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The Overlapping Magisteria of Law and Science: When Litigation and Science Collide

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The Overlapping Magisteria of Law and Science: When Litigation and Science Collide

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In the interest of full disclosure, particularly given the subject matter of the Article: I spend a small amount of time serving as defense counsel in pharmaceutical mass tort litigation, including the Baycol litigation referenced herein. This Article does not necessarily reflect the views of my clients, nor do my clients have any involvement (financial or otherwise) in my academic research.

Of course, any errors are mine alone, as are all views expressed. I can be contacted at wchilds@law.wnec.edu or at the "Torts Prof Blog," found at <http://lawprofessors.typepad.com/tortspref/>.

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"Science" plays a central role in litigation today. Civil litigants spend hours briefing and arguing the value of expert testimony. Studies performed years previously are taken apart and evaluated in detail. Criminal defendants are convicted or exonerated based on DNA evidence. And all of the evidence comes in through expert testimony, from witnesses who seek to explain the scientific issues to a lay jury, often in the form of opinion testimony.¹ This testimony can, and often does, dictate the outcome of cases.

Courts wrestle with the admissibility and use of scientific evidence every day, and have developed an evolving set of standards to apply to those decisions. They are seeking, at least in part, to have legal fact-finders consider only evidence that has already been deemed reliable in science—in other words, to have law follow science. The most recent iteration of the standards adopted to further those goals started, of course, with *Daubert v. Merrill Dow Pharmaceuticals*.² With that case and throughout the cases that followed, courts have sought to use science's paradigms of "reliability" as the standards of admissibility. If the scientific community has approved of the methods or conclusions (through peer-reviewed publication and other criteria), the courts conclude that those methods or conclusions should be admitted. The

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1. While I focus on "scientific" evidence, most of the Article is equally applicable to all forms of expert testimony that have a foundation in traditional academic research. I intentionally leave "science" undefined, especially given the fact that my arguments are not limited to any particular area of study. Additionally, creating a satisfactory definition would be difficult, to say the least. One that may be helpful for these purposes is the following: "Science is not an encyclopedic body of knowledge about the universe. Instead, it represents a *process* for proposing and refining theoretical explanations about the world that are subject to further testing and refinement." *Daubert v. Merrell Dow Pharms. (Daubert I)*, 509 U.S. 579, 590 (1993) (quoting Brief for American Association for the Advancement of Science et al. as Amici Curiae in Support of Respondent at 7–8).
 2. *Daubert I*, 509 U.S. 579. Many gallons of ink have been spilled on *Daubert* and its progeny, and I make no effort to summarize all of them here.

courts (and many commentators) believe that making legal standards track the perceived scientific standards improves the quality of fact-finding—that scientists' standards of reliability should be courts' standards of reliability.

In making this *Daubert* shift—more closely linking admissibility to scientific standards of reliability—the courts paid little attention, if any, to the potential impact making such a shift could have on science itself, or how it could affect the nature and quality of the scientific evidence being presented to fact-finders, usually a jury. In this Article, the first in a broader project in law and science, I explore two unexpected consequences of joining science and law at the hip, and consider whether these consequences represent reciprocal contamination, or instead cross-fertilization, of law and science.³

In the first unexpected consequence, the reliance (perhaps overreliance) on peer-reviewed publication in admissibility decisionmaking has resulted in aggressive litigation discovery into the peer-review process. Documents from the peer-review process have been subpoenaed and the participants in that process have been deposed, all with an eye to undermining the perceived value of the peer-review process, and, possibly, to purposefully deterring the future involvement in that very process by researchers and reviewers. This discovery was described in one instance as “harassment to silence independent research” and an effort to create “a chilling effect on folks who tell the truth.”⁴ Under this view, letting lawyers into the citadel of science in this way could weaken science’s ability to assist the legal fact-finding process and to assist society more generally, by creating disincentives for scholars to participate in the peer-review process or to be fully honest in that process. These disincentives may be created whether or not the litigants intend them.

In the second unexpected consequence, the focus on following science’s paradigms (again most notably peer review) has resulted in what can be called “litigation-driven scholarship.” Some expert witnesses have performed litigation-related research and essentially submitted their expert reports to peer-reviewed journals in what appears to be, at least in part, an attempt to bolster the likelihood of their testimony being admitted.⁵ These efforts may cheapen the value of

3. By “cross-fertilization” I refer to the agricultural concept (also called allogamy) of crossing a male from one species and a female from another. In the same way that crossing gametes in plants can create a disease-resistant plant or crossing an Angus bull with a Hereford cow can result in a faster-growing calf than a purebred calf of either species, I argue below that the interchange between law and science can strengthen both.

4. Jon Wiener, *Cancer, Chemicals and History: Companies Are Using New Tactics to Insure Their Misdeeds Aren’t Revealed in Court*, NATION, Feb. 7, 2005, at 19.

5. Conversely, and importantly, some researchers who first become scholars on a subject later are recruited as litigation experts. Of course, the lag time between

the paradigmatic reliability indicators in science and decrease the quality of scholarship presented to fact-finders (and to consumers of scholarship), reversing one core idea of the shift—that law should follow science, rather than the reverse. Many observers find such scholarship unseemly, if not outright untrustworthy, due to the obvious potential for bias.⁶

This Article explores the conflicts I conclude are inevitable in efforts to join law and science together. The two disciplines' magisteria⁷ overlap, but not precisely, and that lack of fit creates problems and opportunities for both. I conclude that it is far from self-evident that the "contamination" of science by law (e.g., lawyers meddling in peer review) or of law by science (e.g., litigation-driven scholarship showing up in litigation) necessarily weakens either science or law. Instead, they may serve as a check on each other and on other potential problems.

In fact, the reciprocal line-crossing—of litigators into the peer-review process, and of litigation experts into the peer-reviewed scholarship—may make both law and science stronger and provide a better understanding of both. A complete understanding and exploration of the peer-review process by litigants and the court would improve both law and science. Thus, rather than contamination, such interactions should be considered as cross-fertilization, strengthening both. The complex interplay between these overlapping and competing magisteria may cause discomfort and (hopefully transient) confusion, but it should be cautiously welcomed, not avoided.

I proceed in four parts. First, in Part I, I provide a brief review of the evolution of legal standards for the admissibility of what is termed "scientific evidence," including *Frye* and *Daubert* and their progeny. I also explore the stated and unstated goals of the shift from *Frye*'s fo-

research being completed and the review and publication process being completed can undermine either effort, but, especially in major litigation, the lag is not so great as to preclude it. See *infra* note 109 and accompanying text.

6. See, e.g., Peter F. Infante, *The Past Suppression of Industry Knowledge of the Toxicity of Benzene to Humans and Potential Bias in Future Benzene Research*, 12 INT'L J. OCCUPATIONAL & ENVTL. HEALTH 268 (2006) (criticizing industry research into benzene toxicity as biased). Of note, the author, Peter Infante, is a frequent litigation expert witness for plaintiffs in benzene litigation; that affiliation is not disclosed in the article. See Bette Hileman, *Daubert Rules Challenge Courts*, CHEMICAL & ENGINEERING NEWS, Jul. 7, 2003, at 14 (noting Infante's work as a paid expert).
7. "Magisteria" is a term for the teaching authority of a particular area of inquiry—"a word derived not from any concept of majesty or awe but from the different notion of teaching, for magister is Latin for 'teacher.'" Steven Jay Gould, *Non-overlapping Magisteria*, 106 NAT. HIST. 16, 19 (1997). One can think of magisteria as areas of authority—the relevant subject areas of disciplines. Gould used the term in an essay about the purported conflicts between evolution and religious belief, concluding that religion and science need not have overlapping magisteria and thus that religion and science can coexist. See *id.*

cus on the “general acceptance” of the expert’s approach to *Daubert*’s emphasis on the reliability and relevancy of the expert’s methodology.

Second, I give a (necessarily limited) overview of certain modern scientific paradigms of reliability. In Part II, I explore the realities of peer review⁸ and of other institutions in science that are relied upon in evidentiary determinations. Among other things, I explore the history of peer review in science and establish that only relatively recently did peer review become a central aspect of general scientific scholarship. Further, I conclude that the factors judges and litigators see as binary—for example, either the method is peer reviewed or it is not—are not nearly so simple to resolve. That Part, among other things, includes examples of exactly what peer reviewers are asked to do from various journals, duties that can vary significantly. Put more bluntly, there is peer review, and then there is *peer review*—and even careful and thorough peer review can miss fraud, as recent high-profile cases have shown. This conclusion will be important in evaluating whether the unexpected consequences of *Daubert* are problematic.

Third, in Part III, I describe two phenomena (summarized above) that are, to my knowledge, relatively new, and are almost certainly the direct result of courts’ emphasis on peer review as a criterion for the admission of expert testimony. The first phenomenon is discovery in litigation into the peer-review process—document and testimonial discovery exploring and challenging the peer-review process with the purpose of undermining the admissibility of testimony relying on the reviewed publication. The prospect of being put through such discovery may create incentives for scientists to refuse to participate in such a process; it may weaken the honesty of the comments provided in that process; it may be used to intimidate other potential experts; and it may impinge upon academic freedom. At the same time, it may provide valuable and relevant evidence of the reliability of the methods and conclusions in the publication and educate the courts about the nature of peer review.

