daubert}. The \textit{Joiner} and \textit{Kumho Tire} appeals were important and highly influential demonstrations of hard \textit{Daubert}.

Already formidable, the burden upon plaintiffs in the wake of \textit{Daubert} is amplified where judges supplement \textit{Daubert}'s checklist with additional

\textsuperscript{28} See \textit{Joiner}, 522 U.S. at 142-43.
\textsuperscript{29} See id. at 146-47.
\textsuperscript{31} While each criterion has serious limitations, in combination they are sociologically and philosophically incoherent. When this incoherent assemblage is used as an inflexible checklist, the standard can become incredibly onerous. See Gary Edmond & David Mercer, \textit{What Judges Should Know About Falsificationism}, 5 Expert Evidence 29 (1997); Adina Schwartz, A "Dogma of Empiricism" Revisited: \textit{Daubert} v. Merrell Dow Pharmaceuticals, Inc. and the \textit{Need to Resurrect the Philosophical Insight of Frye v. United States}, 10 Harv. J.L. & Tech. 149 (1997).
factors. Admissibility thresholds become even more exacting where the pervasive exclusionary ethos leads judges to supplement the Daubert criteria with concerns about the origins and orientation of expert evidence. At this point, I want to re-introduce the concept of science for litigation. Incorporating questions about the origins and purpose of evidence into the admissibility matrix operates to supersize the considerable exclusionary potential of hard Daubert.32

A. Supersizing Daubert: Science for Litigation as an Admissibility Supplement

In recent years, one of the more prominent expressions of science for litigation appeared in Judge Kozinski's response to the Supreme Court's remand of the Daubert case to the Ninth Circuit Court of Appeals.33 This judgment provides an instructive example of judicial anxiety about science for litigation, confidence in the value of independence and the redemptive potential of the scientific method, peer review and publication.

Writing for the court, Kozinski prefaced his admissibility jurisprudence with the following remarks, intended to convey some of the difficulties confronting federal judges:

Our responsibility, then, unless we badly misread the Supreme Court's [Daubert] opinion, is to resolve disputes among respected, well-credentialed scientists about matters squarely within their expertise, in areas where there is no scientific consensus as to what is and what is not "good science," and occasionally to reject such expert testimony because it was not "derived by the scientific method." Mindful of our position in the federal judiciary, we take a deep breath and proceed with this heady task.34

In the remainder of the judgment it becomes clear that rather than preoccupy himself with the detail of "the scientific method," Kozinski was intent on supplementing the Supreme Court's opinion with more funda-


The term supersized is a marketing technique that was developed in the mid-90s by McDonalds. Employees would ask 'Would you like that supersized?' or 'Would you like to supersize that?' after taking an order. . . . The idea behind this promotion is that for an extra 49 cents a customer could dramatically increase the size of his or her meal. This promotion was such a success that other fast food chains, such as Burger King and Wendy’s, started their own versions of this marketing technique. . . . Immediately after the beginning of its use, popular culture adopted the term in a positive light. To supersize meant to make something better by making it bigger . . . . The term is no longer in use for its original purpose, due to negative connotations with obesity.

Id. (emphasis omitted).

33. See Daubert v. Merrell Dow Pharm., Inc., 43 F.3d 1311 (9th Cir. 1995).

34. Id. at 1316.
mental tools capable of identifying “good science” and resolving expert disagreement.\textsuperscript{35}

The following extract captures Kozinski’s thinking about law and science. As it suggests, the origins or purpose of expert testimony will ordinarily be a “very significant fact” for consideration in admissibility decision-making.

\[5\] One very significant fact to be considered is whether the experts are proposing to testify about matters growing naturally and directly out of research they have conducted independent of the litigation, or whether they have developed their opinions expressly for purposes of testifying. That an expert testifies for money does not necessarily cast doubt on the reliability of his testimony, as few experts appear in court merely as an eleemosynary gesture. But in determining whether proposed expert testimony amounts to good science, we may not ignore the fact that a scientist’s normal workplace is the lab or the field, not the courtroom or the lawyer’s office. [FN 5]

[FN5] There are, of course, exceptions. Fingerprint analysis, voice recognition, DNA fingerprinting and a variety of other scientific endeavors closely tied to law enforcement may indeed have the courtroom as a principal theatre of operations. See, e.g., United States v. Chischilly, 30 F.3d 1144, 1153 (9th Cir. 1994) (admitting expert testimony concerning a DNA match as proof the defendant committed sexual abuse and murder). As to such disciplines, the fact that the expert has developed an expertise principally for purposes of litigation will obviously not be a substantial consideration.\textsuperscript{36}

\[6\] That an expert testifies based on research he has conducted independent of the litigation provides important, objective proof that the research comports with the dictates of good science. See Peter W. Huber, Galileo’s Revenge: Junk Science in the Courtroom 206-09 (1991) (describing how the prevalent practice of expert-shopping leads to bad science). For one thing, experts whose findings flow from existing research are less likely to have been biased toward a particular conclusion by the promise of remuneration; when an expert prepares reports and findings before being hired as a witness, that record will limit the degree to which he can tailor his testimony to serve a party’s interests. Then, too, independent research carries its own indicia of reliability, as it is conducted, so

\textsuperscript{35} See id. at 1316-22. No judges have discussed the detail of Popper and Hempel’s philosophy or the extensive criticism flowing from their efforts. Legal discourse on “the scientific method” tends to be based on lay versions of testing. The actual complexity of testing, particularly what counts as a decisive test, is frequently lost. See Harry Collins, Public Experiments and Displays of Virtuosity: The Core-Set Revisited, 18 Soc. Stud. Sci. 725 (1988).

\textsuperscript{36} Id. at n.5.
to speak, in the usual course of business and must normally satisfy a variety of standards to attract funding and institutional support. Finally, there is usually a limited number of scientists actively conducting research on the very subject that is germane to a particular case, which provides a natural constraint on parties' ability to shop for experts who will come to the desired conclusion. That the testimony proffered by an expert is based directly on legitimate, preexisting research unrelated to the litigation provides the most persuasive basis for concluding that the opinions he expresses were "derived by the scientific method."

. . .

[7] If the proffered expert testimony is not based on independent research, the party proffering it must come forward with other objective, verifiable evidence that the testimony is based on "scientifically valid principles." One means of showing this is by proof that the research and analysis supporting the proffered conclusions have been subjected to normal scientific scrutiny through peer review and publication. [FN 6] Huber, Galileo's Revenge at 209 (suggesting that "[t]he ultimate test of [a scientific expert's] integrity is her readiness to publish and be damned").

[FN 6] We refer, of course, to publication in a generally-recognized scientific journal that conditions publication on a bona fide process of peer review. See Daubert, —at U.S.—, 113 S.Ct at 2797 ("The fact of publication (or lack thereof) in a peer-reviewed journal thus will be. . . relevant. . .") (emphasis added). See generally The Journal's Peer-Review Process, 321 New Eng. J.Med. 837 (1989).

Peek review and publication do not, of course, guarantee that the conclusions reached are correct; much published scientific research is greeted with intense skepticism and is not borne out by further research. But the test under Daubert is not the correctness of the expert's conclusions but the soundness of his methodology. See n.11 infra. That the research is accepted for publication in a reputable scientific journal after being subjected to the usual rigors of peer review is a significant indication that it is taken seriously by other scientists, i.e., that it meets at least the minimal criteria of good science. Daubert,509 U.S. at 593 ("[S]crutiny of the scientific community is a component of 'good science.'"). If nothing else, peer review and publication "increase the likelihood that substantive flaws in methodology will be detected." Daubert, —U.S. at —, 113 S.Ct.2797. [FN7]

[FN7] For instance, peer review might well have brought to light the more glaring arithmetical errors in the testimony presented
edmond: supersizing daubert science for litigation and its implications


Given the formidable task confronting busy federal judges, it might be considered unremarkable that readily identifiable indicia—such as the purpose of a study or report—seem to hold considerable interest for Kozinski and the federal judiciary. 38 After all, concern with the origins and purpose of knowledge shifts the focus from technical details, competence and assessments of "falsifiability" to bright line questions about publication, motivations and the obvious pecuniary interests held by experts.

In order to assist judges with admissibility determinations, in the wake of Daubert, Kozinski introduced three heuristics:

(i) scientific evidence developed independent of litigation is inherently reliable;

(ii) scientific evidence developed for litigation is inherently suspect; and

(iii) "peer review and publication" (and to some extent "the scientific method" or "the policy statement of an association") have the potential to rehabilitate (or guarantee) purposive or directed research. 39

For Kozinski, research conducted "on the very subject that is germane to a particular case" before litigation commences is proof that the evi-

37. Id. at n.7. There are literally hundreds of federal court cases with similar expressions and commitments.

38. A quick search of United States Courts of Appeals decisions disclosed the following verbatim reproductions of Kozinski's wording: Fuesting v. Zimmer, Inc., 421 F.3d 528, 534 (7th Cir. 2005); Clausen v. M/V New Carissa, 339 F.3d 1049, 1056 (9th Cir. 2003); Avery Dennison Corp. v. Four Pillars Enter. Co., 45 Fed. Appx. 479, 483-84 (6th Cir. 2002); Metabolife Int'l, Inc. v. Wornick, 264 F.3d 832, 841 (9th Cir. 2001); In re Unisys Sav. Plan Litig., 173 F.3d 145, 166 (3d Cir. 1999); Smelser v. Norfolk S. Ry. Co., 105 F.3d 299, 303 (6th Cir. 1997). The most recent of these cases, Fuesting, draws attention to the fact that Kozinski's concerns have been incorporated into the Advisory Committee's Notes on Rule 702. In addition to the four Daubert criteria, "the 2000 Advisory Committee's Notes to Rule 702 suggest other benchmarks for gauging expert reliability, including . . . (6) whether the testimony relates to 'matters growing naturally and directly out of research they have conducted independent of the litigation,' or developed 'expressly for the purposes of testifying' . . . ." Fuesting, 421 F.3d at 534.

39. See Daubert, 43 F.3d at 1317-19. Elsewhere in his judgment Kozinski explains:

For such a showing to be sufficient, the experts must explain precisely how they went about reaching their conclusions and point to some objective source—a learned treatise, the policy statement of a professional association, a published article in a reputable scientific journal or the like—to show that they have followed the scientific method, as it is practiced by (at least) a recognized minority of scientists in their field.

Id. at 1319.
dence is "independent" and implicitly reliable.\textsuperscript{40} But he goes even further. Kozinski characterizes testimony based on "preexisting research unrelated to the litigation" as "legitimate" and "the most persuasive basis for concluding that the opinions" were "derived by the scientific method" and are taken seriously by other scientists.\textsuperscript{41} Emphasis is placed on fact that "a scientist's normal workplace is the lab or the field" and "not the courtroom or the lawyer's office."\textsuperscript{42} It would seem that "good science"—to which we might add genuine science and sound science—is outside the influence of, or insensitive to, law and litigation (and regulation). Kozinski is sufficiently concerned with the distorting effects of litigation to suggest that scientific research which takes place beyond the influence of the courtroom is likely to be methodologically pure.\textsuperscript{43} Such an approach, as we shall see, blinds Kozinski—and judges embracing hard Daubert and science for litigation—to the socially-embedded and commercially-oriented nature of scientific practice.

Where the expert evidence is not litigation-free—that is, not independent of litigation—Kozinski places a burden on the proffering party to demonstrate that it is nevertheless "objective, verifiable evidence ... based on 'scientifically valid principles.'"\textsuperscript{44} One way, and indeed the only means suggested, of overcoming the evidentiary stigma of science for litigation is through peer review and publication. For Kozinski, independence is assumed where knowledge claims have been exposed to "normal scientific scrutiny through peer review and publication."\textsuperscript{45} "Establishing that an expert's proffered testimony grows out of pre-litigation research or that the expert's research has been subjected to peer review are the two principal ways the proponent of expert testimony can show that the evidence satisfies the first prong of Rule 702."\textsuperscript{46}

Here, the second of the Daubert criteria \textsuperscript{2} has the potential to provisionally demonstrate independence and reliability. Kozinski, like Blackmun before him, does not attempt to explain how peer review and publication actually generate these reliability effects.\textsuperscript{47} Both judgments concede that peer review and publication "do not, of course, guarantee that the conclusions reached are correct."\textsuperscript{48} In practice, however, these qualifications tend to be neglected or ignored. Where research is accepted for publica-

\textsuperscript{40} See id. at 1317.
\textsuperscript{41} Id. at 1317-18.
\textsuperscript{42} Id. at 1317.
\textsuperscript{43} These claims, as others have noted, are not strictly logical. See Sheila Jasanoff, Law's Knowledge: Science for Justice in Legal Settings, 95 AM. J. PUB. HEALTH S49 (2005). In effect, Kozinski presents a kind of tautology. Science depends on peer review and publishing, rather than the method itself, to prove the correct application of the method.
\textsuperscript{44} Daubert, 43 F.3d at 1318.
\textsuperscript{45} Id.
\textsuperscript{46} Id. Kozinski is referring to "assisting the trier of fact" in Rule 702.
\textsuperscript{47} "Peer review and publication" are, in effect, black boxed.
\textsuperscript{48} Daubert, 43 F.3d at 1318 (emphasis added).
tion in a reputable scientific journal it seems to satisfy "at least the minimal criteria of good science" and usually more.

Unfortunately, many of these rather glib assumptions and commitments have little relevance to modern scientific practice. Some, like the idea of independence and the perceived need to maintain a strict separation between Science and Law, only obtain evidentiary traction to the extent that judges are unwilling to critically explore the organization of the modern sciences, particularly the relations between manufacturers—such as those producing pharmaceuticals, therapeutic products, foodstuffs, weapons, fuels and chemicals—research funding and the scientific and medical literatures. Kozinski places great weight on popular, if simplistic and frequently tendentious, impressions of scientific practice, peer review and the scientific literature. At this juncture we turn to consider more empirically informed descriptions of the sciences before moving to examine some of the practical limitations associated with using science for litigation as an admissibility tool.

III. AGAINST SCIENCE FOR LITIGATION AND HARD DAUBERT: EMPIRICAL STUDIES OF THE SCIENCES

A. Modern Approaches to the Sciences

The admissibility potential of science for litigation is predicated on the prevalent belief that science is a process which ordinarily manages to separate knowledge (or ways of knowing) from society (i.e. the social and the subjective). Unlike genuine science or independent science, science for litigation is understood to fail in this important regard. It is this failure which renders science for litigation subjective and potentially unreliable. Notwithstanding the popularity of these ideas, research from the sociology, philosophy and history of science and technology overwhelmingly suggests that many pervasive ideas about science—and many of the kinds of images on which Daubert and much expert evidence jurisprudence is predicated—are actually fanciful. Often they bear little or no resemblance to modern scientific practice.49 It is useful, in consequence, to consider some of the ways expert evidence jurisprudence is displaced from contemporary thinking about the sciences and technology.50

Let's begin with scientific methodology.

B. A Universal Scientific Method?

In Daubert, Justice Blackmun emphasized the need for "scientific knowledge" derived using "the scientific method." Recall,

49. Some introductory texts include David J. Hess, Science Studies: An Advanced Introduction (1997); Steven Yearley, Making Sense of Science: Understanding the Social Study of Science (2005); and Handbook of Science and Technology Studies (Sheila Jasanoff et al. eds., 1995).

Similarly, on remand, Judge Kozinski linked the purpose and chronology of research to this criterion: "That the testimony proffered by an expert is based directly on legitimate, preexisting research unrelated to the litigation provides the most persuasive basis for concluding that the opinions he expresses were 'derived by the scientific method.'" 52 Recourse to the scientific method—exemplified in these extracts from senior appellate judges—is interesting, and perhaps revealing, when contrasted with what those who actually study scientists have to say about Popper and the scientific method more generally.