The second phenomenon discussed in Part III is related: scholarship that appears to be produced (and submitted to peer-reviewed

8. Here, and throughout the Article, I focus on *editorial peer review*—that is, the review that takes place as part of publication decisions. The ongoing evaluation of scientific research by others, publications of challenging counterpoints, and so on all constitute what some commentators identify as “true peer review.” See Effie J. Chan, Note, *The “Brave New World” of Daubert: True Peer Review, Editorial Peer Review, and Scientific Validity*, 70 N.Y.U. L. Rev. 100, 100 (1995). Some courts, too, recognize this broader (and perhaps more important) concept of peer review. See *infra* note 39. Arguably, there are two levels of peer review prior to editorial peer review: what could be called “local peer review” (intra- and inter-institutional informal discussions, for example) and “editor peer review” (the first-cut determination by a journal editor). *Daubert*, however, put the focus on editorial peer review and the phenomena I discuss are in that area as well.

publications), at least in part, to increase the odds of the authors' opinions being admitted in litigation. This bootstrapping may result in published work that would not otherwise be published—and perhaps should not, under some standards, be published. And indeed, some research motivated by litigation may never be performed at all in the absence of litigation. Research may be published too soon (or sooner than it otherwise would have been) because the litigation schedule associated with the research is more time-pressured than the scientific community's. Science works incrementally; lawyers (especially those on a contingent fee) have every reason to work fast; statutes of limitations, the time value of money, and the ethical obligation to represent one's client zealously all work together to make a litigation timeline much shorter than a pure research timeline might be. This incentive to create scholarship may result in the publication of whole new areas of purported expertise that have not previously been published and that may not genuinely be proper subjects of expert testimony—scientists opining about the ethics of defendants, for example.

Finally, in Part IV, I explore the likely ultimate consequences of the development of discovery into peer review and litigation-driven scholarship and conclude that they are not so bad after all. The phenomena are especially acceptable if they both occur, as they work to balance each other; though I do not limit my conclusions to the situation in which both take place. In both contexts, I provide safeguards that courts, authors, and journals could adopt to mitigate the potential harm of both litigation-driven scholarship and peer-review discovery.

As for litigants taking discovery into the peer-review process, what is meant by "peer review" varies widely, and it is sensible to permit litigants to explore the quality of the peer review involved when its conclusions are being used against them. That process will better educate the courts about how much reliance to place on peer review as an indicator for reliability, much in the way that *Daubert* has educated judges about science and its methods more generally. Further, discovery into peer review can provide a rough analogue to what is called post-publication peer review, and that can only help, rather than hurt, the continued development of reliable knowledge in both science and law. The concerns about disincentives to participants in that process are not trivial, but they are relatively minor and can be mitigated relatively inexpensively and without undercutting the critical features of peer-reviewed scholarship. My conclusions are strongest in the context where the peer-reviewed author is acting as a litigation expert, but are not limited to that context.

Similarly, while there is something unseemly about articles published by litigation experts with an intention to assist the litigants, there are sufficient gatekeepers in place to conclude that such scholar-

ship is not inherently bad.⁹ Between peer review itself, strong disclosure requirements (which I contend should be made stronger, including a requirement that litigation-driven scholarship authors disclose their payments for litigation work), and the availability of discovery into the process, the potential negative impact of that scholarship is minimized. Further, the resources that litigants can bring to bear—especially in mass tort cases, which for economic reasons are the sort of cases where these events tend to arise—may result in important research being performed that otherwise would be performed later or never. The fact that litigants are funding such work is undoubtedly important in its evaluation (and, again, mitigates in favor of permitting discovery into the publication process), but that fact should not, by itself, make the scholarship either unpublishable in journals or inadmissible in court, especially when comparable bias is present in many articles that are admitted without a fight.

I. FROM *FRYE* TO *DAUBERT*, *KUMHO TIRE*, AND BEYOND: A FOCUS ON METHODS, NOT QUALIFICATIONS

It is nearly automatic for lawyers, judges, and commentators to blurt out “gatekeeper!” any time *Daubert* is mentioned. Indeed, the case emphasizes the importance of judges preventing unreliable expert testimony from being heard by the jury, and has by most accounts increased the scrutiny given to proffered expert testimony.¹⁰

Prior to *Daubert* (and still present in states in which *Daubert* or its equivalent has not been adopted), the prevailing standard was the “general acceptance” standard of *Frye v. United States*.¹¹ *Frye*, taking up less than two pages in the *Federal Reporter*, evaluated the admissibility of expert testimony regarding an early lie detector test in a criminal trial. The court, after briefly summarizing the theory underlying the test,¹² concluded that testimony based on that theory could not be admitted:

9. And the unseemliness may be no greater than when the research is performed specifically to support the approval and marketing of a drug, for instance. That said, my perception is that most attorneys and scientists are more uncomfortable with litigation-driven scholarship than with, for example, clinical trials sponsored by a drug maker, though that discomfort may be a function of a lack of familiarity. While research for approval or marketing purposes has existed for years, litigation-driven scholarship is clearly newer.

10. See, e.g., D. Alan Rudlin, *The Judge as Gatekeeper: What Hath Daubert-Joiner-Kumho Wrought?*, 29 PROD. SAFETY & LIAB. REP. (BNA) 329, 329–36 (2001). This more exacting examination has occurred despite the Court’s references to the opinion reflecting a “liberalizing” of standards. See *infra* note 15 and accompanying text.

11. 293 F. 1013 (D.C. Cir. 1923).

12. “[T]he theory seems to be that truth is spontaneous, and comes without conscious effort, while the utterance of a falsehood requires a conscious effort, which is reflected in the blood pressure.” *Id.* at 1014.

Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in this twilight zone the evidential force of the principle must be recognized, and while courts will go a long way in admitting expert testimony deduced from a well-recognized scientific principle or discovery, *the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs.*¹³

Thus, *Frye's* focus is on the general acceptance of the expert's principle by those in the expert's field of inquiry, along with the basic requirement that the expert be generally qualified to speak about the subject.

In 1993, the Supreme Court considered the proper restrictions on expert testimony given the adoption of the *Federal Rules of Evidence* [hereinafter *Federal Rules*], concluding first that the rules superseded *Frye*.¹⁴ In reaching that conclusion, the Court noted that *Frye's* "rigid 'general acceptance' requirement would be at odds with the 'liberal thrust' of the Federal Rules and their 'general approach of relaxing the traditional barriers to 'opinion' testimony.'"¹⁵ Parsing the language of Rule 702 of the *Federal Rules*, the Court concluded that the expert's testimony must be "scientific" and relate to "scientific knowledge," which "implies a grounding in the methods and procedures of science" and "connotes more than subjective belief or unsupported speculation."¹⁶

Based on that interpretation of the *Federal Rules*, *Daubert* focuses trial courts more precisely on the expert's methodology rather than the acceptance of the expert's more general field of work.¹⁷ Most important for purposes of this Article, *Daubert* enumerates nonexclusive criteria for consideration relevant to reliability: testability,¹⁸ peer review,¹⁹ error rate,²⁰ control standards, and (echoing *Frye*) general ac-

13. *Id.* (emphasis added).

14. *See Daubert I*, 509 U.S. 579, 585-89 (1993).

15. *Id.* at 588 (quoting *Beech Aircraft Corp. v. Rainey*, 488 U.S. 153, 169 (1988)).

16. *Id.* at 589-90. The case also addresses further the issues of qualification and fit, but those discussions are not directly relevant to the subject of this Article.

17. *See id.* at 592-93.

18. "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." *Id.* at 593.

19. "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." *Id.* at 594.

20. "[I]n the case of a particular scientific technique, the court ordinarily should consider the known or potential rate of error" *Id.*

ceptance.²¹ These factors were drawn in large part from an effort to define “scientific” in the ways that the Court believed scientists did.²²

The post-*Daubert* amendment to Rule 702 and its official comments expressly incorporated those criteria and added several more: whether the expert’s work is litigation-specific, whether the expert is unjustifiably extrapolating to an improper conclusion, whether the expert has accounted for alternative explanations, whether the expert’s work is as cautious in litigation as it would be in ordinary work, and whether the expert’s claimed field of expertise is known for reaching reliable results.²³ What “reliable” means in connection with methodology can vary, of course—a proper method to measure the temperature of a liquid is presumably a fairly precise question, while the proper method to evaluate the likelihood of user confusion in the face of a particular drug label may be more nebulous.

Since *Daubert*, the Supreme Court has established that the standard of review of district court decisions is abuse of discretion²⁴ and established that the *Daubert* approach is to be used in all expert testimony evaluation, not just what might traditionally be called “science.”²⁵

Consider for a moment the criteria adopted by *Daubert* and described in the comments to Rule 702, and take note of the wiggle room present in virtually every one:

- *Testability*: “whether the expert’s technique or theory can be or has been tested—that is, whether the expert’s theory can be challenged in some objective sense, or whether it is instead simply a subjective, conclusory approach that cannot reasonably be assessed for reliability”;²⁶
- *Peer review*: “whether the technique or theory has been subjected to peer review and publication”;²⁷
- *Error rate*: “the known or potential rate of error of the technique or theory when applied”;²⁸

21. “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’ . . . may properly be viewed with skepticism.” *Id.* (quoting *United States v. Downing*, 753 F.2d 1224, 1238 (3d Cir. 1985)).

22. *See id.* at 592–94.

23. *See* FED. R. EVID. 702. David Bernstein has argued that the adoption of Rule 702 superseded *Daubert*. *See, e.g.*, Posting of David Bernstein to the Volokh Conspiracy, http://volokh.com/posts/chain_1147021015.shtml (May 6, 2006, 09:29 PDT). Such a conclusion does not conflict with my arguments.

24. *See* *General Elec. Co. v. Joiner*, 522 U.S. 136 (1997).

25. *See* *Kumho Tire Co. v. Carmichael*, 526 U.S. 137 (1999).

26. FED. R. EVID. 702 advisory committee’s note to the 2000 amendment.

27. *Id.*

28. *Id.*

- *Control standards*: “the existence and maintenance of standards and controls”;²⁹
- *General acceptance*: “whether the technique or theory has been generally accepted in the scientific community”;³⁰
- *Litigation-based*: “Whether 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’”;³¹
- *Extrapolation*: “Whether the expert has unjustifiably extrapolated from an accepted premise to an unfounded conclusion.”³²
- *Alternative explanations*: “Whether the expert has adequately accounted for obvious alternative explanations.”³³
- *Level of care*: “Whether the expert is being as careful as he would be in his regular professional work outside his paid litigation consulting.”³⁴
- *Reliability of field*: “Whether the field of expertise claimed by the expert is known to reach reliable results for the type of opinion the expert would give.”³⁵

The only one of the bunch that at least seems binary—i.e., it is either there or it is not—is peer review. Its presence can be determined with a single yes-or-no question: “Has the methodology you are following been subjected to peer review?” (And, as discussed below, if the expert can say, “Yes, and my conclusion on this very issue has been peer reviewed too,” so much the better.) Given the apparent ease of evaluating it as a factor, litigants and courts tend to focus on peer review, if not to the exclusion of other factors, at least more heavily than *Daubert* and the drafters of Rule 702 might have intended.³⁶ Put

29. *Id.*

30. *Id.*

31. *Id.* (quoting *Daubert v. Merrell Dow Pharms., Inc.*, 43 F.3d 1311, 1317 (9th Cir. 1995)).

32. FED. R. EVID. 702 advisory committee’s note to the 2000 amendment (citing *General Elec. Co. v. Joiner*, 522 U.S. 136, 146 (1997)).

33. *Id.* (citing *Claar v. Burlington N. R.R.*, 29 F.3d 499 (9th Cir. 1994)).

34. *Id.* (quoting *Sheehan v. Daily Racing Form, Inc.*, 104 F.3d 940, 942 (7th Cir. 1997)).

35. *Id.* (citing *Kumho Tire Co. v. Carmichael*, 526 U.S. 137, 151 (1999)). This factor is relevant when the purported discipline itself lacks reliability—for example, “astrology or necromancy.” *Kumho Tire*, 526 U.S. at 151.

36. The *Daubert I* Court itself was rather cautious in describing peer review’s value, which suggests that courts that rely on peer review to the exclusion of other factors may do so contrary to the Court’s intentions.

Publication (which is but one element of peer review) is not a *sine qua non* of admissibility; it does not necessarily correlate with reliability, . . . and in some instances well-grounded but innovative theories will not have been published 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,’

another way, in considering the panoply of criteria in play, courts might be expected to latch onto peer review as an easy one to consider. And indeed, that is what has happened, at least in some cases.

One need go no further than the Ninth Circuit's consideration of the remanded *Daubert* case itself to see one of many examples of this sort of enthusiasm for peer review. Discussing the scenario where the proposed testimony is not derived from independent research, the court looked for "other objective, verifiable evidence that the testimony is based on 'scientifically valid principles.'" As an example of such evidence, the Ninth Circuit suggested that "[o]ne 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."³⁷

The court then drops a footnote: "We refer, of course, to publication in a generally-recognized scientific journal that conditions publication on a bona fide process of peer review. . . ."³⁸ The court continued:

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.³⁹

in part because it increases the likelihood that substantive flaws in methodology will be detected.

Daubert I, 509 U.S. 579, 593 (1993) (citations omitted).

37. *Daubert v. Merrell Dow Pharms., Inc. (Daubert II)*, 43 F.3d 1311, 1318 (9th Cir. 1995).

38. *Id.* at 1318 n.6.

39. *Id.* Similarly, the Northern District of Georgia has dismissed an unpublished study as having "little probative value" because it was "not subject to peer review." *Smith v. Ortho Pharm. Corp.*, 770 F. Supp. 1561, 1579 (N.D. Ga. 1991). See also, e.g., *Marsh v. W.R. Grace & Co.*, 80 F. App'x 883, 887-88 (4th Cir. 2003) ("Peer review helps to ensure that research papers are scientifically accurate, meet the standards of the scientific method, and are relevant to other scientists in the field [P]eer review involves scientists submitting a manuscript to a scientific publication in the field, journal editors soliciting critical reviews from other experts in the field and deciding whether the scientist has [generally] employed sound science."); *Brock v. Merrell Dow Pharms., Inc.*, 874 F.2d 307, 313 (5th Cir. 1989) ("While we do not hold that this failure [to be peer reviewed], in and of itself, renders his conclusions inadmissible, courts must nonetheless be especially skeptical of medical and other scientific evidence that has not been subjected to thorough peer review.").

Certainly many courts understand the complexity of peer review and recognize that publication peer review is far from a guarantee—or even necessarily a particularly strong indication—of reliability of either methodology or result, possibly to an extreme. See, e.g., *Valentine v. Pioneer Chlor Alkali Co., Inc.*, 921 F. Supp. 666 (D. Nev. 1996) (minimizing the value of editorial peer review); *State v. O'Key*, 321 Ore. 285, 304 (Or. 1995) (noting limitations of peer review). As to

It is therefore clear that whether or not the Court or the Advisory Committee intended it, peer review has become a central part of expert evaluation. Because of that centrality, litigants have done what we might have expected, and some of those actions make lawyers and scientists uncomfortable. Before addressing those consequences and the discomfort, a discussion of peer review as an indicator of scientific reliability is appropriate.

II. THE REALITIES OF PEER REVIEW

Judging from many judicial discussions of peer review, one could be forgiven for thinking that peer review dated to the earliest development of science, and that perhaps peer reviewers were excommunicated along with Galileo for approving the publication of his thoughts about the shape of the planet. But in fact, peer review in its current form is a relatively new concept and it is far from infallible.

Though peer review probably started in the mid-1700s,⁴⁰ as recently as the 1950s peer review was part of publishing in only some journals in some disciplines.⁴¹ Thus, for example, Crick and Watson's seminal article in *Nature* on the structure of DNA was not peer reviewed, not because (as various stories tell it) they did not want to tip off other researchers (in particular Linus Pauling) or because there was nobody competent to review it, but instead because *Nature*—then as now one of a handful of the undisputed leading journals in the world—simply had no peer-review process. As described in an annotation to a reprint of the paper,

Nature (founded in 1869)—and hundreds of other scientific journals—help push science forward by providing a venue for researchers to publish and debate findings. Today, journals also validate the quality of this research through a rigorous evaluation called peer review. Generally at least two scientists, selected by the journal's editors, judge the quality and originality of each paper, recommending whether or not it should be published.

Science publishing was a different game when Watson and Crick submitted this paper to *Nature*. With no formal review process at most journals, edi-

those courts, my later focus on the education of judges is presumably less relevant. But those courts, too, will be already more interested in the specific type of peer review with which they are faced, and thus less automatically skeptical of efforts to obtain discovery into the peer-review process.

While it is beyond the scope of this Article, significant parallels exist between the phenomenon in the context of litigation and similar incentives and responses in regulatory contexts, in particular in connection with the Endangered Species Act's requirement that conclusions be based on the "best scientific . . . information available." 16 U.S.C. § 1533(b)(1)(A) (2006). This requirement has led to a strong preference for peer-reviewed work and thus, predictably, scholarship designed to satisfy that very requirement.

40. See David A. Kronick, *Peer Review in 18th-Century Scientific Journalism*, 263 JAMA 1321 (1990).

41. See Chan, *supra* note 8, at 116.

tors usually reached their own decisions on submissions, seeking advice informally only when they were unfamiliar with a subject.⁴²

Today, the peer-review process varies widely. The most common form is as described above: two (or more) reviewers in a relevant field reviewing the article and advising the editors as to its value.⁴³ Of course, the reviewers do not themselves perform the experiments or primary research to confirm its results, nor do they ordinarily access the underlying data—the data presented are rarely the raw data; they have long since been analyzed.⁴⁴ They instead accept the data as presented by the authors and that the methods used matched the methods described, then decide whether the conclusions reached are appropriate and interesting in the relevant discipline, and whether the methods described were appropriate.⁴⁵

Sometimes the process varies. In a case explored in litigation (and described in more detail below), the reviewers were not anonymous (indeed, some were from one author's institution), and the review process took place in a room with the authors present and an active part of the discussion.⁴⁶ Such an approach is likely more common when the publisher has all but decided to publish the work and is using the peer-review process as more of an editing process than a selection process.

In a handful of cases, journals have begun to use "open peer review," where both the identity of the reviewers and the substance of their comments are open; not just to the authors, but to everyone.⁴⁷ This has the potential of mitigating biases, incentivizing thorough review, and reducing the possibility of discovery into the peer-review process discussed below.

42. Exploratorium—Unwinding DNA, Annotated Version of Watson & Crick's *A Structure for Deoxyribose Nucleic Acid*, <http://www.exploratorium.edu/origins/coldspring/ideas/printit.html> (last visited Jan. 5, 2007).

43. See Chan, *supra* note 8.

44. See *id.* at 120.

45. For a more detailed discussion of questions asked in the peer-review process by some journals, see JAMA Reviewer Instructions, *infra* note 52 and accompanying text.

46. See *infra* note 76 and accompanying text. This review process, as noted in the text, involved a book rather than an article, where the review process might properly differ; nonetheless, an opposing expert stated that it was unusual even in that context.