Most, and perhaps all, contemporary historians and sociologists of science would dismiss the idea of a historically stable, prescriptive and efficacious scientific method doctrine—that is, a universal scientific method—as simply implausible. 53 Virtually all modern philosophers seem to endorse the following appraisals:

In a nutshell the problem is that all characterizations offered of scientific method at the level of generalization and abstraction favoured by philosophers of science fail to be an account of anything specifically scientific. Hence such stories cannot account for what is special about science. 54

'There is no logic of discovery,' . . . there is no logic of testing, either; all the formal algorithms proposed for testing, by Carnap, by Popper, by Chomsky, etc., are, to speak impolitely, ridiculous: if you don't believe this, program a computer to employ one of these algorithms and see how well it does at testing theories! 55

52. Daubert, 43 F.3d at 1317.
To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise.\textsuperscript{56}

There is not a single rule that remains valid under all circumstances and not a single agency to which appeal can always be made.\textsuperscript{57}

I think it has to be said that falsificationism, and Popper’s account of the ‘logic’ of scientific method, have to be accounted as failures.\textsuperscript{58}

Popper is famous for his strikingly simple view of science. Unfortunately, despite its undoubted charms, the view is much too simple to be true.\textsuperscript{59}

Few qualified observers believe that Popper has succeeded in solving the problem of induction or in presenting a non-inductive account of science, and many find his model of scientific theorizing over-simplified.\textsuperscript{60}

What this means is, as the quote from Putnam suggests, there is no single method or algorithm with which judges can identify authentic or reliable scientific knowledge. Actual observation of scientists and study of their publications reveal that formal education and socialization into a research tradition are more important to scientific practice than knowledge of philosophical formulations or formalized rules. It is no coincidence that method courses are more common in disciplines with scientific pretensions, like economics, psychology and sociology, than in the established sciences of mathematics, physics, chemistry and biology.\textsuperscript{61}

It is important to appreciate that the foregoing critiques are not limited to the philosophies of Karl Popper (1902-1993) and Carl Hempel (1905-1997)—whose fundamentally irreconcilable positions were, tellingly, both endorsed in the Supreme Court’s Daubert judgment [1]—but apply to all philosophical and sociological attempts to identify a universal method (or methods) underpinning all scientific practice. These days, there are few scholarly attempts to identify a peculiar scientific rationality or single principle capable of accommodating the disparate activities assembled under the rubric of the modern sciences. Rather, historians, soci-

\textsuperscript{56} Thomas S. Kuhn, Logic of Discovery or Psychology of Research?, in Criticism and the Growth of Knowledge 1, 10 (Imre Lakatos & Alan Musgrave eds., 1970).

\textsuperscript{57} Paul Feyerabend, Against Method 164 (rev. ed., 1988).


\textsuperscript{60} Anthony O’Hear, Introduction to Karl Popper: Philosophy and Problems 1, 11 (Anthony O’Hear ed., 1995).

ologists and anthropologists have become more interested in the rhetorical, institutional and social functions of method discourses (i.e. talk about scientific method).

Writing in the 1980s, two historians of science summarized these developments:

Over the last two decades . . . the traditional belief in the existence of a single, transferable, efficacious scientific method has been challenged. This has opened a range of questions about the actual roles played by methodological doctrines in the development of science and in the social dynamics of scientific communities.62

Historical and sociological studies suggest that abstract formulations of a universal method do not play an important role in scientific research. Instead, method discourses fulfill an important and conspicuous role in science education, science communication, policy interventions and the resolution of scientific controversy.

After the fact, scientists often account for their activities and discoveries in terms of rigorous adherence to the scientific method. Retrospectively, scientists tend to characterize their activities and inquiries as peculiarly rational. Although, as sociologists, historians and anthropologists have repeatedly demonstrated, these retrospective accounting practices often bear the most tenuous relations with actual practice (i.e. what actually happened). They frequently fail to explain that what counts as methodological propriety is part of the scientific consensus which emerges at the end of a controversy rather than the means of actually resolving controversy or prescriptively determining the contours of nature.63

In consequence, it would seem that many judges have been discussing scientific method discourses as if they prescriptively guided modern scientific practice.64 In this way judges have been appealing to the rhetorical (and marketing) dimensions of the sciences rather than confronting the technical aspects of controversy, uncertainty and disagreement or the more difficult process of choosing amongst assemblages of methods (in the plural), equipment, assumptions, levels of competence, interpretations and so on.65 The complications, however, are not restricted to method.


C. The Norms (and Counter-Norms) of Science

Historical and empirical studies have not been able to identify a set of institutional commitments or professional norms consistently adhered to by scientists within a field or sub-discipline, let alone norms embraced by all scientists and experts. The sociologist Robert Merton (1910-2003) provided an early and influential elaboration of scientific norms and their functions. Merton’s account, itself a response to the rise of totalitarianism and the suppression of scientific research in Europe, explained the importance of characteristics like communalism, openness, disinterestedness and skepticism to the pursuit of genuine scientific activity. Not only does this overstate Merton’s actual position, but more recent sociological research suggests that norms, such as disinterestedness and skepticism, are susceptible to manipulation and strategic deployment. The following examples will help to illustrate practical limitations with the legal appropriation of the kinds of norms described by Merton in the 1930s and 1940s.

On the basis of a study of NASA moon scientists, Ian Mitroff explained how experienced scientists routinely derogate from Mertonian-style norms. From his observations and interviews, Mitroff concluded that highly regarded scientists were often passionately committed to pet theories, sometimes in the face of very damaging evidence. In practice, the NASA scientists tended to be far more skeptical of their rivals’ theories and data than their own. And, the scientists were able to provide reasons for preferring their own positions. Some of these reasons included concerns about relative levels of competence; underlying assumptions; (in)consistency with theory, other results and interpretations; reliability of equipment, and so forth. Inconsistent evidence was almost never interpreted as some kind of definitive refutation or falsification. In addition, Mitroff’s moon scientists were not as cooperative or forthcoming with results and techniques as those committed to Mertonian norms might have anticipated. Once again the scientists provided (seemingly credible) explanations for their behavior. Reasons for withholding data and materials included the need to establish priority claims; previous (or anticipated) failure to reciprocate; protecting the work of graduate students; and waiting for confirmatory studies or actual publication.


68. See id.
Derogations from idealized norms were so pervasive that Mitroff developed the idea of the \textit{counter-norm}.\footnote{See Ian Mitroff, \textit{Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study in the Ambivalence of Scientists}, 39 \textit{Am. Soc. Rev.} 579 (1974).} Mitroff found that scientific practices were explained using a variety of discursive resources. Where activities seemed to contradict popular normative expectations, scientists appealed to a range of exceptions and qualifications which helped to excuse or legitimate what might otherwise be understood as deviant behavior. Mitroff characterized these principled or reasoned derogations as "counter-norms."

Interestingly, and perhaps against expectations, derogation from scientific norms and the invocation of counter-norms did not simply correlate with a scientist's standing or professional credibility. Similarly, knowledge derived through secret, non-cooperative or interested activities was not necessarily seen as pathological or unreliable. Instead, Mitroff's work, like subsequent sociological and anthropological studies of scientists, suggests a much richer realm of scientific practice.\footnote{See Karin D. Knorr-Cetina, \textit{The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science} (1981); Bruno Latour & Steve Woolgar, \textit{Laboratory Life: The Social Construction of Scientific Facts} (1979); Michael Lynch, \textit{Art and Artifact in Laboratory Science} (1985); Sharon Traweek, \textit{Beamtimes and Lifetimes: The World of High Energy Physicists} (1992).} Members of specialist communities are often familiar with the personality and temperament of fellow scientists, as well as their previous work, their abilities, their commitment to ideas and theories and earlier normative derogations. Indeed, these are often combined in a complex and morally-inflected evaluation of an expert's capabilities, performance and findings. As with method discourses, idealized norms and allegations of contravention have a tendency to arise during periods of controversy.

These, however, are not the only limitations with recourse to idealized norms. Others, like the sociologist Michael Mulkay, have explained how norms themselves can create interpretative complexity.\footnote{See M. Mulkay, \textit{Interpretation and the Use of Rules: The Case of the Norms of Science}, in \textit{Science and Social Structure: A Festschrift for Robert K. Merton} 111 (Robert King Merton & Thomas F. Gieryn eds., 1980); see also Steve Woolgar, \textit{Science: The Very Idea} (1988). For an example of the complexity of scientific practice, consider S. Epstein, \textit{Impure Science: AIDS Activism and the Politics of Knowledge} (1996).} Vague norms—like openness and skepticism—are unlikely to guide scientific practice or the assessment of knowledge claims especially in contexts where the behavior, motivations and alignments of scientists as well as technical issues are all in dispute. If we momentarily reflect on the idea of skepticism, the significance of under-determination should become clearer. Confronted with unexpected experimental results, should scientists tinker with their equipment and assumptions or simply accept the results even if they are potentially disruptive to widely accepted commitments, background theories and previous findings? Should a scientist be skeptical about the unex-
pected results, the sensitivity and reliability of any experimental apparatus, the theory underlying the equipment and/or the interpretation of the results?72 This simple example raises questions about how a skeptical attitude should be embodied in any given situation. It probably will not surprise lawyers when they are told that norms like skepticism are capable of being interpreted and applied inconsistently. Moreover, as Mitroff's study suggests, norms tend to be mobilized strategically. The fact that behaviors, motivations, alignments and technical issues are not easily disentangled means that assessments of knowledge are always socially-inflected and potentially complex.

Nevertheless, simplistic appeals to norms like disinterestedness, along with purported derogations, tend to be used frequently and detrimentally in the evaluation of expert evidence.73

D. What About Objectivity?

Appeals to objectivity rarely assist in the ultimate resolution of technical controversy.74 Albury, a historian of medicine, helps us to understand why; "[M]atters of disagreement between scientific experts are not typically conflicts between objectivity on one side and bias on the other, but conflicts involving two rival conceptions of objectivity—that is, two different ways of assigning relevance to the available data and of interpreting their meaning."75 Independence—like objectivity, impartiality, neutrality and the norm disinterestedness—is primarily a representational category. Such categories are designed to do work. Their ability to do this (usually depoliticization) work depends upon how scientific performances are represented and categories like "independence" and "objectivity" are cultivated and managed.76 Their significance, in relation to the production and reliability of knowledge, is suggestive but not always determinative.

Whether something will be understood as objective/independent or partisan/biased often depends upon how far an analyst or judge is willing to delve and what is considered (ir)relevant to the production of scientific

72. These difficulties have been described by Collins as the "experimenter's regress." See Collins, Changing Order, supra note 63.


knowledge. There will always be interests, alignments, motivations, assumptions, funding relations, theoretical commitments and other aspects of knowledge-making which might be used to subvert assertions of neutrality. Juxtaposing socially-embedded practice against abstract versions of method and idealized norms can make just about any knowledge or practice appear social/subjective. We can always invoke (or elide) funding, ambition, competence, methodological and normative compliance (or derogation), assumptions, institutional affiliations, animosities and so forth to help mediate objectivity and its reliability effects. It is more difficult to ascertain how the conditions shaping the production of knowledge might be used to make useful reliability assessments.

1. **Objectivity, Independence, Science and Commerce**

Over time, and especially since the Second World War, images of scientific objectivity and independence have been conspicuously compromised by the changing political economies shaping modern scientific practice, its institutional manifestations and spatial locations. Today, most scientific research is financed by for-profit corporations and most scientists and engineers work in the private sector.

One of the difficulties with recourse to scientific objectivity and independence is that the practices, economic potential (especially military and medical), institutional arrangements and social standing of the different sciences actually change. In recent decades many of the sciences have been transformed and traditional disciplinary boundaries weakened. Perhaps the most conspicuous example—related to tort, product liability and intellectual property law through pharmaceuticals and genomics—concerns the commercialization of biotechnology. Biologists, geneticists and chemists now have historically intimate relationships with commercial entities which not only fund much of their research but have changed the way they undertake their science and the way they disseminate knowledge and experience. Moreover, because of these changes, many scientists are now engaged, not only in research but also in a range of traditionally non-scientific activities. These might include entrepreneurialism (e.g. fund raising and venture capitalism), management, marketing, consulting, lobbying and regulation. Many scientists involved in the development and

77. Ultimately, context may explain much, perhaps even everything about the production and content of knowledge, but not in ways that are always readily accessible. Whether something is considered independent or objective is often the subject of controversy or disagreement. Whether, and how, that matters can also be controversial.


testing of pharmaceuticals, for example, are also active in patenting, are consultants to corporations or directors and participate on public advisory and regulatory bodies. 80

While scientists have never been able to “mirror nature” in some a-social or parthenogenetic capacity, historically many were probably more independent of the individuals and agencies which funded their research as well as legal and regulatory issues and commercial considerations. Research was undertaken and often funded on a smaller scale and many of the agencies and institutions funding research were less concerned with direct outcomes, intellectual property regimes or transforming discoveries into technologies, let alone profitable applications. 81 In recent years, funding relationships, like commercial ties and orientations, have become more direct and more directed, if not always more transparent. 82 Even those fields and researchers traditionally more insulated from practical applications and commercialization have been enrolled. Modern scientists, it would seem, are interested in financial rewards as well as honors, prestige and research funding. Consequently, concepts like independence and objectivity have become more complicated, if not less (rhetorically) valuable, in policy advice, regulation and legal decision-making. These issues might not be considered insignificant in an essay on science for litigation.

Once we begin to develop more elaborate conceptions of scientific practice, and the socio-economic circumstances in which the modern sciences are firmly and inextricably embedded, then we may need to re-consider what we mean by objectivity. Here, we are not merely concerned with the research orientation or the conduct and reporting of research, but also the funding of research, the (indirect) role of scientists and sponsors in regulatory processes, lobbying for legal and regulatory reform and the promotion of strategic images of public science. 83 Some examples might prove illuminating.

80. Mercer has described these developments as “the vertical integration of expertise.” David Mercer, Hyper-experts and the Vertical Integration of Expertise in EMF/RF Litigation, in Expertise in Regulation and Law, supra note 50, at 85, 86, 93.


83. Public science refers to general expressions about science and its abilities designed to enhance the influence and prestige of the organized sciences, and particularly the standing and prestige of eminent scientists. See Frank Miller Turner, Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England (1974); Frank Miller Turner, Contesting Cultural Authority: Essays in Victorian Intellectual Life (1993).
The participation of research scientists—with financial connections to for-profit corporations—on government advisory committees provides some indication of the limits of objectivity and the close relations between research, regulation and capital. A recent review of Food and Drug Administration (FDA) expert advisory committees found: “At least one advisory committee member had a financial stake in the topic under review at 146 of the 159 meetings (ninety-two percent). At eighty-eight of the meetings (fifty percent), at least half of the advisory committee members had financial interests in the product being evaluated.”

Based on the review of about 1,600 expert appearances, slightly more than half received a waiver which allowed them to participate notwithstanding a disclosed conflict of financial interest. The relevant rules state that a federal employee cannot participate in an official capacity in a matter where that employee “has a financial interest if the particular matter will have a direct and predictable effect on that interest.”

A second rule dictates the circumstances in which the first rule can be circumvented, and this explains the hundreds of waivers. Waivers can be issued where an agency head determines that the risk from the conflict is remote or insubstantial or, notwithstanding the potential conflict, the need for the individual’s expertise outweighs any risk. The waiver system does not engender confidence.

But consultancies, research sponsorship and representation on government advisory committees are not the only ways for-profit corporations limit regulatory interference and manipulate perceptions of objectivity. Large international pharmaceutical manufacturers (known as Pharma), for example, are actively engaged in law and regulatory reform and, inseparably, public relations. Through the American Pharmaceutical Manufacturers Association (PMA), the pharmaceutical industry has submitted


85. Id.

86. See Sheldon Krimsky, The Ethical and Legal Foundations of Scientific Conflict of Interest, in Law and Ethics in Biomedical Research 65 (Trudo Lemmens & Duff R. Waring eds., 2006).

87. Another study, of financial ties between DSM-IV (Diagnostic and Statistical Manual of Mental Disorders) panel members and the pharmaceutical industry, revealed that more than half of the panels had at least one member with one financial association with the manufacturers of psychiatric drugs, and that all of the members of the panels on “Mood Disorders” and “Schizophrenia and Other Psychotic Disorders” maintained financial ties with pharmaceutical companies. Financial ties included research funding, consultancies, sponsorships, equity interests, holding patents or copyright, travel funding, appearing as an expert witness and speaking commitments. According to Cosgrove et al., the anti-psychotic drug market has global sales of $8.5 billion and anticipated sales of $18.2 billion by 2007. It is probably not surprising that the authors suggest that improving “awareness about the real or perceived COI [conflicts of interest] of panel members is an important public health issue.” Lisa Cosgrove et al., Financial Ties Between DSM-IV Panel Members and the Pharmaceutical Industry, 75 Psychotherapy & Psychosomatics 154, 159 (2006).
amicus curiae briefs to the Supreme Court and other senior appellate courts. Like many other industry briefs, the PMA brief in Daubert decried the social and economic costs of gratuitous litigation caused by judges who are unwilling to exclude plaintiffs' "junk science" and science for litigation from courtrooms.

The availability of safe and effective drugs depends not only on the successful functioning of the regulatory scheme administered by the FDA but also on the proper operation of the tort system. Whether a given drug has a particular effect is generally a scientific question, and while "science" is not a fixed body of knowledge, there are principles and methodologies that are generally accepted by scientific communities; there are also sincerely held individual opinions, based on idiosyncratic views or poorly conducted studies, to which other scientists themselves accord no weight. . . . If the tort system were opened up to allow lay juries to hear and rely on scientific opinions not grounded in generally accepted principles and methodologies—opinions that the relevant scientific community itself would not rely on—the result would be to discourage both the marketing of, and research toward, safe and effective medicines important for the public's health.88

The PMA brief, like other pro-industry submissions, also advanced models of science and admissibility standards designed to shift attention from the way corporations sponsor and shape the scientific record.

In addition, members of the PMA, along with other large, for-profit companies, sponsor think tanks and engage public relations firms with the intention of popularizing alleged problems with the legal and regulatory systems. Contributing to popular legal consciousness—Stella Liebeck's incident with McDonald's coffee is exemplary in this regard—they have sought to harden public attitudes to the allegedly parlous condition of the civil justice system and in so doing have made formally independent, but socially vulnerable, judges more defensive.89 Much of this public relations work, like the claims in the amicus briefs, sits awkwardly against the empirical findings of non-aligned legal scholars and sociologists.90 Nevertheless, through political lobbying and campaign funding, Pharma (and other industries) have not only reformed admissibility standards but limited civil liability and encouraged moves to cap damage awards. At the same time,


90. See CARL T. BOGUS, WHY LAWSUITS ARE GOOD FOR AMERICA 137 (2001); DIXON & GILL, supra note 30; HALTOM & MCCANN, supra note 89; THOMAS H. KOENIG & MICHAEL L. RUSTAD, IN DEFENSE OF TORT LAW (2001).
for-profit corporations have been campaigning for wider de-regulation and insisting that any scientific evidence used in the regulation of their products and environmental impacts should be based on Daubert-inspired versions of *sound science*. Rather than adopt precautionary orientations—more relevant to modern scientific practice and its limitations—increasingly, corporations are insisting that regulatory interventions should be based only on evidence that would satisfy *hard Daubert*.91

Large, well-resourced corporate players—that is, prototypical civil defendants, like large Pharma, chemical companies and automobile manufacturers—are actively engaged at every stage from sponsoring researching and privatizing knowledge, to patenting organisms and technologies, to developing and marketing products, to lobbying for the reform of the legal and regulatory regimes in which their products will be used, insinuating consultants and sponsored research onto government committees as well as helping to shape public and judicial attitudes to litigation and liability through advertising and the media.

With the occasional exception, judicial practice tends to ignore the many levels and diverse range of activities in which commercial organizations and increasingly retained or aligned scientists participate. Judges (and many lawyers) tend to construe *relevance, reliability and objectivity* in ways that often appear inexplicably narrow and insensitive to context. Frequently, they ignore or disaggregate a company's history and extra-litigation activities—its lobbying efforts, commercial orientation, the institutional commitment of its researchers and consulting scientists or the strategic orientation, design and dissemination of its safety and efficacy research and public definitions of science—from the way they challenge plaintiffs' expert evidence or actually use evidence when they go to trial. Rather than consider a defendant's overall record or the socio-institutional framework of which publication is often just one strategic part, many courts restrict their focus to published or completed research. Extracted from its role in the goals of a vertically integrated commercial enterprise—acutely sensitive to legal and regulatory factors—such restrictive approaches may elide a deeper set of relations and orientations which structure research, reporting and external mobilization, as well as the ability to identify fraud, breach of fiduciary duties and corporate malfeasance. Judges who (un)wittingly blind themselves to these activities, or characterize them as non-scientific or irrelevant, misunderstand modern scientific

enterprises and their relations with organized capital and modern nation states.

In the context of a discussion on science for litigation, the meaning and significance of terms like independence, impartiality, neutrality and objectivity require re-conceptualization. There is conspicuous need to take account of contemporary social and professional realities. Judges cannot afford to disregard the fact that modern knowledge is primarily developed for commercial purposes in ways that are acutely sensitive to proprietary issues and potential risks. It is entirely unpersuasive to think that massive international corporations are insensitive to the legal and regulatory implications of their products or their efficacy and safety research. It is inappropriate to persist with simplistic ideas like objectivity and independence, especially sociologically "thin" versions which focus on whether an expert is appearing for a plaintiff (or defendant), and ignore broader structural influences and institutional orientations which actually shape many areas of contemporary scientific research, regulation and law.92

E. Peer Review and Publication

Judges routinely characterize peer review and publication as means of warranting the authenticity, independence and reliability of scientific knowledge. For judges, peer review and publication—but especially the fact of publication—provide a bright admissibility threshold which is relatively easy to administer. Writing for the majority, Justice Blackmun included "peer review and publication" among the Daubert criteria.

[2] Publication (which is but one element of peer review) is not a sine qua non of admissibility; it does not necessarily correlate with reliability, see S. Jasanoff, The Fifth Branch: Science Advisors as Policymakers 61-76 (1990), and in some instances well-grounded but innovative theories will not have been published, see Horrobin, The Philosophical Basis of Peer Review and the Suppression of Innovation, 263 JAMA 1438 (1990). Some propositions, moreover, are too particular, too new, or of too limited interest to be published. But submission to the scrutiny of the scientific community is a component of "good science," in part because it increases the likelihood that substantive flaws in methodology will be detected. See J. Ziman, Reliable Knowledge: An Exploration of the Grounds for Belief in Science 130-33 (1978); Relman & Angell, How Good Is Peer Review?, 321 New Eng. J. Med. 827 (1989). The fact of publication (or lack thereof) in a peer-reviewed journal thus will be a relevant, though not dispositive, consideration in assessing the scien-

---

92. The idea of "thin" is drawn from the influential work of the anthropologist Clifford Geertz in The Interpretation of Cultures (1973).
tific validity of a particular technique or methodology on which an opinion is premised.93

These sentiments were endorsed and developed in Kozinski’s Daubert decision for the Ninth Circuit:

[7] If the proffered expert testimony is not based on independent research, the party proffering it must come forward with other objective, verifiable evidence that the testimony is based on “scientifically valid principles.” One means of showing this is by proof that the research and analysis supporting the proffered conclusions have been subjected to normal scientific scrutiny through peer review and publication.

*Peer review and publication do not, of course, guarantee that the conclusions reached are correct; much published scientific research is greeted with intense skepticism and is not borne out by further research. But the test under Daubert is not the correctness of the expert’s conclusions but the soundness of his methodology. . . . That the research is accepted for publication in a reputable scientific journal after being subjected to the usual rigors of peer review is a significant indication that it is taken seriously by other scientists, i.e., that it meets at least the minimal criteria of good science.*94

Here, peer review and publication are presented as a means of rehabilitating what might otherwise appear to be aligned—that is, implicitly dependent—scientific testimony. While not infallible, peer review and publication are characterized as a means of guaranteeing “the minimal criteria of good science.” This valorization is consistent with judicial practice. Judges rarely reflect on the italicized qualifications. Peer review and publication tend to operate as a simple threshold test where reasons for non-publication and any practical limitations tend to be downplayed or ignored.95 These commitments, however, begin to appear problematic when we compare them with what sociologists have to say about peer review and publication, and seem decidedly fragile when we consider how the editors of leading scientific and medical journals have been responding to commercialization and the strategic interventions of large Pharma over the past two decades.

On closer examination, peer review and publication do not always operate in the rigorously skeptical manner conventionally attributed to them. Scientific journals, especially the most prestigious, have many,

sometimes competing, obligations. They have interests in rapid dissemination, maintaining broad appeal, providing the most accurate information available and remaining solvent through subscriptions and sometimes advertising revenues.\textsuperscript{96} Increasingly, they have obligations not just to scientists, engineers and physicians but to security investors, venture capitalists and retirees. Nevertheless, submissions are not always thoroughly reviewed, not always written by named authors and only very rarely replicated. Perhaps it will not be surprising to find that reviewers tend to be more sympathetic to those whose views are consonant with or reinforce their own.\textsuperscript{97} Remember Mitroff's NASA scientists and the norm of skepticism. Moreover, in recent decades, commercial competition, private funding and concerns about liability have provided new incentives for not undertaking and not publishing research and imposed new contractual restrictions on the dissemination of technologies, data and results.

Superficially, peer review and publication might appear to provide a presumptive indication of reliability. Actual study, however, reveals a more complex state of affairs. In a longitudinal study of physicists, the sociologist Harry Collins found that the published literature held a range of inconsistent meanings to different audiences.\textsuperscript{98} Among small groups of specialists, who were extremely conversant with ongoing controversies in a particular discipline or sub-field, even articles which had been published in highly respected, peer-reviewed physics journals were often dismissed or trivialized on the basis of familiarity with the scientists involved and their earlier work; detailed knowledge of their experiments, equipment and interpretation of results; (alternative) theoretical commitments and assumptions; degrees of (in)competence and attention to detail, and so on. By way of comparison, those on the peripheries of the sub-group and further afield—usually unfamiliar with individual scientists (or teams) as well as the contours of the field—were far more likely to uncritically accept published work.

Revealingly, Collins found that the members of specialist physics communities were most likely to respond to competing theoretical approaches and results, especially those already in print, when external funding was at stake. This was particularly conspicuous when those managing the fund--

\textsuperscript{96} Krimsky gives the example of the journal \textit{Transplantation and Dialysis} rejecting a favorably reviewed submission which questioned the benefits of a drug because of concerns expressed by the journal's marketing department. \textit{See} Sheldon Krimsky, \textit{Publication Bias, Data Ownership, and the Funding Effect in Science: Threats to the Integrity of Biomedical Research}, in \textit{Rescuing Science from Politics: Regulation and the Distortion of Scientific Research} 61, 72 (2006) [hereinafter Krimsky, \textit{Publication Bias}].


ing were generalists or had expertise that was considered, by the specialist physicists, to be insufficiently discriminating. The physicists studied by Collins *intervened* because they were worried that a publication—widely considered by specialists as anomalous, mistaken or even disingenuous—might be considered representative or reliable merely because it had been refereed and published in a leading international physics journal. Factors like funding decisions could mobilize interested, but otherwise passive or indifferent, scientists into action and publication. On the basis of this and other sociological research, the meaning of peer review and publication appears to depend on a complex range of technical, institutional, professional and social factors. The study by Collins suggests that it may be dangerous to approach the simple fact of publication without some sensitivity to a range of often tacit and subterranean dynamics.99

Another important issue with peer review and publication is that their meaning and significance change across fields and over time.100 Sometimes peer review refers to the refereeing of papers prior to publication. When papers are reviewed prior to publication, however, the referees are not very consistent in their performances and editors tend to have a range of views on how to respond to comments and criticisms. Sometimes peer review refers to the attention given by the scientific community to papers that have already been published. Publication also has many valencies. Not all scientific publications—and this even extends to articles featured in refereed journals—are refereed prior to publication. Indeed, it is common for conference proceedings to escape refereeing. Many journals feature non-refereed supplements and few journals devote resources to quantitative reviews of the substance of the articles they publish.101 Also, the quality and depth of review varies considerably among publications and referees. Studies suggest that the average reviewer spends just a couple of hours refereeing a journal submission and even less time reviewing grant applications.

Limitations with peer review and publication have become the subject of more direct discussion and action by a range of scientific publishers, particularly in relation to public health studies and research into chemicals, pharmaceuticals and other therapeutic products. Recent developments in the medical and scientific literature make judicial confidence grounds for further disconcert.

---

99. Collins's study also illustrates how different theoretical commitments and the politics of fields can profoundly shape the ability to publish and attract public funding for research.


1. Science for Litigation and Recent Controversies in the Scientific and Medical Literatures

Recent controversies in scientific and medical publishing provide compelling reasons for believing that judicial confidence in peer review and publication is fundamentally misplaced. With their attention narrowly focused on the restorative potential of peer review and publication, federal judges seem to have overlooked the variety of influences and the number of conflicts of interest and non-disclosures in the mainstream medical and scientific literatures. Here, we can expand our introductory sociological assessment in ways bearing more directly on modern product liability and tort litigation.

a. (Undisclosed) Conflicts of Interest

When an investigator has a financial interest in or funding by a company with activities related to his or her research, the research is lower in quality, more likely to favor the sponsor’s product, less likely to be published, and more likely to have delayed publication.


In the discussion of “Objectivity” we gained some sense of how many scientists working on government advisory committees had conflicts of interest. Now we turn to consider how some of these close relations with industry impact the medical literature. Once again, recent studies and the need for continuing editorial interventions do not engender confidence.

A study by Krimsky, Rothenberg, Stott and Kyle, of articles published in “fourteen preeminent” medical journals in 1996, found that one-third of the articles they examined featured a lead author with some kind of


undisclosed conflict of interest. The conflicts they considered included holding an executive position or substantial stock in a company whose products were related to the publication; serving on the board of such a company; or holding or applying for a patent for an invention closely related to the research. These figures, as Krimsky et al. report, are probably conservative because of the difficulties identifying financial relations and the reluctance of many conflicted scientists to publicly acknowledge sponsorships and consulting arrangements.

Meta-analyses of published medical and scientific literatures also suggest that research funding is strongly correlated to research outcomes. Studies funded by for-profit corporations on their new drugs and therapies tend to favor the new treatments over traditional and existing drugs and therapies. In a review of articles on psychiatry, orthopedics and cardiology published in the British Medical Journal between 1997 and 2001, research sponsored by for-profit corporations was significantly more positive toward experimental and novel interventions than trials conducted by those without private sponsorship. One study, of 107 controlled clinical trials by Davidson, identified a statistically significant association between private sponsorship and support for new therapies. Among those trials which supported existing therapies, Davidson found that only thirteen percent were supported by drug manufacturers whereas eighty-seven percent emerged from research sponsored by universities and other not-for-profit institutions. Support for novel therapies seems to be closely tied to private commercial interests and intellectual property rights.