47. See, e.g., Biology Direct, <http://www.biology-direct.com/info/about/> ("Biology Direct's key aim is to provide authors and readers of research articles with an alternative to the traditional model of peer review. This includes making the author responsible for obtaining reviewers' reports via the journal's Editorial Board; making the peer-review process open rather than anonymous; and making the reviewers' reports public, thus increasing the responsibility of the referees and eliminating sources of abuse in the refereeing process."). Open peer review would be an option available to journals that seek to avoid discovery into the peer-review process. See *infra* note 125 and accompanying text.

Occasionally—possibly even frequently—authors suggest potential reviewers. Especially in particularly specialized subject matters, this may be a necessity in order to find qualified reviewers. Even if the authors do not suggest reviewers, the fields may be so small that the author can guess reviewers' identities with some accuracy.

No matter the structure of the process, the realities of the review itself can vary widely as well. Peer reviewers are typically active researchers themselves with limited time. One article might receive an hour of review, while another might receive days.⁴⁸

The reviewers themselves may have agendas in the field (simply by being active), and may well have a financial interest affected by the work.⁴⁹ With a growing proportion of research being privately funded, the odds of finding an entirely disinterested researcher—especially in narrow subject matters—may be slim. Even if the reviewer's interest is simply a disinclination to endorse an article that questions a view the reviewer has previously held, rather than a financial stake, it still presents potential bias.⁵⁰ And, of course, the reviewer may perceive the author as a competitor for funding even if the author's view is consistent with the reviewer's, so bias may exist whether or not the author and the reviewer agree.⁵¹

Precisely what a reviewer is asked to do varies by journal. The *Journal of the American Medical Association* (JAMA), for example, categorizes comments into seven areas of interest: "(1) Decision; (2) Priority; (3) Specific questions about the manuscript's quality; (4) Manuscript background information; (5) Remarks to the editor; (6) Remarks to the author; and (7) Conflict of interest statement for [the

48. Two studies indicate that the average is probably around two to three hours. See Alfred Yankauer, *Who Are the Peer Reviewers and How Much Do They Review?*, 263 JAMA 1338, 1339 (1990); Stephen Lock & Jane Smith, *What do Peer Reviewers Do?*, 263 JAMA 1341, 1342 (1990).

49. The interest could be based on a desire for ongoing grants or future employment as a clinical trial investigator, for example.

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 passion.

INT'L COMM. OF MED. JOURNAL EDITORS, UNIFORM REQUIREMENTS FOR MANUSCRIPTS SUBMITTED TO BIOMEDICAL JOURNALS: WRITING AND EDITING FOR BIOMEDICAL PUBLICATION 8 (2006), <http://www.icmje.org/icmje.pdf> [hereinafter UNIFORM REQUIREMENTS].

50. Such bias may, in fact, still be financial, but indirectly so, with concerns about challenges to the reviewer's views reducing available funding.

51. See UNIFORM REQUIREMENTS, *supra* note 49.

reviewer].”⁵² JAMA further cites another paper for advice to reviewers, which suggests, among other things, that the reviewer

should pay close attention to the methods section and understand what the authors did. Does the study follow a known study design? Did the authors follow the principles of this design? If you have specific references on methods or design that you think the authors should include, it is tremendously helpful to provide that information.⁵³

Nature instructs its reviewers (called referees) with the following primary instructions:

The ideal review should answer the following questions:

- Who will be interested in the paper, and why?
- What are the main claims of the paper and how significant are they?
- Is the paper likely to be one of the five most significant papers published in the discipline this year?
- How does the paper stand out from others in its field?
- Are the claims novel? If not, which published papers compromise novelty?
- Are the claims convincing? If not, what further evidence is needed?
- Are there other experiments or work that would strengthen the paper further?
- How much would further work improve it, and how difficult would this be? Would it take a long time?
- Are the claims appropriately discussed in the context of previous literature?
- If the manuscript is unacceptable, is the study sufficiently promising to encourage the authors to resubmit?
- If the manuscript is unacceptable but promising, what specific work is needed to make it acceptable?⁵⁴

The instructions continue:

[I]f time is available, it is extremely helpful to the editors if referees can advise on the following points:

- Is the manuscript clearly written?
- If not, how could it be made more clear or accessible to nonspecialists?
- Would readers outside the discipline benefit from a schematic of the main result to accompany publication?
- Could the manuscript be shortened?
- Should the authors be asked to provide supplementary methods or data to accompany the paper online?
- Have the authors done themselves justice without overselling their claims?
- Have they been fair in treating previous literature?
- Have they provided sufficient methodological detail that the experiments could be reproduced?
- Is the statistical analysis of the data sound, and does it conform to the journal's guidelines?

52. JAMA Reviewer Instructions, http://manuscripts.jama.com/cgi-bin/main.plex?form_type=display_rev_instructions (last visited Jan. 5, 2007).

53. Peter Cummings & Frederick P. Rivara, *Reviewing Manuscripts for Archives of Pediatrics & Adolescent Medicine*, 156 ARCHIVES PEDIATRICS & ADOLESCENT MED. 11, 12 (2002).

54. Authors and Referees @ Nature Publishing Group, Peer-review Policy, http://www.nature.com/authors/editorial_policies/peer_review.html (last visited Jan. 16, 2007).

- Are the reagents generally available?
- Are there any ethical concerns arising from the use of human or other animal subjects?⁵⁵

Note that the reviewers are expressly requested to discuss methodology only in the secondary, “[i]f time is available” set of questions. Presumably, of course, the publication’s editorial board can evaluate methodologies to some extent, especially given the editors’ backgrounds.⁵⁶ Further, problems with methodology could likely come through in response to some of the first set of questions, in particular in a discussion of whether the paper is convincing. But the relegation of methodology, at least expressly, to a second-tier question may be relevant to evaluating how well peer review can be used to evaluate a methodology for *Daubert* purposes, especially considering that *Daubert* is about *methodology* above all else.

The range of activities—all of which can accurately be called “peer review”—is such that entire conferences have taken place solely about peer review.⁵⁷ According to some commentators,

There are thousands of scientific and medical journals in the world . . . and many cannot fill their pages. The resulting seller’s market means that a researcher can publish even an inadequate article somewhere. Serious and adequate publication peer review remains relatively rare. Even adequate publication peer review is sometimes limited in that the review may involve only one or two peer reviewers, and even the best reviewers can only identify gross errors in methodology or conclusions.⁵⁸

All of this means, of course, that peer review is not a unitary concept, and it is not a universally reliable proxy for reliability; the fact that an article is peer reviewed is not remotely the final word on its accuracy and reliability.⁵⁹ One need go no further than recent headlines for evidence of this, as in the case of the Korean stem-cell researcher who published his work in none other than *Science*—work that turned out to be fraudulent.⁶⁰ When the stakes are high—as is

55. *Id.*

56. See Nature: International Weekly Journal of Science, About the Editors, <http://www.nature.com/nature/about/editors/index.html> (last visited Jan. 5, 2007).

57. See, e.g., Fourth International Congress on Peer Review in Biomedical Publication, http://www.ama-assn.org/public/peer/prc_program2001.htm (last visited Jan. 5, 2007). Extensive scholarship exists on the range of peer review, and I have merely attempted to summarize it. For a lengthier and more detailed summary, see William L. Anderson et al., *Daubert’s Backwash: Litigation-Generated Science*, 34 U. MICH. J.L. REFORM 619, 624 (2001).

58. See Anderson et al., *supra* note 57, at 624.

59. See Chan, *supra* note 8, at 127–29 (summarizing misconceptions about the reliability of conclusions published in peer-reviewed journals).

60. See Gina Kolata, *Amid Confusion, Journal Retracts Korean’s Stem Cell Paper*, N.Y. TIMES, Dec. 31, 2005, at A8; Nicholas Wade, *Clone Scientist Relied on Peers and Korean Pride*, N.Y. TIMES, Dec. 25, 2005, at A1. For a compilation of the original reports and the magazine’s reaction, see Science, Special Online Collection: Hwang et al. and Stem Cell Issues, <http://www.sciencemag.org/sciext/hwang2005/> (last visited Jan. 16, 2007).

true in cutting-edge research with millions of research dollars at stake, in drug development efforts where a negative study might stop development of a potential blockbuster medicine, or when millions of dollars in damages may be in play—the incentives are commensurately high for researchers to push the limits of accuracy in providing data. In such situations, peer reviewers may be unable or unwilling to test the assertions in a publication, and in fact are, by necessity, unable to evaluate underlying data.

III. THE RATIONALITY OF LITIGANTS AND EXPERTS

So far, we have seen two important facts about peer review: First, courts like it as a hook to rely upon in making decisions about the reliability (and thus admissibility) of expert testimony, and second, its reality varies tremendously. These two facts create opportunities for litigants discussed in this Part—opportunities that have been taken by unsurprisingly rational and zealous advocates.

First, it makes the litigants want, in some circumstances, to undercut the probative value of the fact of peer review. In the context of litigation, that means taking discovery and calling witnesses at trial. Litigants have subpoenaed and will continue to subpoena documents, not just from the litigation experts, but also from publications relied upon and the reviewers involved in the peer-review process. They also have deposed, and will continue to depose, those involved with the process, including editors and reviewers.

Second, it makes litigants seeking to admit expert testimony—especially expert testimony likely to be challenged as unreliable or novel—want to improve their chances by converting litigation research into peer-reviewed published research. I refer to these efforts as “litigation-driven scholarship.” As noted above, when an expert can cite not just a peer-reviewed article using the methodology in question but also to a peer-reviewed article reaching the very conclusions to which he proposes to testify, courts are likely to place some weight on that fact.