These findings are confirmed by reviews of safety and efficacy research. In their review of toxicological research on bisphenol A—a substance used in many plastics—von Saal and Hughes found that of the 115 studies they examined, none of the eleven studies sponsored by for-profit


107. See Lise L. Kjaergard & Bodil Als-Nielsen, Association Between Competing Interests and Authors’ Conclusions: Epidemiological Study of Randomized Clinical Trials Published in the BMJ, 325 BRT. MED. J. 249 (2002). Interestingly, the authors of a similar study suggested that the results might be due to study design: “[P]referential support was given to trials that had a greater chance of favoring one intervention over another.” Benjamin Djulbegovic et al., The Uncertainty Principle and Industry-Sponsored Research, 356 LANCET 635-38 (2000).

108. See Richard A. Davidson, Source of Funding and Outcome of Clinical Trials, 1 J. GEN. INTERNAL MED. 155 (1986).

109. See id.
corporations reported adverse effects at low exposures. In contrast, ninety-four of the 104 studies funded from public sources reported adverse effects at extremely low levels. Friedman and Richter reported that authors with financial interests in drug trials were ten to twenty times less likely to present negative findings than those without financial interests. A study by Freidberg et al. concluded that "pharmaceutical sponsorship of economic analyses [of drugs] is associated with reduced likelihood of reporting unfavorable results." Other studies support these findings, identifying strong associations between published opinions about the safety, efficacy and cost-effectiveness of pharmaceuticals and therapeutics and financial relationships with the manufacturers.

Overall, recent studies suggest that sponsored research is somewhere between four to eight times more likely to favor the sponsor's product or interests than research funded from public sources. Companies, and here it is important to emphasize that we are not referring exclusively to the tobacco and asbestos industries, routinely encourage and actively facilitate research which will help with the marketing of their products as well as prevent (or help with the defense in) future litigation.

Conflicts of interest related to sponsorship and other commercial relations are endemic in scientific medicine. Moreover, all of the research suggests that sponsorship and commercial interests shape the content of scientific knowledge and the published record. Barnes and Bero concluded their assessment of medical review articles as follows:

The conclusion of a review article may be suspect whenever the author has a financial interest in the outcome of the review. Therefore, our findings suggest that the authors should disclose their affiliations, source of funding, and other potential financial

110. See Frederick S. vom Saal & Clause Hughes, An Extensive New Literature Concerning Low-Level Dose Effects of Bisphenol A Shows the Need for a New Risk Assessment, 113 ENVTL. HEALTH PERSP. 926 (2005).
111. See id.
112. See Lee S. Friedman & Elihu D. Richter, Relationship Between Conflicts of Interest and Research Results, 19 J. GEN. INTERNAL MED. 51 (2004).
conflicts of interest, and that readers should consider these disclosures when deciding how to judge an article’s conclusions.116

A recent review of 1,396 high-impact science and medical journals found that only 15.8% had a published policy on conflict of interest information in 1997.117 (It is useful to remember that this is the very decade when the Supreme Court was consolidating the (admissibility) revolution against plaintiffs.) About one-third of journals request conflict of interest information from peer reviewers and about half require it from their editors. On the basis of their investigations, Krimsky and Rothenberg determined that less than one percent of the articles published in the 1,396 different journals featured any positive disclosure of personal financial interest by a contributor. These figures suggest that even in those instances where science and medical journals actually have policies there would seem to be extraordinarily low levels of compliance and little, if any, enforcement. Many of the conflicts of interests, sponsorships and funding arrangements seem to endure without disclosure. One study of corporate support for biotechnology published in 1984 estimated that even by that stage one quarter of research funding was already private.118

The prevalence of industry-university collaborations and conflicts of interest more generally leads Krimsky to the following conclusion:

Conflicts of interest have been linked to research bias as well as the loss of a socially valuable ethical norm—disinterestedness—among academic researchers. As universities turn their scientific laboratories into commercial enterprise zones and as they select their faculty to realize these goals, fewer opportunities will exist in academia for public interest science—an inestimable loss to society.119

This interpretation might be considered troubling for judges looking to litigation-free science—especially the scientific and medical literatures and university departments—for independence and reliability.

b. Secrecy Agreements and Suppression

In recent years there has been growing concern about for-profit companies funding research with restrictive covenants. These covenants are con-

116. See Barnes & Bero, supra note 115, at 1570.
117. See Sheldon Krimsky and L.S. Rothenberg, Conflict of Interest in Science and Medical Journals: Editorial Practices and Author Disclosures, 7 SCI. & ENGINEERING ETHICS 205, 210 (2001). The medical journals were the journals with the highest number of COI policies in place. See id.
118. David Blumenthal et al., Industrial Support of University Research in Biotechnology, 231 SCI. 242 (1986); see also Eric Campbell et al., Looking a Gift Horse in the Mouth: Corporate Gifts Supporting Life Sciences Research, 279 JAMA 995 (1998).
119. Krimsky, SCIENCE IN THE PRIVATE INTEREST, supra note 84, at 75-79 (defining “public interest science” as research addressing “issues that elude a market solution”).
tracts which provide researchers with access to drugs, patients and data on condition that the sponsoring organization may withhold results or delay their wider dissemination. There have been instances where scientists whose findings were adverse (to the sponsoring company’s economic interests) were threatened with breach of contract for going public. This includes circumstances where preliminary findings suggested that therapies were dangerous, associated with increased risk or physical injury, did not seem to be effective and the research scientists had legal obligations to notify regulators.

Corporations have an unenviable record of challenging adverse findings, including findings produced by researchers contracted to them. In many cases these challenges are ad hominem and create considerable difficulties for scientists and their institutions. Notorious instances, like the cases of Tyrone Hayes at Berkeley, Betty Dong at the University of California, San Francisco and Nancy Olivieri at the University of Toronto, provide instructive lessons for researchers contemplating publicizing results which raise doubts about the safety or efficacy of profitable pharmaceuticals.120 Restrictive covenants and industry attacks may inhibit research or discourage those interested in undertaking research on issues pertinent to public health.

The suppression of results is a major problem, especially when so much public health research is sponsored or undertaken by private, for-profit corporations.121 Research contracts may entitle a sponsor to discontinue studies or withdraw support on the basis of unfavorable preliminary results. Those sponsoring research can avoid the appearance of outright fraud by aborting studies or identifying methodological flaws which are used to suggest that undesirable results are suspect on technical grounds. Corporations, and their scientists, can be very skeptical when it suits their interests.

In 2004, the Canadian Medical Association Journal reported that one of the world’s largest pharmaceutical manufacturers (GlaxoSmithKline) had withheld, since 1998, trial results suggesting that one of its antidepressants (Paxil) offered no benefits to adolescents. A company memorandum, discovered during the preparatory stages of litigation, provided the explana-

120. Dong is discussed in Krimsky, Science in the Private Interest, supra note 84, at 14-18. Hayes is discussed in Sheldon Krimsky, Autonomy, Disinterest, and Entrepreneurial Science, 43 Soc’y 22, 25 (2006). Dong’s contract with Flint Laboratories (subsequently acquired by Knoll) for research on Synthroid included details of experimental design and data analysis and contained the following clause:

All information contained in this protocol is confidential and is to be used by the investigator only for the conduct of this study. Data obtained by the investigator while carrying out this study is also considered confidential and is not to be published or otherwise released without written consent from Flint Laboratories.

Krimsky, Publication Bias, supra note 96, at 69.

tion for the suppression: "It would be commercially unacceptable to include a statement that efficacy had not been demonstrated. . . ."  

Companies have a tendency to withhold results or challenge findings which are not in their commercial interests. Litigation, in contrast, offers the possibility to bring conflicts of interest, sponsorship and otherwise secret arrangements and knowledge into the public domain.

c. Ghost Writing

Although not a new phenomenon, in recent decades it has become increasingly common for product manufacturers, like Pharma, to pay eminent scientists to attach their name and imprimatur to studies undertaken and written by others. Sometimes the scientists engaged do not even have access to the study protocols or the primary data. Draft papers tend to be prepared by company employees or those contracted to a company. Once again, and for reasons which are self-evident, these arrangements tend to be suppressed.

One survey of 809 articles published across six medical journals in 1996 found that eleven percent (or ninety-three) were ghost-authored. Nineteen percent featured an honorary author.

d. Studies Designed to Confound

Another concern is that for-profit corporations fund research which is unlikely to achieve decisive results. Rather than make serious contributions to "knowledge," some research is pursued for its regulatory and legal potential (to confound). Even in areas with established protocols there is often sufficient interpretive flexibility around test subjects, laboratory conditions, duration, reporting requirements and the treatment of "anomalies" to generate favorable, or less unfavorable, interpretations. Sometimes studies are organized so that, notwithstanding appearances, researchers are not actually comparing drugs or therapies under similar conditions. A review of the literature by Bekelman et al. found that

125. See Annette Flanagan et al., Prevalence of Articles with Honorary Authors and Ghost Authors in Peer-Reviewed Medical Journals, 280 JAMA 222 (1998).
127. See W.J. Assendelft et al., The Relationship Between Methodological Quality and Conclusions of Reviews of Spinal Manipulation, 274 JAMA 1942 (1995); Helle Johansen & Peter Gøtzsche, Problems in the Design and Reporting of Trials of Antifun-
industry-sponsored studies were more likely to use inappropriate comparisons and inactive controls than publicly funded studies.\(^{128}\)

Perhaps it is ironic, given the reduction in civil litigation caused by more demanding admissibility standards, that the discovery attending litigation has provided some of the clearest indications of the manipulation of study protocols (i.e. design tweaking). Consider the following example of sponsored research which surfaced in litigation over the safety of silicone-breast implants.

According to documents introduced into the litigation, one company [Dow Corning] funded research that was designed to maximize the probability that silicone implants would not be found to cause a disease. The company established four conditions before it would award funds for the research. These conditions were viewed by the plaintiffs' lawyers as biasing the outcome of studies against the claims of their clients.

The conditions are as follows: First, studies should look at traditional connective tissue diseases and not the atypical symptoms reported by clinicians in the literature. Second, studies should include saline as well as silicone implants. Assuming the saline implants were not a problem, they would dilute the cases of concern, reducing the possibility of obtaining a statistically significant finding that silicone caused disease. Third, the studies should use a test of significance (two tailed) that considered both the positive and negative impacts of having silicone-breast implants, even though there were no hypotheses that silicone implants improved women's health. Fourth, all women who exhibited symptoms after 1991 should be excluded from the study. This exclusion kept the mean "years with implant" to between seven and nine, although some experts believe it can take ten or more years for symptoms to develop.\(^{129}\)

Other documents revealed that Dow Corning Corporation persuaded medical researchers at Johns Hopkins to modify the protocols used to study the health effects of breast implants.\(^{130}\)

\(^{128}\) See Bekelman et al., supra note 105, at 458.

\(^{129}\) Krinsky, Science in the Private Interest, supra note 84, at 156-57.

e. Qualifications, Caveats, Emphases and Omissions

In recent years, in response to problems associated with litigation, there is evidence that journal editors (and their lawyers) have encouraged those submitting papers on issues relevant to public health to moderate their findings and conclusions. This is not unusual where findings are adverse to consumer products, particularly products manufactured by those companies that advertise in or sponsor the journal.

A study of the interpretative statements included with epidemiological publications—the kinds of evidence typically preferred by federal judges in tort and product liability cases—suggests that authors routinely moderate their findings.131 Moreover, it is not uncommon for editors to give advance notice about adverse research to pharmaceutical manufacturers and for these companies to correspond prior to publication. Often correspondence about the precise wording of pending publications is managed by company lawyers. Perhaps against expectations, adverse findings and potential risks are more likely to be underestimated or represented conservatively than overstated.

Caveats and tempered conclusions tend to operate against the public interest and against those burdened with proof in litigation. Studies with ambivalent or less certain results are a valuable resource for corporate defendants where plaintiffs are burdened with proving causation. Not content with requiring plaintiffs to produce medical evidence to explain the cause of their injury or loss, several federal circuits formally require published epidemiological studies with statistically significant results to support causation in toxic tort cases.132 This is an incredibly onerous impost which is compounded by the placement of uncertain, ambiguous or qualified studies in the published medical literature.133

Emphases, omissions and other choices can also create difficulties or mislead. The following passage indicates how emphases and omissions in the reporting of research may foster misleading—i.e. situationally advantageous—impressions:

One does not have to manipulate data or use invalid methods tantamount to fraud to bias a scientific paper. The omission of the citation of contrary data and studies is very difficult to pick up in the process of peer review. There is considerable leeway within acceptable choices to investigate, interpret, and present data—and cite other studies . . . Intentional bias in choices of

132. See, e.g., Brock v. Merrell Dow Pharmaceuticals, Inc., 884 F.2d 167 (5th Cir. 1987).
methodology, data, and styles of interpretation within well-accepted limits are well-nigh impossible to detect or prove.\textsuperscript{134}

Ashford’s comments indicate how those with conscious or unconscious biases or disclosed or undisclosed interests may exaggerate the safety and efficacy of drugs, chemicals and therapeutics without engaging in behavior that would be derided as fraud or widely perceived as methodologically improper. The propriety of particular methods, interpretations, emphases and omissions, individually and in combination, can “fly under the radar” of reviewers, editors and more remote audiences.

f. Redundancy, Retraction and Correction

Favorable sponsored research—that is, research perceived as favorable to a sponsor—is sometimes cosmetically altered and re-published to create the impression that there are multiple studies which independently arrived at similar (and always favorable) conclusions.\textsuperscript{135} This, again, impacts adversely on plaintiffs because the record suggests that products are safer or more efficacious than is known to be the case. In one study of redundant publications the authors found that “inclusion of these duplicate data led to a twenty-three percent overestimation of the efficiency of” the drug.\textsuperscript{136} Perhaps as serious, the re-publication of favorable sponsored studies contributes to a false impression of corporate responsibility and concern with public health and safety.

On the subject of correction, studies suggest that journals are not only generally delinquent in this regard, but encounter practical difficulties repairing published literatures retrospectively. Publications demonstrated to be incorrect or fraudulent often receive little if any response, even in the journal where they were originally published. As Collins’s study of physicists suggests, the scientific literature—and this extends to published studies known to be fraudulent or implausible—is open to many inconsistent readings.

g. Institutional Responses: The Medical Journals Fight Back

We editors of medical journals worry that we sometimes publish studies where the declared authors have not participated in the design of the study, had no access to the raw data, and had little to do with the interpretation of the data. Instead the sponsors of


\textsuperscript{135} See Peter Gøtzsche, Multiple Publication of Reports of Drug Trials, 36 EUR. J. CLINICAL PHARMACOLOGY 429 (1989); Patricia Huston & David Moher, Redundancy, Disaggregation, and the Integrity of Medical Research, 347 LANCET 1024 (1996); Martin R. Traymér et al., Impact of Covert Duplicate Publication on Meta-Analysis: A Case Study, 315 BRIT. MED. J. 635 (1997).

\textsuperscript{136} See Drummond Rennie, Fair Conduct and Fair Reporting of Clinical Trials, 282 JAMA 1766, 1766 (1999).
the study—often pharmaceutical companies—have designed the study and analysed and interpreted the data. Readers and editors [and here we could include judges] are thus being deceived. Editors are also concerned that the declared authors might not have ultimate control over whether studies are published. That decision may rest with the funders of the research—perhaps a government department or a pharmaceutical company—which could mean that results unfavourable to the funders are suppressed. This distorts the scientific record and again deceives readers, allowing them to read only favourable results. Editors have taken steps to counter the problem by revising the uniform requirements for manuscripts submitted to biomedical journals of the International Committee of Medical Journal Editors, and changing editorial practices.


Just in case the previous examples are thought to be unrepresentative or exaggerated, and to provide the reader with some sense of institutional responses to problems with the medical literature, the actions of the International Committee of Medical Journal Editors (ICMJE or “the Committee”) provide an instructive example.