Both of these actions have engendered discomfort in both the scientific community and the courts along the lines of that described, in a different context, in a products liability suit against keyboard manufacturers in New Jersey based on the plaintiff’s carpal tunnel syndrome:

Science coexists uneasily with litigation’s adversary system, as the imperatives of partisan advocacy coupled with powerful economic incentives often seem to overwhelm good science. Lawyers, judges, and forensic experts sometimes engage in what literature teachers call willing suspension of disbelief. Scientific propositions that would cause even laymen to gasp in disbelief are

routinely argued in courts of law. Such are the dangers of a legal system allowing partisan expert testimony.⁶¹

Before addressing that discomfort, let us explore exactly what is happening.

A. Peering Behind the Peer-Review Curtain: Taking Discovery

What do I mean when I write of discovery into the peer-review process? It can take a number of forms, and in this section, I provide representative examples, starting with one that has received a fair amount of attention.

In the mid-1990s, a plaintiff's attorney in Louisiana contacted two well-credentialed historians, Gerald Markowitz of the City University of New York and David Rosner of Columbia University's Mailman School of Public Health, regarding the vinyl chloride monomer industry.⁶² Vinyl chloride is a product used in the production of, among other things, pipes and hoses.⁶³ At one point, plaintiffs' attorneys thought it might grow into a massive mass tort. To date, however, litigation has focused not on the end users but on individuals in the manufacturing process who develop a particular liver cancer.⁶⁴

An employee with that ailment retained the attorney who hired the historians.⁶⁵ In discovery, the defendants (representing a substantial portion of the industry) had produced hundreds of thousands of pages of documents, and the historians were initially asked to act as consulting experts in order to create a chronology of industry knowledge.⁶⁶ Their work was primarily focused on the activities of an industry group and not on any particular company. The historians concluded that the companies conspired to hide the dangers of the products.⁶⁷

The historians' research led to two relevant work products. The first was a largely well-regarded book, *Deceit and Denial: The Deadly*

61. Reiff v. Convergent Techs., 957 F. Supp. 573, 584 (D.N.J. 1997). Judge Irenas went on: "Almost a century ago, when the use of hired-gun scientific witnesses was less common and on shakier evidentiary grounds, Judge-to-be Learned Hand deplored the growing trend towards the use of partisan scientific opinion testimony, deeming it an 'evil in the present system.'" *Id.* at 584 n.16 (quoting Learned Hand, *Historical and Practical Considerations Regarding Expert Testimony*, 15 HARV. L. REV. 40, 55 (1901)). Obviously, the propriety of partisan experts is well beyond the scope of this Article, but the discomfort with that concept is similar to the discomfort with the phenomena discussed herein.

62. See Alexander Lane, *Tempestuous Times in the Ivory Tower*, NEWARK STAR-LEDGER, Dec. 5, 2004, at 1.

63. See *id.*

64. See *id.*

65. See *id.*

66. See *id.*

67. See *id.*

Politics of Industrial Pollution, published by the University of California Press and the Milbank Fund.⁶⁸ The book explored the lead and vinyl chloride industries. The book explores industry knowledge of dangers of the product and its manufacturing process, and the industry's response (or lack thereof) to those dangers. One chapter focuses on the vinyl chloride industry. As noted above, the authors concluded that the industry possessed and conspired to cover up material evidence regarding the dangers of its product. The material in the book relating to the vinyl chloride industry was based largely, if not entirely, on documents reviewed as the historians' work as paid consulting experts.⁶⁹

The second work product was an expert report (with only Professor Markowitz's signature) provided to the plaintiffs' attorneys in anticipation of deposition and trial testimony:

I have been asked to review the history of the vinyl chloride industry to understand and analyze what they [sic] discovered about the health hazards of vinyl chloride, what the industry's response was to information about vinyl chloride's toxicity, and what the industry did with the information and knowledge that they [sic] acquired.

Specifically, I was asked my opinion of whether the vinyl chloride industry forthrightly furnished information to the United States government and its responsible agencies, whether it forthrightly furnished information to the public, and whether it forthrightly furnished information to the people who worked in the vinyl industry's plants. It is my opinion that it did not.⁷⁰

68. GERALD MARKOWITZ & DAVID ROSNER, *DECEIT AND DENIAL: THE DEADLY POLITICS OF INDUSTRIAL POLLUTION* (2002). For a selection of book reviews, see DeceitandDenial.org Book Reviews, <http://www.deceitanddenial.org/reviews/> (last visited Jan. 5, 2007).

69. As I note below, this work could potentially be classified as "litigation-driven scholarship" as well as being an example of discovery into the peer-review process. It is not clear, however, that the historians' scholarly work was produced with an eye to litigation; at the time of the book's writing, it is possible that the authors were only retained as consulting witnesses. Additionally, there are significant questions about whether this subject is properly the basis of expert testimony at all, or if it is rather an effort to have a single well-credentialed witness telling the story of the opponent's bad documents. That, however, is beyond the scope of this Article and will be the subject of a future article. See generally Patricia Cohen, *History for Hire in Industry Lawsuits*, N.Y. TIMES, June 14, 2003, at B7 (noting disagreement among historians as to whether testifying in public health endangerment cases is appropriate). Also of note, Markowitz and Rosner have served as expert witnesses in lead-paint litigation as well. See Gerald Markowitz & David Rosner, *DeceitandDenial.org Purpose*, <http://www.deceitanddenial.org/purpose/> (last visited Jan. 5, 2007) (noting the authors' involvement in the Rhode Island nuisance lawsuit); see also Eric Tucker, *Public Health Expert Testifies in Lead Paint Case*, BOSTON GLOBE, Dec. 12, 2005, at A1 (describing Markowitz's testimony in a Massachusetts lead-paint case). I am unaware of any comparable discovery being taken in the lead-paint litigation.

70. Gerald Markowitz, *Expert Report on the Vinyl Chloride Industry* (May 19, 2004) (on file with author).

The report attached a 251-page chronology that provided substantially more detail on the actions than the expert report itself did.⁷¹ The chronology and report tracked *Deceit and Denial* fairly closely, and, based on the discovery sought, the defendants appear to have anticipated that the fact of *Deceit and Denial*'s publication and peer review would be important in the court's evaluation of Professor Markowitz's proffered testimony.⁷²

Attorneys for the defendants sought to take discovery into the process by which *Deceit and Denial* was published—particularly into the peer-review process. They served document subpoenas, seeking correspondence and the like, on the Milbank Memorial Fund and the University of California.⁷³ They also subpoenaed several participants in the process, including five of the eight actual reviewers, asking, among other things, how they were selected as reviewers and what the review included.⁷⁴ The specific results of that discovery, whatever they might have been, are publicly unknown; the case settled in February 2006 and the motions in limine regarding Markowitz did not address the methodology or otherwise disclose what was learned.⁷⁵

We do know that one of the defendants' experts, history professor Philip Scranton of Rutgers, opined that (among other things) Markowitz and Rosner acted unethically in participating in an "open" peer-review process (described earlier):

Markowitz . . . admitted that he knew the Milbank Foundation reviewers for [the book] and had coauthored publications with some of them. Indeed, not only did Markowitz join in selecting the manuscript's referees; he chose several faculty members from his own institution to evaluate the draft.

Such practices subverted confidential, objective refereeing of scholarly manuscripts (single- or double-blind), for this review was largely done "among friends."⁷⁶

The discovery into peer review (and Scranton's report) received national attention, including articles in *The Chronicle of Higher Educa-*

71. Gerald Markowitz and David Rosner, *Deceit and Denial Chronology* (2002), <http://www.deceitanddenial.org/docs/timeline.pdf>.

72. While articles and books may have different processes for peer review, in this instance the publisher did subject the book to a peer-review process, and I assume, I think reasonably, that if the reviewers had recommended rejection of the book, or recommended changes that the authors refused, publication would not have gone forward. Regardless of the differences, the issues in taking discovery into the processes are similar if not identical.

73. Lane, *supra* note 62.

74. *Id.*

75. Gulf Oil Corp.'s Motion in Limine to Exclude Plaintiffs' Historian, Gerald Markowitz, and For Other Relief, *Spann v. Airco, Inc.*, No. 3:02cv1645WS (S.D. Miss. Feb. 10, 2006).

76. Philip Scranton, Expert Report on Gerald Markowitz's Work (Aug. 3, 2004), <http://www.deceitanddenial.org/docs/Scranton.pdf>. I should note that I do not necessarily agree with Professor Scranton's views of Professor Markowitz's work.

tion⁷⁷ and *The Nation*.⁷⁸ Both included suggestions by the experts and others that the discovery was part of an effort to intimidate or silence industry critics—to scare future experts off of testifying contrary to the industry's wishes. In the *Chronicle of Higher Education* story, the head of the university press, Lynne Withey, is quoted as describing the discovery as “disturbing” and “pretty sleazy on [the defendants'] part.”⁷⁹ The historians created a substantial website presenting similar views, calling the actions “highly unusual, if unprecedented, intrusions into the academic peer-review process.”⁸⁰

Efforts to obtain discovery from the peer-review process are not, in fact, unprecedented. In one case, a court rejected a “scholar's privilege” when faced with discovery into the peer-review process.⁸¹ Similarly, in the breast implant litigation, the editor-in-chief of the *New England Journal of Medicine* was served with a subpoena seeking information about the publication of a relevant article.⁸²

The converse can take place as well, with the fact that an expert's theory failed the peer-review process becoming part of *Daubert* or other arguments. For example, in a case involving a purported epidemiological study⁸³ of illnesses near a Kerr McGee plant, the defendants obtained the comments of peer reviewers who recommended rejection of the study. Those comments were used to bolster the defendant's criticisms of the study:

The Plaintiffs' Report was *rejected* by the journal to which it was submitted. That journal, *Occupational and Environmental Medicine*, has provided [Kerr McGee] with the notes of the reviewers assigned to evaluate the Report. One reviewer lodged the same criticisms that [Kerr McGee] sets out in its motion and brief. Her first note raises the problem of selection bias, and questions the “internal validity” of the Report. She then asks whether researchers and interviewers were blinded to the status of the study subjects. They were

77. Lila Guterman, *Peer Reviewers are Subpoenaed in Cancer Lawsuit Against Chemical Companies*, CHRON. HIGHER EDUC., Nov. 19, 2004, at A20.

78. See Wiener, *supra* note 4.

79. Guterman, *supra* note 77. On the other hand, one of the peer reviewers, David Kotelchuck, said that apart from having received the subpoena at home at 11:45 p.m., he did not feel harassed by it. He said the industry “wishes to defend itself. . . . It's perfectly reasonable for them to want to speak to people who have some information about the evidence.” *Id.*

80. Markowitz & Rosner, *supra* note 71.

81. *Solarex Corp. v. Arco Solar, Inc.*, 121 F.R.D. 163 (E.D.N.Y. 1988).

82. MARCIA ANGELL, *SCIENCE ON TRIAL: THE CLASH OF MEDICAL EVIDENCE AND THE LAW IN THE BREAST IMPLANT CASE* 142–46 (1996).

83. According to the motion to exclude testimony based on the study, the researcher used as the “exposed” population a list of “plaintiffs or potential plaintiffs” provided to him by the plaintiffs' attorney, while the “control” group was recruited from church attendees. The participants were not blinded in any way, among other potentially problematic methodologies. Plaintiffs' Opposition to Defendants' Motion to Exclude Use or Reference to Plaintiffs' Report at 10, *Andrews v. Kerr-McGee Corp.*, 2001 WL 1704150 (N.D. Miss. Dec. 3, 2001) (No. 1:00CV158-D-A).

not. She wonders whether the questionnaires were designed specifically for this Report. They were not. Continuing, the reviewer points out that statements in the introduction and discussion "are not backed up by data in the study." "This," she says, "is not acceptable for publication." Finally, addressing the Report's conclusions, she writes: "The discussion makes conclusions that are not fully supported by the data. . . . *The poor internal validity of this study does not allow the final conclusions to be made . . .*"⁸⁴

I include the full discussion to point out that careful peer reviewers are often asking precisely the questions that a court applying *Daubert* should be applying:

- Is this a sound methodology?
- Are the conclusions supported by the data?
- Is it acceptable in the field?

And when peer review is done properly—as it appears to have been in the *Kerr McGee* case⁸⁵—it can be a powerful basis for either approval or rejection of the methodology in court as well as in publishing decisions, and is appropriately considered highly useful in determining the reliability of a methodology.⁸⁶ But when it is not done well, its status as an indicator for reliability is highly questionable.

Outside of the peer-review process, litigants sometimes seek discovery into underlying data—for example, data files or interview notes. Such discovery is most potentially concerning when the researcher involved has no connection to the litigation, and is less directly related to the *Daubert* process and is more related to an attempt to directly undercut or reevaluate the research.⁸⁷

If the specifics of the peer review in a particular case are potentially relevant to a court's evaluation of expert testimony, then why might there be a problem at all with taking discovery into that process? The details will come later, but for now, the core challenge to such discovery is a fear of intimidation and of chilling participation in the process, and of harm to academic freedom. Whether the litigants intend it or not, the specter of becoming involved in litigation—which

84. Memorandum of Law in Support of Motion to Exclude Plaintiffs' Report at 5–6, *Andrews v. Kerr-McGee Corp.*, 2001 WL 1704150 (N.D. Miss. Dec. 3, 2001) (No. 1:00CV158-D-A) (citations omitted).

85. Of course, the plaintiffs defended the study. *Id.*

86. Sometimes the peer reviewers reject scholarship because the scholar has failed to clearly explain the results, rather than any flaw with the results themselves. Assuming that the basis for the rejection is clear, such a rejection should have no impact on the *Daubert* analysis.

87. See generally Sheila Jasanoff, *Research Subpoenas and the Sociology of Knowledge*, 59 LAW & CONTEMP. PROBS. 95 (1996) (discussing knowledge areas in which judges need education on scientific practices to effectively evaluate subpoenaed research materials); see also Rebecca Emily Rapp, In re Cusumano and the Undue Burden of Using the Journalist Privilege as a Model for Protecting Researchers from Discovery, 29 J.L. & EDUC. 265 (2000) (discussing a First Circuit case, *In re Cusumano v. Microsoft*, 162 F.3d 708 (1st Cir. 1998), which extended the journalist privilege to third-party researchers).

is undeniably at minimum a hassle and at maximum highly disruptive and expensive—has the potential to make potential peer reviewers hesitate before agreeing to take part in the process, especially if the litigation removes anonymity that had previously been promised the reviewers. Further, if it is permitted on demand, without special standards or controls, litigants may indeed recognize and take the opportunity to chill the participation of skilled researchers and thus slow the development of science. Any approach to this issue must at least consider these concerns. Markowitz and Rosner themselves summarized the concerns well, describing the defendants' efforts as "attempts by the chemical industry to shut down discussion of their past and to interfere with the peer-review process, academic freedom and open debate."⁸⁸ They continue:

Should the peer review process be subject to industry-sponsored subpoenas? Should we worry about being brought in for depositions if we review a "controversial" book? Would we work on controversial subjects at all if industry lawyers are able to get at our notes, our documents and even our colleagues? Would we feel free to research these topics if we knew that industry can hire fellow historians to devote months to pick through footnotes and develop sophisticated (not sophisticated) arguments meant to undermine colleagues' professional credibility and integrity? Should we be concerned that reviewers may decide to avoid participating in the review process if they know that industry lawyers can later order them into court proceedings? These are certainly issues that should be discussed.⁸⁹

B. You Want Peer Review? We'll Get Peer Review: Litigation-Driven Scholarship

As courts' consideration of expert testimony becomes more focused on judging reliability based on the presence or absence of peer review of the principles, methodology, and conclusions, litigants (and their experts) have seen an opportunity to improve their chances by obtaining peer review themselves—often by submitting something very similar to the litigation expert report to a peer-reviewed journal in hopes of getting it published. Again, some examples may help.

Perhaps the most striking example came in litigation against DuPont relating to alleged birth defects resulting from fungicide exposure.⁹⁰ In that case, the plaintiffs' attorneys paid for a series of

88. Gerald Markowitz & David Rosner, *The Chemical Industry's Attack on Historians*, HIST. NEWS NETWORK (Dec. 6, 2004), <http://hnn.us/articles/8730.html>.

89. *Id.* In the context of discovery into uninvolved researchers, Jasanoff states that the efforts "may be motivated by both legitimate and illegitimate considerations." Jasanoff, *supra* note 87, at 97. "Litigants may try to use the research subpoena to intimidate and overburden researchers or to introduce irrelevant and confusing issues into the fact-finding process." *Id.* But they may also "expect to find genuine discrepancies between the 'facts' reported by scientists and the observations on which their findings were based." *Id.*

90. See Anderson et al., *supra* note 57, at 624–25.

studies, with one of the studies costing at least \$200,000.⁹¹ The paid expert “discussed two of the tests with the lead researcher (his colleague at the same university), provided samples of the fungicide, and assisted in the design and interpretation of the studies.”⁹² One study, which supported the assertions of the paid experts, was published in the peer-reviewed journal *NeuroToxicology* without any conflict disclosure (and none, evidently, was required).⁹³ The other studies were not submitted for publication.⁹⁴

An example not involving new studies relates to the anti-cholesterol drug Baycol (cerivastatin), which was voluntarily withdrawn from the market on August 8, 2001, by its manufacturer, Bayer Corporation.⁹⁵ Over 10,000 lawsuits, most of them alleging various muscle injuries, were eventually filed, with the federal multidistrict litigation eventually reaching the third largest federal multidistrict litigation to date. As has become common in mass tort litigation, attorneys for both sides retained experts in many areas, including the obvious subjects of general and specific causation, but also subjects such as medical and corporate ethics, with experts opining on the appropriateness of the company’s actions based on knowledge at various times.

The first two trials, both in early 2003, ended in defense verdicts (as did all trials to date). Later, three paid experts retained by plaintiffs’ attorneys in the litigation published an article in the *Journal of the American Medical Association* discussing Bayer’s actions in connection with the medication. The article largely tracked the opinions given as paid expert witnesses by the authors and disclosed their role as plaintiffs’ experts while stating that the article was based only on public information.⁹⁶ To date, those authors have not appeared in any Baycol trials, though they have been designated as experts since then.

91. *Id.* at 624 n.24.

92. *Id.* at 624.

93. *See id.* at 624–25; *see also* W.G. McLean et al., *The Effect of Benomyl on Neurite Outgrowth in Mouse NB2A and Human SH-SY5Y Neuroblastoma Cells In Vitro*, 19 *NEUROTOXICOLOGY* 629 (1998) (containing no reference to the authors’ involvement with related litigation).

94. *See* Anderson et al., *supra* note 57, at 624. As of the date of the Anderson article, the plaintiffs’ expert had been excluded in at least two matters. *See id.* at 624 n.21.

95. U.S. Food and Drug Administration, FDA Talk Paper, Bayer Voluntarily Withdraws Baycol (Aug. 8, 2001), <http://www.fda.gov/bbs/topics/ANSWERS/2001/ANS01095.html>. In private practice, I worked as one of Bayer’s attorneys in the Baycol litigation and continue to do a minimal amount of consulting work in the litigation.