The ICMJE is composed of editors from the leading medical journals. Members include the New England Journal of Medicine (NEJM); Journal of the American Medical Association (JAMA); British Medical Journal (BMJ); Lancet; Canadian Medical Association Journal; Annals of Internal Medicine, Medical Journal of Australia; Journal of the Danish Medical Association (Ugeskrift for Laeger) and the Dutch Medical Journal (Nederlands Tijdschrift voor Geneeskunde). Initially working under the title of the “Vancouver Group,” members of the ICMJE have been developing guidelines for submissions to biomedical journals since 1978. Over the last decade, however, the Committee has expanded its remit, developing a number of “Separate Statements” on editorial policy, and in the last few years, consolidating its work. A more comprehensive guide is now published as the Uniform Requirements for Manuscripts Submitted to Biomedical Journals: Writing and Editing for Biomedical Publication (2005) ("the Uniform Requirements"). For our purposes the ICMJE’s work on conflicts of interest, disclosure and trial registration is of particular interest.

The extract from the British Medical Journal reproduced at the beginning of this sub-section suggests concern about conflicts of interest and suggests that the deliberate contamination of the medical literature is ubiquitous and has encouraged vigorous responses from aggrieved edi-

---

137. Richard Smith, Maintaining the Integrity of the Scientific Record: Editors Make a Move, 323 BRIT. MED.  J. 588 (2001). Smith took pains to make clear that this was not “an attack on the pharmaceutical industry. Almost all new drugs are developed by the industry, and many companies have high ethical standards and will see no problem in complying with the new policies.” See id.
Supersizing Daubert

2007]

Supersizing Daubert

In 1988 the ICMJE encouraged authors to identify financial relationships which might create conflicts of interest.\textsuperscript{138} This position was reinforced in 1993 when the Committee passed a resolution urging those submitting manuscripts to acknowledge all financial support and financial and personal interests associated with submissions.\textsuperscript{139} In 2001, in response to surprisingly low numbers of disclosures, the Committee imposed a new protocol that made disclosure of conflicts mandatory and encouraged other biomedical journals to follow its lead.

The new policy is set out in the Uniform Requirements. The ICMJE defines conflicts of interest broadly and insists that authors disclose any conflicts so that editors, peer reviewers and other readers might take them into consideration when "judging the manuscript":

Conflict of interest exists when an author (or the author's institution), reviewer, or editor has financial or personal relationships that inappropriately influence (bias) his or her actions (such relationships are also known as dual commitments, competing interests, or competing loyalties). These relationships vary from those with negligible potential to those with great potential to influence judgment, and not all relationships represent true conflict of interest. The potential for conflict of interest can exist whether or not an individual believes that the relationship affects his or her scientific judgment. Financial relationships (such as employment, consultancies, stock ownership, honoraria, paid expert testimony) are the most easily identifiable conflicts of interest and the most likely to undermine the credibility of the journal, the authors, and of science itself. However, conflicts can occur for other reasons, such as personal relationships, academic competition, and intellectual passions.

All participants in the peer review and publication process must disclose all relationships that could be viewed as presenting a potential conflict of interest. Disclosure of these relationships is also important in connection with editorials and review articles, because it can be more difficult to detect bias in these types of publication than in reports of original research. Editors may use information disclosed in conflict of interest and financial interest statements as a basis for editorial decisions. Editors should publish this information if they believe it is important in judging the manuscript.


\textsuperscript{139} See International Committee of Medical Journal Editors, Conflict of Interest, 341 LANCET 742 (1993).
When authors submit a manuscript, whether an article or a letter, they are responsible for disclosing all financial and personal relationships that might bias their work. To prevent ambiguity, authors must state explicitly whether potential conflicts do or do not exist.\(^{140}\)

Although now mandatory, the success of disclosure and the extent of compliance is uncertain. Journals, like end users, are not well-positioned to make assessments about compliance or unilaterally identify private financial arrangements between scientists and corporate sponsors.

Limitations with disclosure and its relative weakness have encouraged more drastic actions. In 1994, for example, the NEJM introduced a new policy on the publication of "cost-effectiveness analyses." Analyses about the relative cost-effectiveness of different treatments were so dependent upon potentially biased assumptions and values that:

They will not be excluded from consideration if they are supported by a grant from industry to a non-profit institution, but like review articles they will be excluded from consideration if any of their authors has a personal financial conflict of interest. Simply disclosing such conflicts of interest, as others have suggested authors do, will not suffice.\(^{141}\)

For cost-effective analyses and review articles, even the disclosure of financial interests was considered by the editorial staff of the NEJM to be an inadequate safeguard.

More recently, the ICMJE has extended its program of reforms by calling for the prospective registration of all clinical trials. Formal registration is intended to reduce suppression, redundancy, exaggeration and fraud as well as produce a comprehensive, publicly accessible database.\(^{142}\)

\(^{140}\) See International Committee of Medical Journal Editors, Uniform Requirements for Manuscripts Submitted to Biomedical Journals: Writing and Editing for Biomedical Publication §II.D (2005), available at www.ICMJE.org (last visited December 19, 2006) [hereinafter ICMJE, Uniform Requirements]; Sheldon Krimsky, Small Gifts, Conflicts of Interest, and the Zero-Tolerance Threshold in Medicine, 3 Am. J. Bioethics 50 (2003). Notably Krimsky concludes his paper with the following: "There are perhaps some institutions whose fiduciary duty responsibility to the public is of such importance that it makes sense to establish a zero-tolerance for gifts of any value. These might include journalists, judges, professors, and physicians." Id. at 51. We might want to reflect on sponsored judicial education and conferences for judges and lawyers.

\(^{141}\) Jerome P. Kassirer & Marcia Angell, The Journal's Policy on Cost-Effectiveness Analyses, 331 New Eng. J. Med. 669, 670 (1994). The New England Journal of Medicine required that industry grants be provided to organizations rather than individual scientists; written assurance that the scientists were independent; and the manuscript to include all data and all assumptions. See id. at 670-71; see also Chaim M. Bell et al., Bias in Published Cost Effectiveness Studies: Systematic Review, 332 Brit. Med. J. 699 (2006).

The Committee now requires clinical trials to be registered on a public database before they will even consider publishing trial results: "The ICMJE member journals will require, as a condition of consideration for publication in their journals, registration in a public trials registry." The Committee explained the reasoning behind this requirement: "If all trials are registered in a public repository at their inception, every trial’s existence is part of the public record and the many stakeholders in clinical research can explore the full range of clinical evidence."

The Uniform Requirements list the necessary data elements and recommend that trial registration numbers be published at the end of article abstracts. Again, the Committee encourages the editors of other biomedical journals to adopt a similar policy. Registrations, as the Committee and several commentators recognize, will not resolve many difficult issues such as the kinds and stages at which trials should be registered; the kinds of information to include; the format of the information and the temporal relationship between a trial and registration; how to determine what counts as genuine compliance; how those accessing the data can ascertain the quality of the study and the statistical analysis; how the various results should be included in meta-analyses; how non-published results ought to be treated, and so on. Nevertheless, like the policies on conflicts of interest, the Committee’s actions demonstrate a concerted attempt to identify and manage the impact of private sponsorship on scientific and medical publication.

These, of course, are not the only subjects on which the Committee has developed guidelines. Other policies include the call to "consider seriously for publication" any "study of an important question . . . where the results are negative (that is, convincingly allow the null hypothesis to be accepted) or positive (that is, allow the null hypothesis to be rejected)." Failure to publish negative studies contributes to "publication bias." The Committee explains: "Many studies that purport to be negative are, in fact, inconclusive; publication of inconclusive studies is problematic, since they add little to biomedical knowledge and consume journal resources."

The Uniform Requirements also detail policies on "Corrections, Retractions and ‘Expressions of Concern,’” “Redundant Publication,” “Advertisements,” and "Ethical Practices in Clinical Research: The Requirements," which are discussed in the following sections.
ing” and “Supplements, Theme Issues and Special Series.” Given federal admissibility jurisprudence over the last two decades the Uniform Requirements should be required reading for judges.

h. Overview

Upon closer examination, peer review and publication—the very benchmark(s) federal judges hoped to use to rehabilitate dependent scientific knowledge, including scientific knowledge that was purposive or encumbered with limitations—do not seem to guarantee reliability. Rather, the medical and scientific literatures appear to be contaminated with purposive research designed to help chemical companies, oil producers, and pharmaceutical and other manufacturers market their products, secure regulatory compliance and resist liability. These efforts would seem to epitomize science for litigation. They provide some indication of the sophistication, planning and resources devoted to scientific research and strategic publication. They are also suggestive of duplicity and hypocrisy. At the same time they were submitting amicus curiae briefs decrying lax admissibility standards and demanding that courts require plaintiffs to produce objective and independent expert evidence, for-profit corporations were actively sponsoring and tailoring research.147

In the very years when judges were searching for extrinsic epistemic anchor points—like publication in peer-reviewed journals—the editors of scientific and medical journals were reforming editorial policies in order to improve the reliability of their publications. Interestingly, scientific journals have been drawing upon traditional legal and administrative

147. In this context consider the image of science included in an amicus curiae brief submitted in the Daubert appeal to the Supreme Court on behalf of the Product Liability Advisory Council (PLAC), the National Association of Manufacturers (NAM) and the Chemical Manufacturers Association (CMA):

The scientific method speaks to the process by which scientists reached a conclusion . . . . In particular, the scientific method requires that one (1) first set forth a hypothesis, (2) design an experiment, or more properly a set of experiments, to test the hypothesis, (3) conduct the experiment, collect the data, and then analyze those data, (4) publish the results so that the may add not only to the body of knowledge, but also be subject to external scrutiny, and (5) ensure that those results are replicable and verifiable . . . . When attempting to draw scientific conclusions, whether at the laboratory bench or the courtroom bar, one should at a minimum be required to formulate those conclusions in accord with the scientific method. That method provides a straightforward, relatively simple, and reasonable test for the admissibility of expert opinion. It is a test, moreover, which federal judges can readily apply . . . .


Many large for-profit organizations and collectives have aggressively championed commercially advantageous reform to regulatory and legal practice based on images of scientific practice which bear little resemblance to the kinds of strategic research they routinely undertake and sponsor. See Edmond, Just Truth, supra note 4.
tools. The editors of scientific and medical journals have not been calling for enhanced peer review or methodological assessments based on the work of archaic philosophers like Popper. Instead, they have emphasized the need to disclose conflicts of interest, funding arrangements and consultancies in order to make the origins and orientations of the published research more transparent. Professional scientific recourse to disclosure and registration betrays the weakness of the scientific method and the limitations of peer review. Insights like these should breach the confidence with which judges, like Kozinski and Blackmun, approach the published medical (and scientific) literature. It might be unsettling but corporate sponsored science—that is, science for marketing, regulation and litigation—tends to be oriented to future uses in ways where the orientation may be embedded, subtle or deliberately covert. The underhanded and clandestine nature of much of the sponsorship might itself be considered revealing.

Given the work of sociologists of science, in conjunction with the sustained intervention of many of the world’s preeminent medical editors, where, we might wonder, is the jurisprudence tempering the Daubert criteria, science for litigation or the implications of reforms to medical publication for plaintiffs and the viability of tort and product liability? Where is the discussion (or supplementation) of the qualifications perfunctorily included with the celebration of publication and peer review in Daubert in the Supreme Court and on remand to the Ninth Circuit? Where are the limitations with peer review and publication seriously considered or applied by federal judges? Where are the judicial expressions of skepticism or concerns about research sponsored by corporations? And, why are federal judges more trusting of methods and norms and their remedial potential than the technically proficient editors of the New England Journal of Medicine, the Journal of the American Medical Association, the British Medical Journal and the Lancet?

F. Demarcation Difficulties

Philosophers, sociologists and scientists have been unable to identify or develop criteria which can be consistently operationalized to distinguish between the scientific, the non-scientific and the pseudo-scientific. Arguably, no such criteria exist.

According to Popper’s student, Lakatos: “Is, then, Popper’s falsifiability criterion the solution to the problem of demarcating science from pseudoscience? No. For Popper’s criterion ignores the remarkable tenacity of scientific theories. Scientists have thick skins. They do not abandon a theory merely because facts contradict it.”

Lakatos was unimpressed with falsification as a tool for practical demarcation and recognized that scientists (and natural philosophers) routinely main-

tain(ed) theoretical commitments in the face of inconsistent evidence—that is, recalcitrant "facts."

The philosopher Laudan summarized the search for a demarcation criterion as follows: "The quest for a specifically scientific form of knowledge, or for a demarcation criterion between science and non-science, has been an unqualified failure. There is apparently no epistemic feature or set of such features which all and only the 'sciences' exhibit."\(^{149}\) In consequence, the boundaries around classifications like "science," "pseudo-science" and "junk science" become indistinct and potentially porous.\(^{150}\) Indeed, the boundaries used to distinguish between the sciences and to demarcate scientific from non-scientific activity, seem more comprehensible when understood as flexible, strategically manipulated and political.\(^{151}\)

If we momentarily turn our attention to the concept junk science, we can begin to appreciate that it is more of a polemical tool than an analytical one. Many of those who use the term may not encounter difficulties identifying what they believe to be "junk science," but things become more complicated when it comes to credibly distinguishing so-called junk science from authentic scientific knowledge on the basis of consistent application of some preferred demarcation criteria.\(^{152}\) Often the process of classification (or demarcation) seems to incorporate overt ideological commitments rather than practical, technical or methodological distinctions.\(^{153}\) It might not be considered coincidental that the individual most responsible for popularizing the term "junk science" (more below) relies heavily on simplistic models of scientific method, scientific norms, peer review and publication.


151. See Gieryn, supra note 79.


153. For an influential study of attempts to distinguish the study of parapsychology from the realm of legitimate science, see H. Collins & T. Pinch, Frames of Meaning (1982); see also G. Bowker & S. Star, Sorting Things Out: Classification and its Consequences (1999).
We should not be deceived by apparently easy solutions to scientific disagreement, uncertainty and risk. There are no simple or stable means capable of distinguishing so-called good science from so-called bad science. Rather than getting mired in the distractions caused by pejorative labels and allegations, we ought to be concerned with attempts to locate expertise that is sufficiently reliable for legal decision-making as well as the broader regulatory and distributive goals of our civil and criminal justice systems.\(^\text{154}\)

G. An Imaginary (Scientific) Community

It is difficult to identify a homogenous scientific community. Historically, scientists from different disciplines tended to interact infrequently and, even within fields and sub-specializations, often relied on different assumptions, equipment, practices, methods and heuristics.\(^\text{155}\) The idea of a community of scientists linked by practices, values or even a shared ethos is not merely historically and sociologically illusory, but actually fictitious.\(^\text{156}\) In reality there is no coherent scientific community, only a collection of rather disparate sciences (in the plural). Once we lose the universal method, and realize that norms are routinely contravened, it becomes increasingly difficult to locate shared features meaningfully linking the many different activities we classify as sciences. Collins’s study of gravitational physicists, discussed earlier, raises questions about where fields and disciplinary boundaries start and stop as well as who should be considered to be a competent member of a field or community.\(^\text{157}\)

H. Recapitulation

Together, these insights transform knowledge production and modern forms of expertise—including the hardest sciences (e.g., physics, chemistry, biology and even medicine)—into complex social activities where what constitutes good science, along with reliability, images of objectivity, independence and purpose, are opened to contestation and become susceptible to investigation. Consequently, as a reflection of the modern sciences and modern manifestations of expertise, the models of science appearing in Daubert and subsequent judicial practice, like Kozinski’s re-


course to *science for litigation*, would seem to be fundamentally and irreparably flawed.