96. Bruce M. Psaty et al., *Potential for Conflict of Interest in the Evaluation of Suspected Adverse Drug Reactions: Use of Cerivastatin and Risk of Rhabdomyolysis*, 292 *JAMA* 2622, 2630 (2004) [hereinafter Psaty et al., *Potential for Conflict*]. In the same issue, Bayer’s response (authored by Joseph D. Piorkowski, Jr., D.O., J.D., M.P.H., who served as Bayer’s counsel) to Psaty et al. was published, as was an alternative view authored by Brian L. Strom, M.D., M.P.H., of the University

The *Kerr McGee* case cited above is perhaps the most typical case of litigation-driven scholarship.⁹⁷ The report in question there was clearly created in connection with litigation—indeed, the “exposed” population was identified by the plaintiffs’ firm itself, obviously creating enormous potential for selection bias. If not for the fact of the retention of the experts, those studies would almost certainly not have occurred.

Litigation-driven scholarship is not, to be sure, limited to plaintiffs’ experts. A 1992 Ohio asbestos case discussed the admissibility of an article written by one of the defendant’s experts:

The article was written by defendant’s expert, Dr. Corn, with fifteen other scientists. Dr. Corn compiled and analyzed air sampling tests conducted in seventy-one school buildings across the country which contained asbestos materials. The expert concluded that the levels of asbestos fibers in the air inside the schools were not higher than the levels in air outside the schools, and, therefore no incremental health risk from asbestos existed in these buildings. The article was submitted for peer review before publication in the scientific journal [*Regulatory Toxicology and Pharmacology*].⁹⁸

The expert acknowledged that the data he used came from other court cases in which he was involved as an expert, but stated that he prepared the article as a researcher, not as an expert witness.⁹⁹ The court admitted the testimony.¹⁰⁰

This incentive to create scholarship may result in the publication of whole new areas of purported expertise that have not previously been published and that may not genuinely be properly the subject of expert testimony—scientists opining about the ethics of defendants, for example.¹⁰¹ The potential for bias is, of course, obvious:

of Pennsylvania (who had served as an expert witness for Bayer). Joseph D. Piorkowski, Jr., *Bayer’s Response to “Potential for Conflict of Interest in the Evaluation of Suspected Adverse Drug Reactions: Use of Cerivastatin and Risk of Rhabdomyolysis,”* 292 JAMA 2655, 2657 (2004); Brian L. Strom, *Potential for Conflict of Interest in the Evaluation of Suspected Adverse Drug Reactions*, 292 J. AM. MED. ASS’N 2643, 2645 (2004). The issue also included a *Journal of the American Medical Association* editorial on the subject and a reply from Psaty et al. Bruce M. Psaty et al., *Authors’ Reply to Bayer’s Response to “Potential for Conflict of Interest in the Evaluation of Suspected Adverse Drug Reactions: Use of Cerivastatin and Risk of Rhabdomyolysis,”* 292 JAMA 2658 (2004) [hereinafter Psaty et al., *Authors’ Reply*]. All authors’ involvement as counsel or experts was disclosed, though no authors disclosed the amounts they had been paid in the litigation. See Piorkowski, Jr., *supra*; Psaty et al., *Authors’ Reply, supra*, at 2659; Psaty et al., *Potential for Conflict, supra*; Strom, *supra*.

97. See *supra* note 83 and accompanying text.

98. *Worthington City Schs. v. Abco Insulation*, 616 N.E.2d 550, 553 (Ohio Ct. App. 1992).

99. See *id.*

100. See *id.*

101. The propriety of this sort of expert is beyond the scope of this Article. Briefly, though, my basic concern with such testimony is not *Daubert*-related as such, but instead has to do with whether the testimony aids the jury. Thus, for example, I

[T]he reliability and accuracy of litigation-based research is likely to be viewed with suspicion because of the potential bias arising from the source of funding for the research and the relationship between the researchers and the lawyers. Pressures resulting from the need to have the "right" outcome may result in manipulated procedures, distorted data, selective reporting of results, or even falsified outcomes.¹⁰²

It is no surprise, given the incentives created by *Daubert* and the *Federal Rules*' implementation of *Daubert*, that litigation-driven scholarship has developed.¹⁰³ Nor is it surprising, as noted earlier, that discovery into peer review is perhaps on the upswing. In the next Part, I consider whether either of these developments is necessarily problematic.

IV. HOW SCIENCE AND LAW CAN STRENGTHEN EACH OTHER

Expert witnesses are allowed to testify if they are able to, among other things, "assist the trier of fact to understand the evidence or to determine a fact in issue."¹⁰⁴ The goal of their testimony, therefore, must be to explain why a disputed allegation is either more or less likely to be true. In order to evaluate any testimony, a court or jury considers the basis of the testimony—the data on which it is based, to put it another way—and the reasonableness of the witness's conclusions.

This process—questioning the premises, the methods, the reasonableness—tracks in large part the process used in evaluating scientific research.¹⁰⁵ The post-publication evaluation of research, whether by

certainly can see the value of an attorney hiring a historian to prepare a chronology of documents and events that supports a particular interpretation of the facts of a case. But once that chronology is completed, I believe a jury can ordinarily reach the conclusions on its own, and that presenting a purported expert to tell the story is potentially improperly placing what should be an attorney's closing argument in the middle of the trial. As noted earlier, *see supra* note 69, I will address these issues in a future article.

102. Anderson et al., *supra* note 57, at 621.

103. Indeed, the Ninth Circuit (prior to the Supreme Court's *Daubert* decision) noted that a lack of peer review is often a problem with litigation-driven scholarship. *See Daubert v. Merrell Dow Pharms.*, 951 F.2d 1128, 1131 n.3 (9th Cir. 1991) ("Scientific studies conducted in anticipation of litigation must be scrutinized much more carefully than studies conducted in the normal course of scientific inquiry. This added dose of skepticism is warranted, in part, because studies generated especially for use in litigation are less likely to have been exposed to the normal peer review process, which is one of the hallmarks of reliable scientific investigation.").

104. FED. R. EVID. 702.

105. *See supra* Part II. I do not suggest that the litigation process and its adversarial nature are the same as peer review, only that there is substantial overlap. *Cf. SHEILA JASANOFF, SCIENCE AT THE BAR: LAW, SCIENCE, AND TECHNOLOGY IN AMERICA* 8 (Harvard Univ. Press 1995) ("[T]he cultures of law and science are in fact mutually constitutive in ways that have previously escaped systematic anal-

replicating it or building on it, is a critical part of the progress of science.¹⁰⁶ Indeed, this post-publication evaluation is probably more important than the publication peer review that courts (and this Article) focus on.

Such a searching examination of research is highly effective at eventually ferreting out bad research, and that conclusion is the premise of two key conclusions: First, science and law have numerous checks already in place to catch bad research, and so courts need not automatically reject litigation-driven scholarship. Second, those same checks are strengthened and complemented, especially in the context of litigation-driven scholarship, by permitting discovery into the peer-review process. I address each in turn.

A. Why Courts Should Not Exclude All Litigation-Driven Scholarship

Under *Daubert* and Rule 702, the trial judge makes a threshold determination of reliability leaving the ultimate conclusion of credibility and probative value to the jury. For the same reason that judges instruct juries that they are entitled to consider the financial interest of a witness in determining the witness's credibility, the fact that a particular item of research was performed and published by a paid litigation witness (whether paid by the plaintiff or defendant) is, of course, relevant to consideration of its credibility. The source of the funding is relevant just as the funding of clinical trials by pharmaceutical companies is relevant to the evaluation of those studies because the funding of research demonstrably can affect its outcomes.¹⁰⁷

To put it mildly, such bias likely exists in work performed by experts in litigation; it is highly unusual for a party's retained expert to testify contrary to the party's litigation position. Though experts are generally precluded from working for a contingent fee,¹⁰⁸ attorneys undoubtedly consider their success in obtaining verdicts in their favor in deciding whether to rehire experts, so even when publishing an article nominally separate from their litigation work, the authors of litigation-driven (or even merely litigation-related) scholarship do have a financial interest in the outcome of the research. The conflict of inter-

ysis. . . . [T]hese institutions *jointly* produce our social and scientific knowledge").

106. See Anderson et al., *supra* note 57, at 628–29.

107. See generally Frank Davidoff et al., *Sponsorship, Authorship, and Accountability*, 345 NEW ENG. J. MED. 825 (2001), available at <http://content.nejm.org/cgi/content/full/345/11/825>.

108. See, e.g., MODEL RULES OF PROF'L CONDUCT R. 3.4(b) cmt. 3 (2003) ("The common law rule in most jurisdictions is that it is improper to . . . pay an expert witness a contingent fee.").

est is at least as real as in the context where research is funded by, for example, a pharmaceutical company.

An additional reason to be skeptical of litigation-driven scholarship arises from the timelines of scientists as compared the timelines of lawyers. Science (and academic research more generally) tends to be incremental, where it can take years to develop a body of research that addresses a hypothesis.¹⁰⁹ The ongoing review of conclusions by colleagues and competitors takes time, as theories are considered, tested, and discarded, revised, or confirmed. Litigation necessarily moves faster: statutes of limitations run; witnesses forget, move, or die; plaintiffs want a recovery; defendants want a resolution; and attorneys want their fees, whether hourly or contingent. In most lawsuits, the expert's opinion (and thus, if relevant here, the litigation-driven research) is a one-time event, with at most minor changes as the case progresses. Even if the work receives publication peer review, it may not receive the ongoing review of peers (at least during the relevant litigation time period), and so it has not been genuinely tested.¹¹⁰

Further, litigants demand firm conclusions and an expert's commitment to those conclusions. For a testifying expert, "probably" is not a good answer very often. In contrast, the incremental nature of much scientific research requires hints, false starts, ideas that fail, and good guesses all along the process.