There is a conspicuous gap between judicial descriptions of scientific activity and the images of the sciences developed in more specialized scholarly literatures. The significance of this gap may have been accentuated by the way senior judges explained changed admissibility conditions, particularly the idea that they were facilitating convergence between the laboratory and the courtroom. If, however, legal approaches to scientific evidence are empirically tenuous then some of the implications may be unintended or, what is more alarming, illegitimate. By adhering to "mechanistic" formulations—like *science for litigation* and the *Daubert* criteria—judges risk making determinations on the basis of highly artificial formulations with little relevance to either the historical sciences or contemporary practice.\(^{158}\) Some of these issues are perhaps most conspicuous in relation to reliance on one-dimensional impressions of peer review and publication and a reluctance to respond to widespread, if cynical and corrosive, corporate (mis)conduct.

At this point I want to consider just a few of the evidentiary and procedural implications arising from the use of *science for litigation*. Now we can observe how *science for litigation* and the *Daubert* criteria tend to be used differentially. We will pay particular attention to how they assume their *hardest* manifestation when applied to evidence adduced by plaintiffs and criminal defendants.

### IV. SOME IMPLICATIONS OF SCIENCE FOR LITIGATION

Apprised with a more empirically-grounded sense of the limitations of idealized images of expertise and with renewed sensitivity to the way scientific research is embedded in the modern economy and strategically (and often legitimately) oriented to legal and regulatory questions, in this section it is my intention to reconsider some of the implications attending the adoption of *science for litigation* and the kinds of historically and sociologically impoverished images of science associated with *Daubert*. Exploring the way (the concept) *science for litigation* is mobilized and put to work will help us to understand some of its ideological functions.

#### A. Civil (In)Justice: Marginalizing Plaintiffs and Insulating Defendants

In civil litigation it is the plaintiff who bears the burden of proof and needs to establish causation. What federal judges, like Kozinski, seem to desire from plaintiffs is non-aligned scientific research which unambiguously supports their case. We can understand why judges would want conclusive, independent and uncontroversial research on which to base their admissibility decisions and judgments, but where it is available this type of

---

evidence encourages settlements, and generally favorable settlements, rather than litigation.

The expectation that the plaintiff will produce expert evidence which is independent of litigation, yet pertinent to the issues before a court, creates practical difficulties. If the relevant expert literature is not clear, if research has not been undertaken or published, or if the findings do not bear directly on the legal issues, what are plaintiffs to do? How are plaintiffs to obtain evidence that is independent of litigation or not-for-litigation? How can they ever escape the stigma associated with purposive research, tailored evidence and partisan experts? An extremely important question in this context is: Who is doing the kinds of research which federal judges expect plaintiffs to produce? This question links legal practice, the viability of product liability and tort law, to the organization and funding of modern scientific research. Are public institutions apprized of these legal expectations when they make decisions about funding public health research? For-profit corporations certainly are.

Even in those situations where there is research, published or otherwise, available to a plaintiff, it almost always requires adaptation, refinement, interpretation or meta-analysis. Where there is concern about science for litigation, these processes introduce the possibility of judicial cynicism, recrimination and the exclusion of evidence. Unless they rely directly on the results of published studies—assuming they exist—plaintiffs can always be criticized for tailoring their evidence and presenting implicitly unreliable science for litigation. This essay has endeavored to explain that labels like “tailoring,” “adaptation,” “distortion” and “pre-litigation” can be deceptive. Their popularity reveals much about contemporary judicial attitudes toward science and expertise and also toward plaintiffs and juries.

In a striking contrast, civil defendants benefit from the pervasive belief that plaintiffs’ expert evidence is a kind of partisan (or pathological) knowledge. These beliefs are reflected in onerous admissibility standards and pre-trial determinations (e.g., Daubert hearings) which do not require civil defendants to present their evidence. Typically, defendants have far greater resources—economic, institutional, legal, scientific, political and public relations—than plaintiffs. Defendants are frequently experienced litigants (“repeat players” in Galanter’s terminology) with considerable control over (the possibility of) research and information related to their products.159 Significantly, much of this information may be confidential (and remain undisclosed). Moreover, judicial confidence in research undertaken before litigation, along with the remedial power of publication, allows defendants to rely upon their in-house and sponsored research with relative impunity. Compliance research and sponsored research which merely pre-dates a legal claim or is not undertaken by a defendant corpo-

ration's employees is frequently treated as not-for-litigation or independent.

While it is rarely described in this way, there can be little doubt that modern corporations produce and sponsor science for litigation and strategically tailor their expert evidence on those occasions when plaintiffs' evidence overcomes the admissibility hurdles (and offers of settlement) to force a jury trial. When you start to think about it, sharp divisions between in-house and sponsored research undertaken by large corporations habituated to potential tort, contract and product liability suits, on the one hand, and plaintiffs' research or reviews assembled to support pending litigation, on the other, start to break down. Drawing upon mythical, though no doubt appealing, images of expertise, judges have developed standards which tend to insulate (and therefore privilege) the litigation-oriented dimensions of research sponsored by corporate defendants while emphasizing (and devaluing) the social and litigation-oriented aspects of evidence assembled by plaintiffs. This approach exacerbates the many disadvantages already confronting plaintiffs.

Here, it is important to recognize that only the corporations have options. They could sponsor research at arms-length, disclose funding arrangements with scientists and research institutions, register all of their clinical trials and make all of their data publicly available, but they choose not to. There are not compelling commercial reasons for all of these choices. Existing doctrinal and adjectival rules compel plaintiffs to rely primarily on published studies and disclosed materials, notwithstanding the fact that many of these studies are sponsored by litigation-wizened defendants and, whether disclosed or not, undertaken by scientists with industry linkages and/or financial conflicts.160 Where research identifies heightened risks or other adverse findings of potential value to plaintiffs, corporate defendants routinely confound the issues by suppression, sponsoring and gerrymandering further studies, formally criticizing the methods and protocols of the existing studies, perhaps in conjunction with the credibility of the scientists involved.161 These deconstructive efforts may


161. See Paul Fischer, Science and Subpoenas: When Do the Courts Become Instruments of Manipulation?, in RESCUING SCIENCE FROM POLITICS: REGULATION AND THE DISTORTION OF SCIENTIFIC RESEARCH, supra note 96, at 86-92. Fischer makes the persuasive point that sometimes it would be easier for the aggrieved companies to simply replicate the offending studies rather than attack scientists and their performance. See id. This course of action, however, is very rarely undertaken, even when the studies could be reproduced quickly and inexpensively. Fischer's work recognizes that discrediting previous studies has other more instrumental and demonstrative functions:

Had RJR [RJ Reynolds Tobacco Company] been concerned about the veracity of our findings, it could have duplicated our research in several weeks for a few thousand dollars. Instead, the corporation spent two and
occur prior to litigation as well as in court. *Adverse* studies, including large credible studies funded with public monies and undertaken by scientists without conflicts of interest, are often criticized by corporate consultants and advisers.

It is both unfortunate and curious, then, that judges have been so complicit in the exclusion of plaintiffs' expert evidence especially if corporations are allowed to introduce science for litigation—that is, their own sponsored research—where a case makes it to trial. But even this is to put it too mildly. Private corporations are strategically developing research and cultivating the published scientific (especially biomedical) medical record to assist with the marketing of their products as well as the management of regulatory and legal risks. And, unlike the average plaintiff, private corporations have considerable experience and resources in terms of study design, reporting and publication. Federal judges would seem to have taken an inexplicably narrow view of *science for litigation*. Relying upon public rhetorics, or folk versions, of expertise, federal judges have blinded themselves to the relations between science and capital and the incredibly strategic (and cynical) potential of corporations.162

One final issue, particularly apposite to public health and safety, concerns the stultifying effects of recent admissibility jurisprudence on public health research:

[M]anufacturers understand the significance of science to liability and regulation so well that they may actually resist conducting basic tests on their products or auditing the potential harms caused by their activities. As long as scientific information can be incriminating and lead to costly liability and regulatory requirements, ignorance is bliss.163

And, “[w]orse, as word gets out that policy-relevant [i.e. public health and environmental] research is likely to precipitate vicious attacks from economically interested industries, academic scientists may shy away from such research, preferring instead to labor in less controversial vineyards.”164

The admissibility difficulties confronting plaintiffs manifest in a variety of ways. Corporations now have fewer incentives to undertake safety research. Because safety and efficacy research always has the potential to assist future claimants, defendants—who are not required to prove anything when sued—have few incentives to undertake it. Safety research, even in-house and confidential research, entails the possibility that compa-

---


164. *See id.* at 25.
nies will subsequently be vilified for not disclosing risks or acting on the basis of retrospectively constructed cognizance.

Our current regimes of admissibility and proof discourage research and prevent public reviews of manufactured products and corporate behavior. Removed from obligations to persuasively demonstrate the safety and efficacy of their products, particularly once they have secured marketing approval, we have reached the parlous situation where federal courts have made it preferable for manufacturers to avoid safety research, or to strategically sponsor research designed to assist with marketing and litigation, rather than assiduously pursue research into public health. Raised admissibility standards have shifted responsibility for proof of risk to individual citizens.

B. Criminal (In)Justice: Tough on Crime, Soft on State Science

Another way to identify the practical limitations of science for litigation and simultaneously to unravel some of its ideological dimensions is to consider its impact on the criminal justice system. Against expectations, perhaps, admissibility standards do not seem to have been applied with the same kind of vigor in criminal trials. Notwithstanding the supreme importance of reliable evidence and accurate decision-making, given the seriousness of conviction, with relatively few exceptions federal (and state) judges have been reluctant to hold the government’s experts—forensic scientists and technicians—to the same kinds of standards they routinely require of expert witnesses appearing on behalf of civil claimants like the Joiners and Carmichaels.165

Returning to Daubert on remand, we can see that in developing science for litigation, Judge Kozinski was conscious of an asymmetry between the civil and criminal spheres. Having introduced the purpose of expert evidence as an important consideration in admissibility decision-making, Kozinski qualified its implications for forensic scientific evidence:

[FN5] There are, of course, exceptions. Fingerprint analysis, voice recognition, DNA fingerprinting and a variety of other scientific endeavors closely tied to law enforcement may indeed have the courtroom as a principal theatre of operations. See, e.g., United States v. Chischill, 30 F.3d 1144, 1153 (9th Cir.1994) (admitting expert testimony concerning a DNA match as proof the defendant committed sexual abuse and murder). As to such dis-

165. In many cases, plaintiffs’ experts in civil cases are much better qualified to testify than the technical experts routinely appearing on behalf of the state in criminal matters. For example, the Carmichaels’ expert, prevented by the Supreme Court from testifying about tire wear and damage, had worked for Michelin America for ten years and possessed a Masters Degree in Engineering from Georgia Institute of Technology.
Kozinski recognized that some types of scientific endeavors have the courtroom as their "principal theatre of operations." For reasons that are not explained, however, he suggests that these purposes "will obviously not be a substantial consideration." The state's expert evidence seems to be inoculated from contamination (by the socio-legal) in ways that criminal defendants' (and plaintiffs') evidence is not, and ordinarily cannot be, inoculated. Interestingly, there is no reference here to the redemptive potential of peer review and publication. Confidence in the state's forensic sciences seems to be an article of faith paralleling the general confidence in commercially-oriented, pre-litigation, "public health" research. The great irony here, of course, is that of all the locations in our legal system where one party should bear responsibility for producing reliable expert evidence, it is the prosecution in criminal matters. Yet these are some of the very cases where busy federal judges have been reluctant to extend their (putatively) skeptical gaze to the state, its scientists and its technicians. Margaret Berger puts this diplomatically when she suggests that "the courts seem very conscious of the need to protect society against dangerous persons."  

Interestingly, in Barefoot v. Estelle, just a decade before he wrote the Daubert majority decision, Justice Blackmun distinguished between the standard of reliability required for civil litigation, which he equated with "money damages," and those where "a person's life is at stake." He concluded that in the latter "a greater reliability should prevail." Given these sentiments, we must wonder, "[w]hy are there higher admissibility thresholds in civil litigation than many life and capital cases?"  

While the criminal justice system may have been an important contributor to the development of standards in some of the forensic sciences, like DNA typing and the related use of population statistics, the same claim could not be made about many other areas of specialization—such as latent fingerprints, handwriting analysis, voice identification, ballistics comparisons and explosives identification—which gained legal recognition in earlier decades. These forensic sciences appear to have been "grandfathered" and seem to be taken largely on (judicial) trust.
cally, the perceived strength of DNA typing evidence and knowledge of its
limitations may owe much to forensic controversy and wide publicity. As the sociologists Wynne and Jasanoff have explained, courts have con
siderable potential if only judges are willing to allow expert evidence to be
admitted and contested. It might be considered revealing that in the
last few years there have been persistent attempts to challenge—using the 
Daubert criteria and concepts like science for litigation—some of the very 
forensic techniques endorsed by Kozinski in 1995. To their discredit, fed
eral judges have, on average, been unwilling to entertain these important 
and festering controversies.

How should we interpret these circumstances? Why are federal
judges soft on the expertise adduced by the state yet hard on expert ev
idence mobilized by plaintiffs and those accused of crime? The differential
approaches preferred by federal judges might be thought to betray ideo
logical and institutional anxieties. High admissibility standards—which
effectively preempt litigation—suggest a skepticism of plaintiffs, their ex
perts and lay juries. The asymmetrical application of Daubert and recep
tiveness to the government’s litigation-oriented forensic evidence appear
to reflect an acute sensitivity to crime control and a desire to secure con
victions. Institutionally, both approaches tend to reduce the likelihood
and length of resource-intensive trials, as (potential) civil actions are
“lumped,” abandoned or settled (for historically low damages), and crimi
nal cases are more likely to feature admissions, plea bargains and convic
tions.

Unquestioning confidence in the state’s expert evidence is both inap
propriate and misplaced. Judges have an important role to play in keep
ing the state and its evidence reliable and accountable. Here, failures may
counter to miscarriages of justice and institutional illegitimacy. For
judicial acquiescence disadvantages the most vulnerable: minority groups;


178. Obviously there are tensions here because concerns about crime are driven by community attitudes, politicians and the media.
the mentally ill; the usual suspects; and those who are generally unable to afford experts and more experienced lawyers to contest incriminating scientific, technical or other specialized evidence.\textsuperscript{179} Judicial exasperation with the heavy case loads imposed on under-resourced courts and concerns about crime, in conjunction with confidence in the state’s forensic sciences, should not, however, prevent judges from expecting (or imposing) the highest possible admissibility thresholds on the state and its experts. Governments, after all, are in the best position to improve the standard of expert evidence relied upon by those negotiating pleas and prosecuting crime. For reasons of public policy and social legitimacy, judges should disaggregate themselves from the prosecution and hold the state’s expert evidence to the highest standards practicable.

V. Conclusions

The subjects discussed in this essay raise many important issues for legal practice and public confidence in legal and regulatory institutions. In closing, I want to raise several issues related to science for litigation, the way it is used, and its implications.

A. The Limits of Science for Litigation

The purposes, or apparent purposes, of research do not provide conclusive guides to reliability. We cannot systematically read what we believe to be self-serving, interested or dependent relationships into the content of scientific knowledge. "We cannot infer from decisions that we believe to be self-serving that they are in fact the consequence of questionable relationships a policy maker [we might add expert witness or author] had with different stakeholders."\textsuperscript{180} Conflicts of interest and oriented knowledge do not prove bias, distortion, inefficacy or unreliability.\textsuperscript{181} This might be considered unfortunate. For, the influence of sponsors, employers, personal interests and (continuing) financial relationships may each affect research orientations and outcomes in a variety of ways—sometimes ways that can be quite difficult to detect.

Nevertheless, it would be unwise to simply ignore conflicts of interest and funding arrangements or persist with images of expertise that blind us to the varieties of contemporary practice and some of their risks.\textsuperscript{182} Earlier we saw that meta-analyses of medical literatures have consistently iden-
tified strong associations between sponsorship and the results of published research. Experience (as well as studies) has taught us that we should be cautious about directed research and interested knowledge and that we should not overlook the circumstances in which knowledge is developed. But these lessons should be applied to civil defendants as well as plaintiffs, and to the state's forensic science as well as any exonerating evidence adduced by those accused. Many federal judges (and commentators) have inverted what we might expect from more principled and socially responsible approaches to expertise.183

Federal judges do not seem to have appreciated the prevalence and potential significance of conflicts of interest, nor the potentially pernicious effects of some corporate behavior on the scientific and medical record. Many seem oblivious to the way modern corporations (i.e. repeat players and serial defendants) systematically prepare research for regulatory compliance and anticipated litigation. Alarmingly, these corporate strategies appear to be remarkably successful.

Given the manipulation of scientific research and publication, disclosure and transparency would seem to be important procedural responses. Even where studies appear to be methodologically rigorous and supported by broader literatures, conflicts of interests, continuing relations and funding arrangements should all be disclosed so they can be factored in. Transparency will not always provide clear answers, but in almost all circumstances it will be a pre-requisite to any socially credible assessment of scientific knowledge.184 In the absence of algorithms or bright-line demarcation tools, we might want to know if scientists have previous or pending relations with the corporations whose products they study; whether they have undertaken research which has not been registered or published; whether they have signed contracts restricting their ability to publish (or were willing to); whether they own or owned stocks or held interests or options in the companies supporting the studies, and so on.185

There is no doubt that we should approach the expert evidence assembled by plaintiffs with caution. However, we can not afford to overlook the position of the plaintiff in the overall civil justice system. It would be a mistake to impose unrealistic expectations on plaintiffs or to judge plaintiffs’ expert evidence using implausible or onerous models of expertise, especially when the same standards might embarrass a corporate defendant were the case to go to trial. Moreover, we need to take a broader


183. This should not be understood as some kind of conspiracy because many judges and commentators appear committed to their definitions of expertise.


185. These kinds of questions are routinely asked of potential jurors and judges.
look at the evidence used for legal and regulatory purposes and the kinds of organizations that have evolved to produce, administer and misrepresent it.\textsuperscript{186}

One way of approaching these issues is to think about the goals and purposes of criminal and civil law. Without wanting to be too prescriptive here, we might ask, “Are these purposes advanced by the way judges currently characterize scientific and expert evidence in admissibility decision-making?” Using tort as an example, the regulatory and restorative potential of tort law may be seriously jeopardized if plaintiffs are unable to get their cases into court, if civil juries are prevented from considering corporate behavior and possible malfeasance and if potential defendants are discouraged from undertaking or sponsoring independent and publicly accessible research into the safety and efficacy of their products.\textsuperscript{187}

B. Different Standards of Admissibility and Proof?

Trial lawyers frequently decry the effects of Daubert and the exclusionary ethos. They lament the existence of too much hard Daubert, too much gatekeeping and an obsession with science for litigation. In contrast, those representing criminal defendants or those, like Cole, Faigman, Saks and Risinger, trying to make the forensic sciences more accountable, cannot get enough hard Daubert.\textsuperscript{188} They bemoan the failure of judges to take their gatekeeping responsibilities seriously, rigorously apply the Daubert criteria and extend concerns about the origins and purpose of expert evidence to the state in criminal trials. Given this state of affairs, it might be time to begin to think about formally distinguishing admissibility standards so that plaintiffs and criminal defendants bear lower admissibility burdens than the state. Once again we might want to reflect on the distinctive purposes of civil and criminal law and the respective capabilities of the parties. It might also be an opportune moment to reconsider the expectations we place on the manufacturers of foods, chemicals,


\textsuperscript{187} In consequence, Kozinski’s orientation must be considered questionable, if not specious. Unfortunately, the very indicia Kozinski suggests we can rely upon will be least reliable in the midst of scientific disagreement, uncertainty and commercial activity. Here, Kozinski’s recourse to ideals actually inverts the reality. For, privately funded research—that is research undertaken “in the usual course of business”—may actually be of low standards and/or designed primarily to help with marketing (purportedly effective) products as well as minimizing the extent of regulatory and legal exposure. We should not proceed, it would seem, as though the published scientific record is independent and presumptively reliable in the ways judges, like Kozinski, have confidently assumed.

pharmaceuticals, therapeutics and other consumer products as well as those responsible for environmental degradation.

The point here is not that Daubert or some combination of criteria captures (or could capture) the essence of (that illusory category) Science, but rather, it is time to recognize that there are a range of models of science and different standards of admissibility that might be applied according to the type of litigation, the seriousness of the issues at stake, the capabilities of parties as well as the kind of regulatory impact, restorative potential and accuracy we expect from the different parts of our legal and regulatory processes. These are salient issues in an age of deregulation and consensus regulation, where conflicted scientists participate on government expert committees responsible for policy advice, regulation and research funding.

In a provocative essay, Margaret Berger has suggested that we might even think about shifting burdens of proof, especially in relation to causation, in certain types of toxic tort cases, so that multinational corporations with resources and regulatory obligations might be required to (at least presumptively) convincingly demonstrate the safety of their products. This would certainly ease the burden on plaintiffs and absolve them from having to produce peer-reviewed and published epidemiological studies with statistically significant associations. Judges might even show sensitivity to the range of informal ways that modern corporations have been manipulating the published scientific and medical literatures. They could deem sponsored research—where a corporation, corporate-sponsored entity or collective of corporations exerted control or influence—as inadmissible or presumptively suspect.

The mere fact of publication should not be used as a surrogate for reliability—especially when the circumstances surrounding the funding, design and publication of the research might change how it is understood. Nor should the mere fact of publication be understood to confer the imprimatur of some nondescript and imaginary scientific community. If sponsorship systematically distorts research results, then to ignore this pervasive feature of modern scientific practice would seem to be sociologically naïve or worse. If the ICMJE is alarmed, then judges should respond.

Judges might develop rebuttable presumptions about public health research. Unless there are signed declarations that (published) research was not directly sponsored by corporations and that researchers do not have conflicts of interest, then the evidence might be deemed inadmissible. The risk of unwittingly treating partisan or sponsored research—that is, pre-litigation science for litigation—as independent or unaligned warrants the limited inconvenience of having to demonstrate the provenance of research by contacting authors to obtain statements about who did the

189. See Margaret A. Berger, Eliminating General Causation: Notes Towards a New Theory of Justice and Toxic Torts, 97 COLUM. L. REV. 2117, 2140-52 (1997); see also CARL CRANOR, TOXIC TORTS (2006) [hereinafter CRANOR, TOXIC TORTS].
work, details of contractual or funding arrangements, or other financial relations with defendants. Given the reforms in medical publication, these kinds of disclosures should become routine features of discovery. Published research with full disclosure should generally be admissible, although the meaning of this research could be vigorously challenged at trial. These kinds of expectations might actually encourage manufacturers to undertake new and more socially responsible forms of product research or fund research (more) remotely.

In addition, judges could develop adjectival doctrines which prevent the exclusion of expert evidence or grants of summary judgment where the defendant is intending to adduce—or has previously relied upon—types of evidence similar to those advanced by plaintiffs. We could develop new procedures such as admissibility estoppel where defendants are obliged to indicate if they intend to introduce similar types of evidence and similar methods to those routinely and aggressively denounced during pre-trial admissibility hearings. If a defendant sought to challenge the admissibility of a type of evidence—such as meta-analysis of epidemiological studies or physical inspection of tires—the defendant could be prevented from adducing evidence of that type if the case proceeds to trial. Civil defendants should not be encouraged to cynically exploit pre-trial procedures. Admissibility estoppel would not prevent credibility, methodological and interpretative challenges, but these would become issues for the trial and the jury. Any defendant would have a choice. Similarly, if serial defendants have relied upon a specific methodology or technique in other trials—such as the meta-analysis of epidemiological evidence or toxicological studies—then they should be prevented, in the absence of compelling justification, from contesting these types of evidence and approaches at their convenience (i.e. strategically) in other litigation.190

Less controversially, judges should be attentive to the entire evidentiary record assembled by both parties and should be careful about making admissibility decisions in isolation or in a piecemeal fashion. The majority assessment of the evidence in Joiner is a regrettable instance of this “corpuscular” tendency.191 Judges should consider the weight of evidence rather than quibble with isolated features or extrapolations from individual studies.192 Given the difficulties and costs of public health research, and given the inevitable limitations with epidemiological studies, judges should not simply dismiss plaintiffs’ evidence—even when comprised of animal and in vitro studies or toxicology—unless the defendant

190. This would be better than treating the evidence itself as some kind of binding precedent. See generally John Monahan & Laurens Walker, Scientific Authority: The Breast Implant Litigation and Beyond, 86 Va. L. Rev. 801 (2000).


192. See CRANOR, TOXIC TORTS, supra note 189, at 75-79.
can persuasively demonstrate that such evidence has little or no value. Judges should be thinking about the practical difficulties of proof as well as the evidence to be adduced by defendants and the extent to which defendants have exerted influence over research—even research published in respectable peer-reviewed journals—when making admissibility determinations.

With respect to defendants, judges might consider any failure (or omission) to undertake research or to organize research of sufficient quality and independence. The fact that industry, manufacturers and health care providers have legal and moral obligations to consumers and citizens, as well as the resources to undertake or fund high-quality (at arms-length) safety and efficacy research, should all be in the foreground of expectations and assessments. This should include assessments of the amount of reliable knowledge and the extent of uncertainty. Defendants ought not to be rewarded for inactivity, sponsoring research of poor quality or low power or fostering uncertainty, secrecy and even duplicity.

In criminal cases judges should be exceedingly reluctant to disallow expert evidence adduced by defendants. Unless the evidence has such limited probative value as to be close to irrelevant and/or universally de- rided as unreliable, judges should, on grounds of principle, be willing to admit it. Here, we could, perhaps against expectations, draw on a fragment of Kozinski’s judgment: “[T]he court and the parties are not limited to what is generally accepted; methods accepted by a minority in the scientific community may well be sufficient.”193 Rather than wade through the meanings attributable to peer review and publication or wrestle with methodological imbroglios, judges should probably use more sociologically-ori- ented heuristics like minority acceptance during admissibility decision-making. In the past, during the Frye era, experts’ claims about the field were frequently taken on trust. This might continue, although where there was disagreement, minority acceptance could be supported by reference to literatures or affidavits from other experts or even the evidence of sociologists, anthropologists and legal scholars studying areas of science and/or scientific controversy. Obviously, any claims could be challenged and the ipse dixit of the expert, or “fields” composed of only one or two tightly-knit individuals, might be treated with the skepticism they deserve.

For admissibility decision-making, minority acceptance is easier to administer and, appropriately, a less restrictive standard than aggressively ap-

193. Daubert v. Merrell Dow Pharm., Inc., 43 F.3d 1311, 1319 n.11 (9th Cir. 1995). Typically, Kozinski qualifies the more liberal possibilities of such a standard:

However, the party proffering the evidence must explain the expert’s methodology and demonstrate in some objectively verifiable way that the expert has both chosen a reliable scientific method and followed it faithfully. Of course, the fact that one party’s experts use a methodology accepted by only a minority of scientists would be a proper basis for impeachment at trial.

Id.
plied hard and supersized Daubert. It is also easier to ascertain the existence of a minority than whether some techniques, equipment or approaches have gained “general acceptance” in some field or among a majority of practitioners.\(^{194}\) The value of the evidence, its meaning in relation to the litigation and its limitations can all be developed during the course of the trial. This kind of approach is more consistent with centuries of legal practice and a continuing participatory role for citizens in the guise of the lay jury.

As for the state, there seem to be few convincing reasons why it should not be burdened with the most onerous standard in our entire legal system when adducing expert evidence against defendants in serious criminal trials. Maybe the state’s forensic scientists should have to pass the strictures imposed by hard Daubert or some similarly onerous standard more credibly linked to scientific practice—such as the general acceptance standard supplemented with Daubert-style criteria like established error rates. Acceptance alone may be inadequate because some of the techniques relied upon by state-employed technicians and scientists—like latent fingerprinting, ballistics, hair and explosives comparison and handwriting analysis—may have (or have had) acceptance among practitioners even though the extent of their reliability is unknown.

C. Simplification, Emphasis, Omission and Representation

One of the features of the whole science for litigation discourse is that evidence conspicuously developed or adapted for litigation tends to be stigmatized, but evidence developed in ways that can be displaced—through strategic representation—from the immediate legal context tends to be treated (by judges, at least) as independent and implicitly reliable. We have already seen how these kinds of commitments are inconsistent with modern commercial realities. But there are additional complexities with science for litigation. Focusing on whether evidence was developed for a particular case tends to overlook the tremendous amount of work that lawyers and experts (on all sides) need to undertake in order to translate expertise, studies, information and experience into a tractable legal cause of action.

There is an abundance of judicial commentary about the dangers of science for litigation but almost nothing written about how lawyers and experts should identify and translate vast amounts of formal and informal specialized knowledge and experience into the sui generis contours of a case. What makes selection, emphasis and omission appropriate or inappropriate? What ought to limit or shape simplification? Can knowledge remain stable as it moves between contexts? If lawyers select an expert

\(^{194}\) “Field” or “discipline” needs to be recognized as flexible, contingent and mutable. Obviously, there may be ongoing issues about what constitutes the appropriate field or set of relevant experts/practitioners. All standards will have these kinds of interpretative and classificatory issues.
who is among a minority or known to be inclined to a particular view, does the mere fact of selection make the expert partisan and their pre-disposition or pre-litigation orientation science for litigation? Issues of selection, translation, simplification and representation raise difficulties that endanger and blunt the utility of science for litigation. But the issues are more complex still.

Another important, but relatively unexplored, dimension of adversarial litigation is the fact that a party’s lawyers engage and brief the expert witnesses. This means that experts, particularly those who do not consult or appear regularly, tend to be at the mercy of the broader account of the case and the interpretation of the law supplied by lawyers. In addition, interactions between lawyers and experts are usually brief and sometimes cursory. Even well-intentioned lawyers can create difficulties for experts and judges if they base their selection of experts on strategic legal interpretations or fail to clearly convey some of the substantial rules and procedures to the experts.

In a recent empirical study of disputes over geographical indications for wine regions in Australia, I found that several expert geographers were criticized for producing science for litigation simply because they relied on the interpretation of a set of regulations preferred by the lawyers who had engaged them. The particular interpretation was inclusive—informed by the background to an international treaty and the domestic regulations which emerged in its aftermath. When the Tribunal eventually rejected this “inclusive” approach, the expert evidence—tightly coupled with the legal interpretation—was marginalized and able to be presented in a way that made the experts appear partisan. The fact that the geographers’ understanding of the regulations shaped the way they developed their evidence and drew a boundary for the particular wine region was effectively ignored. The Tribunal decoupled the evidence from the legal interpretation supporting it and vilified the experts on the basis that they had produced expert evidence which was inconsistent with what became the authoritative legal interpretation. Here, experts were criticized for providing partisan opinions even though their expert opinions were based on a particular interpretation of the law which was rejected by the Tribunal after the geographers had developed and presented their evidence.

Interestingly, when the Tribunal’s decision was reviewed during an appeal to the Federal Court of Australia on a matter of law, that court dismissed the Tribunal’s interpretation of the regulations. Perhaps predictably, the geographers’ credibility and opinions were not rehabilitated or even reconsidered in the light of the Federal Court’s revised interpretation. Vilifications are rarely reversed even when new legal interpretations might facilitate more favorable treatment and even the redemption of the

This example is particularly useful because it captures some of the diachronic complexity of law-science interactions as well as how ideas like science for litigation can be invoked—as they were by the Tribunal—to rhetorically manage dispreferred expertise. Marginalizing the geographers’ evidence as science for litigation meant that the Tribunal did not have to address the substance of their expert evidence in detail.

While the events in the previous paragraph are more likely to occur in circumstances where the substantive law is not particularly evolved or settled, the example illustrates the contingency and interpretive flexibility potentially available to lawyers, judges and experts. It also introduces temporal, spatial and interactive dimensions to our understanding of expertise. What counts as law and how to integrate law and evidence may change over time and space. It also suggests that because law-science interactions are “thick,” there is a need not just to consider the field(s), the studies and the information available, but these have to be refined and developed in a way that makes them (not just relevant but) strategically valuable and admissible.

Lawyers, experts and judges are constantly wrestling with expertise and law, their interactions and their integration. Here, “expertise” and “law” are simply labels for larger and more complex processes and assemblages of knowledge and practice. However, interpreting them as fundamentally separate domains is potentially misleading. Each shapes or influences how the other is developed, represented and understood. Moreover, moves or interpretations in either the law and/or the evidence can be decisive. Decision-makers, like the Tribunal in the previous example, are likely to resort to familiar tropes like “partisan,” “junk science” and “good science”—all features of the science for litigation worldview—rather than consider the complex institutional, professional and rhetorical dimensions of different kinds of expertise and how evidence might be modified to suit legal categories and audiences. Decision-makers are also unlikely to consider how legal and regulatory settings may actually shape scientific practice and the content of scientific knowledge.

The socio-economic significance of law and legal processes means that, whether judges like it or not and regardless of their level of awareness, legal activity encourages science for litigation and defensive scientific research in locations remote from (actual or anticipated) litigation. Legal categories, legal doctrine and the possibility of litigation all shape the content and reporting of much contemporary scientific research.


197. See Irwin & Michael, supra note 65.
D. Ethical Solutions?

Once we introduce more sophisticated models of science and expertise, questions of ethics and ethical sensibilities become potentially quite complex. Once we lose the power of a single efficacious method and prescriptive (or restrictive) norms, the meaning of expertise and the behavior of experts, lawyers and judges becomes not just messier but more intimately related.\(^{198}\) Knowledge claims and the value of knowledge become harder to extricate from specific contexts and particular purposes.

Rather than try to erect ethical obligations around synthetic models of expertise, however, we need to develop more critical ethical sensibilities which recognize the complexities and diversity of modern scientific practices as well as the goals of our civil and criminal justice systems. The complexity associated with different kinds of scientific practice and expert disagreement will not be resolved through the imposition of simplistic ethical duties and expectations. The example of conflicts of interest demonstrated how mandatory rules—let alone norms—did not guarantee compliance. Instead, the ICMJE simply banned some types of sponsored studies (e.g. reviews and cost-effectiveness assessments) where an author had a financial conflict of interest and required the formal registration of clinical trials before findings could even be considered for publication.

To reinforce the difficulties with ethical solutions, an example might help. In recent litigation in South Australia, an anthropologist (Fergie) who appeared on behalf of a group of indigenous people seeking to prevent the construction of a bridge, in what was allegedly a culturally-sensitive environment, was vilified in the popular press and the findings of a Royal Commission. The anthropologist was subsequently sued for negligence and for breaches of the Trade Practices Act (Cth)—for misleading and deceptive conduct in “trade and commerce”—by developers who alleged her expert report had delayed construction of the bridge and bankrupted the resort and residential estate they were building on Hindmarsh Island. Fergie was subsequently vindicated in the Federal Court of Australia, primarily because of technical legal issues pertaining to the ability of a third party (here the developers) to sue an expert who appeared for another party, in negligence, and whether an expert (here the anthropologist) who was consulting through her University’s commercialization arm was actually engaged in “trade and commerce” according to the way the Trade Practices Act had been interpreted.\(^{199}\)

\(^{198}\) See David Caudill, Legal Ethics and Scientific Testimony: In Defense of Manufacturing Uncertainty, Deconstructing Expertise, and Other Trial Strategies, 52 VILL. L. REV. 953 (2007); David Caudill, Ethical Dimensions of Law-Science Relations in U.S. Courtrooms, in EXPERTISE IN REGULATION AND LAW, supra note 50, at 184.

\(^{199}\) See Gary Edmond, Thick Decisions: Expertise, Advocacy and Reasonableness in the Federal Court of Australia, 74 OCEANIA 190 (2004). Interestingly, Justice von Doussa did not consider Fergie’s consulting work to undertaken in “trade or commerce.”
Fergie was originally approached in relation to an injunction to stop the construction of the bridge joining Hindmarsh Island to the mainland. At very short notice she was asked to write a report about secret-sacred knowledge (known colloquially as “secret women’s business”) held by a select group of Aboriginal female elders which was publicly disclosed just as construction on the bridge was scheduled to commence. While familiar with many of the issues and an experienced anthropologist, Fergie had not formally studied the local culture and history of the peoples involved. Like many areas of public health, knowledge of Australian Aboriginal society, tradition and history has many gaps. Without the anthropologist’s intervention, however, the women who claimed that connecting Hindmarsh Island to the mainland was culturally inappropriate would have been denied an effective voice and any decision-maker or judge—especially if male—would have been in a culturally and epistemologically awkward position. This raised the first of many ethical quandaries. Should Fergie have applied the (limited) expertise she possessed to these politically sensitive issues or taken the easier option of disengagement, knowing that the injunction and action against the bridge would probably fail?

Where this case becomes particularly illuminating is in relation to the conflicting ethical obligations and duties confronting the anthropologist. Fergie owed obligations or duties to the Court (in many Australian jurisdictions this duty has been formalized); the lawyers and the Aboriginal organization that retained and remunerated her; the custodians who had been obliged to disclose culturally sensitive knowledge; her profession; the University of Adelaide and its consulting company; a small group of dissentient Aboriginal women who contested the existence of the secret knowledge; her own public credibility and personal integrity, and so on.200 The judge hearing the case in the Federal Court indicated that in certain circumstances an expert like Fergie might even owe some obliga-

---

200. Many of these obligations could be developed in detail, but the responsibilities of a professional anthropologist are suggestive of the complexities. According to the Australian Anthropological Society’s Code of Ethics (2003), anthropologists have a range of responsibilities and duties. Some of the those relevant to the Hindmarsh Island Bridge litigation might include:

3.10 Anthropologists should not knowingly or avoidably allow information gained on a basis of trust and cooperation of the research participants to be used against their legitimate interests by hostile third parties.

4.2 Anthropologists should maintain integrity in the recording and presentation of anthropological data, and should not discredit the profession of anthropology by knowingly colouring or falsifying observations or interpretations, or making exaggerated or ill-founded assertions, in their professional writings, as expert witnesses, or as authors of any other form of reportage related to their work.

5.2 Anthropologists should not accept anthropological work which they are insufficiently qualified to do, whether by way of training or experience.

---
tion to the developers—that is, if she had been engaged in “trade and commerce” or their loss was causally related to a negligent expert report and was foreseeable. While this example may be somewhat unusual, it clearly demonstrates how an expert may have a range of competing and conflicting interests, obligations and duties which can become quite difficult to reconcile or manage even where experts are primarily concerned with assisting a court in good faith. As the wine regionalization example illustrated, the fact that interpretations of the law and/or evidence can change diachronically—especially as fresh evidence is adduced or a judge decides whether a paid consultant is engaged in “trade and commerce”—can make ethical obligations, legal assessments and expertise particularly unwieldy.201

It is no coincidence that ethical considerations bear many similarities to the earlier discussion of norms. In the absence of refinement or detail, ethical precepts are often insufficiently prescriptive to guide activity. The proper ethical course in a particular situation may be unclear and there may be a broad range of counter-ethics (i.e. alternative ethical stances) that might be credibly invoked to justify a range of inconsistent actions.

Ethics and ethical stipulations depend on our models of expertise, our epistemologies, and our broader social commitments. In consequence, different models of expertise and the recognition that science and expertise are fundamentally social might require more sophisticated responses.202 Once we realize how complex and subtle modern scientific practice and evidence can be, the attraction of ethical solutions tends to recede.

7.1 Anthropologists should take care to know of and generally understand the requirements of laws affecting their professional activity.

8. Responsibility to the wider public

Anthropologists also have responsibilities towards other members of the public and wider society. They depend upon the confidence of the public and they should in their work attempt to promote and preserve such confidence without exaggerating the accuracy or explanatory power of their findings.


201 Moreover, anthropologists tend to be sympathetic to the plight of indigenous Australians, especially at the hands of Anglo-Australian law and many may, for deep(er) ethical reasons, be willing to use legal processes (or research) instrumentally in order to address or correct past injustices. Such instrumental ethical stances may be comprehensible when we consider the massive over-representation of Aboriginal and Torres Strait islander peoples in Australian jails, their poor health, low life expectancies, and low standard of living. Along with historical relations, these ongoing existential issues, condition the construction of specialized anthropological knowledge and mediate access to subjects.

E. Political Origins: The Genealogy of Science for Litigation and Junk Science

This brings me to my final points—intended to place the emergence of the exclusionary ethos, skepticism toward science for litigation and hysteria about junk science into a broader socio-legal context. Once again Kozinski’s judgment is instructive. Consider the source of authority in the extracts below:

[6] That an expert testifies based on research he has conducted independent of the litigation provides important, objective proof that the research comports with the dictates of good science. See Peter W. Huber, Galileo’s Revenge: Junk Science in the Courtroom 206-09 (1991) (describing how the prevalent practice of expert-shopping leads to bad science).205

And,

[7] If the proffered expert testimony is not based on independent research, the party proffering it must come forward with other objective, verifiable evidence that the testimony is based on “scientifically valid principles.” One means of showing this is by proof that the research and analysis supporting the proffered conclusions have been subjected to normal scientific scrutiny through peer review and publication. [FN6] Huber, Galileo’s Revenge at 209 (suggesting that “[t]he ultimate test of [a scientific expert’s] integrity is her readiness to publish and be damned”).204

Science for litigation, along with the concept of “junk science”—which trades on the idea that there are simple ways to demarcate between good and bad science—were popularized in the years immediately before the Supreme Court’s Daubert decision, most notably by Peter Huber of the Manhattan Institute.205 The Institute is a New York-based think tank committed to economic choice, individual responsibility and pro-corporate law reform. It would not seem to be a coincidence that it is sponsored by some of the largest corporations in the United States, including Abbott Laboratories, Alcoa Inc., Exxon Mobil, General Motors, Johnson & Johnson, Pfizer Inc. and so on.

Throughout this essay I have explained how the images of science appearing in federal jurisprudence bear limited correspondence to the way the modern sciences are organized and practiced or the way the scien-

204. Id. at 1318. (italics added, footnote omitted).
205. After the success of Galileo’s Revenge, Foster and Huber sought to consolidate their work. See Kenneth Foster & Peter Huber, Judging Science: Scientific Knowledge and the Federal Courts (1997).
ences are treated in specialized scholarly literatures. This introduces a grand irony to this attempt to understand the origins of Daubert and science for litigation. Attempting to address a range of institutional and professional difficulties, like widespread impressions of a litigation crisis—allegedly fueled by science for litigation—and popular concerns about malfunctioning legal institutions, federal judges have themselves embraced, wait for it, images of science for litigation. Put simply, federal judges have embraced corporate-sponsored models of expertise—like “junk science,” science for litigation and the resurrection of Popperian falsification—and applied them in ways that reveal their anxieties, prejudices and institutional vulnerability.206

Unfortunately, the impacts of these developments have not been limited to the federal courts.207 One of the most disconcerting aspects of this whole episode is that judicial use of concepts like science for litigation actually encourages for-profit corporations to engage in the kinds of research practices which the leading medical and scientific journals (e.g. ICMJE) are trying to eliminate. The failure to register trials, disclose conflicts of interests or declare funding arrangements means that scientific and medical literatures and much of the evidence adduced by corporate defendants presents a misleading impression of its independence and reliability as well as the safety and efficacy of pharmaceuticals, therapeutic products, manufactured goods and foods.

By selecting different models of science and expertise, judges could help to improve the quality of medical and scientific publication and the safety of therapeutics and consumer products. More empirically grounded models of science and expertise might also allow plaintiffs back into the courts and make manufacturers more accountable for the safety and efficacy of their products and practices.

F. Downsizing Daubert: Recapturing Legal Legitimacy

Federal judges have adopted admissibility standards which systematically privilege large manufacturers and other for-profit collectives. These admissibility standards are predicated upon images of science and expertise developed and promoted by the very groups they are supposed to regulate. They are strategic ideas enlisted to help in a contemporary (and continuing) socio-legal struggle around responsibility for risk, damage and illness. Those with the greatest concentration of economic and epistemic power are effectively privileged, regardless of their own research practices or approaches to expertise, over individual plaintiffs and indigent criminal defendants. It is no coincidence that ideas like science for litigation are rarely invoked against defendants. In the end, science for litigation is a crude and unreliable means of evaluating expert knowledge. Applied in-

206. See Edmond, Just Truth, supra note 4.
207. Many state courts and legislatures have also embraced the exclusionary ethos.
sensitively, it is poorly suited to litigation. Moreover, substantial doctrine and constitutional guarantees should not be swept away or eviscerated by changes to adjectival law based on controvertible images of expertise, objectivity and ethical objectives.

More critical appraisals of modern scientific practice suggest the inappropriateness of the Daubert criteria and asymmetrical expressions of concerns about science for litigation. Contemporary jurisprudence minimizes the potential for law to enhance scientific research or discipline corporate malfeasance. Imagine having your tort action rejected on the basis that your expert evidence was science for litigation, or did not comply with Daubert, when the corporate defendant had suppressed or abandoned safety studies, sponsored studies designed to show commercially favorable or ambiguous results, and, if the case got to trial, was proposing to use similar kinds of studies and evidence to that which they uncompromisingly challenged at a pre-trial admissibility hearing.

What should judges do? Federal judges should begin to think about an exit strategy. They should be systematically downsizing Daubert. They should start winding-back the Daubert era by rejecting simplistic models of science and revising their anxiety about plaintiffs and civil juries. They should also begin to apply the Daubert criteria more flexibly and take a more expansive view of science for litigation that extends to the state and corporate-sponsored science. They should also reflect on the type of litigation and the rights, interests and responsibilities at stake. Judges should also consider the kinds of evidence and the resources available to the parties. This assessment should include the kinds of evidence that ought to be available if manufacturers were responsible corporate citizens. Manufacturers and polluters should not be allowed to resile from their continuing obligations to consumers and citizens or be rewarded for shifting the costs of their harms. Restrictive admissibility standards have prevented the testing of the evidence supporting drugs, therapeutics, chemicals and other products and the scientific and technical bases behind the state’s forensic techniques. Federal judges ought to consider how to facilitate expert disagreement in public contexts. As interested and conflicted scientists crowd our public advisory committees and expert panels, courts remain one of the few domains where modern knowledge and corporate conduct might be publicly contested and experts from a range of perspectives examined and held to exacting standards.

The challenge for the federal judiciary is to resist simplistic models of science and the institutional efficiencies they seem to confer by developing more critical and principled approaches which take account of the many (and sometimes competing) aims of our legal system. If judges are unable to identify more credible ways of identifying reliable evidence or managing evidence, then the answer may be to have fewer pre-trial admissibility hearings and more jury trials. Juries, it would seem, can do pretty well with expert evidence and very well when it comes to understanding
the social context of scientific research, corporate strategy and issues of malfeasance.208

In closing we can only wonder how many deserving plaintiffs have been left without remedy and how many innocent people accepted pleas or were convicted on the basis of corporate and government science for litigation.209


209. See Carl Cranor, Dual Legacy, supra note 191, at 122. According to Cranor: "The legitimacy of law as an institution is being threatened; legal decisions are vulnerable to challenge of being illegitimate, incomplete, or, too often, just wrong." See id.; see also Edmond, Just Truth, supra note 4.