That said, just as medical journals and courts do not automatically reject studies paid for by drug sponsors or reject the first study in a particular area, the fact that research is done in the context of litigation does not mean it should automatically be excluded, either from journals or evidence in trial. Rule 702, indeed, provides that the litigation origin of research should be considered a factor, but only a factor, in evaluating expert testimony.¹¹¹ Many questions of interest to public health (or other important policies) may only be evaluated in the context of litigation. In cases where a product has been withdrawn or recalled, for example, there is no well-funded group other than attorneys who have an interest in learning more about the product. If paid litigation experts do not perform certain research, it may well not be done at all—and the financial interest in the outcome is not significantly different for attorneys than for sponsoring companies paying for studies of their products. This is likely the most obvious in mass tort litigation, where nobody besides attorneys has any incentive to fund ongoing research into withdrawn products; but that research may provide useful information for future products, and those attorneys can have hundreds of thousands of dollars available for perform-

109. See Anderson et al., *supra* note 57, at 630–31.

110. See *id.*

111. FED. R. EVID. 702 advisory committee's note to the 2000 amendment.

ing the research, whether it is original research or a post-hoc evaluation of a combination of clinical trials.¹¹²

Put briefly, we trust scholarly journals, and juries, to be able to evaluate the weight that should be given to the bias in financial support in other contexts. That trust should extend to judges' abilities to evaluate the reliability of methodology in a *Daubert* analysis and to juries' abilities to consider the relevancy of potential bias—at least enough to identify bias as one factor rather than a *per se* basis for exclusion. One can quite reasonably argue that we ought not be asking juries (or even judges) to be evaluating science at all; such an argument counsels in favor of systems of health courts and the like. But if judges and juries are to be evaluating science at all, they will be considering scholarship with the potential of bias, and we ought to trust them to consider that bias as well as they consider other biases.

Additionally, serious scholars have substantial reasons to self-regulate to avoid the scorn of colleagues or, perhaps more problematic, a published court decision declaring them unreliable or dishonest. Reputation is, in many respects, the coin of the realm in academia, though in some cases that coin may well be traded for the more literal coin of retention as a paid expert. While it obviously is not a panacea, danger to reputation (a danger that may threaten future expert retentions) helps balance the risks of accepting litigation-driven scholarship.

Finally, the availability of discovery into the peer-review process itself (discussed in the next section) helps mitigate the potential harm of litigation-driven scholarship. Attorneys can learn about numerous subjects potentially relevant to the *Daubert* decision (and to the fact-finder's evaluation of the expert's credibility):

- What was the nature of the peer review (anonymous, open, one reviewer, expertise of reviewer)?
- How long did the reviewers take to review the piece?
- Did the reviewers know that the authors had a financial interest in the outcome?
- Did the reviewers or editors have a financial interest in the outcome of the litigation?
- Were the reviewers acquainted or friendly with the author?
- Did the author exploit personal connections with the editors or reviewers?
- Is the journal or publisher involved well regarded?
- Do they have an interest in the outcome of the litigation?

Thus, especially with liberal discovery available to litigants to fully develop the potential biases or other problems with the research, litigation-driven scholarship should not be rejected strictly because it

112. See *supra* note 91 and accompanying text (discussing a case in which plaintiffs' attorneys spent \$200,000 on a single study).

comes out of litigation. That fact does not sufficiently distinguish it from other scholarship that is published and admitted daily in reputable journals and in courts.

B. Why Courts Should Allow Some Discovery into Peer Review

The potential value of litigation-driven scholarship is not terribly controversial; after all, most of the examples cited above were published, some in prestigious journals. The practice of litigants taking discovery into the peer-review process, however, appears more broadly disturbing. In this section, I argue that the concerns with such discovery, should lead us to regulate, not bar, the practice. Taking discovery into peer review offers an opportunity for litigants and courts to better evaluate the resulting scholarship and to know how much weight to put on the fact of peer review. Additionally, this discovery process has the salutary effect of educating judges about the realities of peer review, discussed above, giving them a better foundation for *Daubert* and related decisions. I address the reasons in the following three subsections.

1. The Possibility of Subpoenas and Depositions Helps Counter Bad Litigation-Driven Scholarship

As noted above, the availability of discovery into the peer-review process can provide real assistance in balancing the realities of litigation-driven scholarship. The adversarial system is not unique to law; it is in fact a critical part of science, and adding to it can strengthen it. As Nobel Laureate in Medicine Georg Von Békésy put it:

[One] way of dealing with errors is to have friends who are willing to spend the time necessary to carry out a critical examination of the experimental design beforehand and the results after the experiments have been completed. An even better way is to have an enemy. An enemy is willing to devote a vast amount of time and brain power to ferreting out errors both large and small, and this without any compensation. The trouble is that really capable enemies are scarce; most of them are only ordinary. Another trouble with enemies is that they sometimes develop into friends and lose a good deal of their zeal. It was in this way the writer lost his three best enemies. Everyone, not just scientists, need a few good enemies!¹¹³

113. Von Békésy's statement, originally at GEORG VON BÉKÉSY, *EXPERIMENTS IN HEARING* 8-9 (E.G. Wever ed. & trans., 1960), was quoted in George A. Olah's Nobel Lecture in 1994. See George A. Olah, Nobel Lecture, *My Search for Carbocations and Their Role in Chemistry* (Dec. 8, 1994), available at <http://nobelprize.org/chemistry/laureates/1994/olah-lecture.pdf>. This "enemy" concept likely has particular strength in the context of dramatic changes in a field, such as Crick and Watson's paper. If their assertions had been disproved, their careers would have been at least seriously injured, if not ended.

Except for the absence of compensation, someone who “is willing to devote a vast amount of time and brain power to ferreting out errors both large and small” sounds a lot like a devoted attorney preparing for a cross-examination. But attorneys can do even more than even “really capable enemies” in the sciences can do—lawyers can compel document production and testimony and find not just large and small errors, but flaws in the publication process itself.

Consider the following questions: How often would a competing researcher challenge the method used in peer review? How often would a competing researcher demand access to the underlying research, to the extent of wanting to (and being allowed to) reinterview subjects of a study? How often could they demand an opportunity to spend hours asking the peer reviewer about the review and obtain copies of the comments of the reviewers? How often could they bring the court’s power of subpoena to bear? How often would they have the money available to do those interviews and hire a consulting expert to painstakingly evaluate the work? The answer, of course, is “virtually never” to every one of the questions. As suggested by Von Békésy’s speech, science already is reliant on an adversarial system; allowing lawyers into it is an extension and expansion of the same. It may be uncomfortable, but it is not new.

Sheila Jasanoff, writing in the context of subpoenas to researchers who do not have a connection to the litigation in issue, disputes the ability of courts “to distinguish between valid and invalid scientific claims,” arguing that the expectation rests on “questionable assumptions about the practice of science.”¹¹⁴ Those assumptions are as set forth below:

- that evidence of fraud, error, and poor scientific practice can be detected unambiguously from written records;
- that challenged methodologies (including methods of analysis and interpretation) clearly conform or do not conform to “scientific” standards;
- that such standards preexist and hence can be mechanically applied during legal inquiry; and
- that the adversarial process is conducive to sorting out disputes concerning the validity and reliability of competing research practices.¹¹⁵

Jasanoff argues that those assumptions are inconsistent with, if not contradicted by, modern thought about the sociology of science.¹¹⁶

While this argument does counsel in favor of caution when allowing wide-ranging discovery into nonparty research, in fact it supports the availability of certain peer-review discovery because it will permit litigants to explore the very ambiguities in science that Jasanoff identifies, and because it will assist in educating the courts about the limitations of science. Those courts may well still make declara-

114. Jasanoff, *supra* note 87, at 97.

115. *Id.*

116. *See id.*

tions that suggest a belief in scientific certainty, but the more they learn about the scientific process, the more that claimed certainty will be a useful fiction rather than oversimplifying. Put another way, a necessary assumption of practicing law is that a fact can be established, while a foundation of science is that all purported facts can be challenged. Courts that expect final answers from science are being unrealistic, and opening the black box of peer review will help reveal the reality while permitting courts to do a better job of reaching final legal answers. Thus, rather than using a lack of understanding of science to rationalize keeping litigants and courts out of it, we should recognize peer-review discovery as a way to improve that understanding.

2. *The Possibility of Subpoenas and Depositions Provides Incentives to Do the Research and the Peer Review Correctly*

Researchers quite reasonably lament the possibility of being deposed.¹¹⁷ Depositions are unpleasant experiences, presumably even more so when being done with the aim of undercutting the validity of one's scholarly work. But having to face that possibility may create additional incentives to strive for better research and better peer review. Peer reviewers, properly, do not generally examine the underlying documents, data, etc. on which scholarly research is based, and so researchers may feel somewhat safe in using fudged data. When the stakes are sufficiently high, cross-examining attorneys will often do exactly what peer reviewers do not (as well as what they do).

Having such detailed scrutiny as even a remote possibility may decrease the likelihood of scholars using any sleight of hand. And that possibility becomes ever more remote when the research is genuinely above reproach. Similarly, if the peer-review process is demonstrably solid—demonstrable through correspondence and notes, which can be produced with relatively little difficulty—the likelihood of a deposition shrinks, as there is little potential value.¹¹⁸

117. See Wiener, *supra* note 4, at 21 ("Academics aren't used to being 'commanded' to do anything, and are unlikely to have attorneys of their own to accompany them to depositions."). In describing the depositions involved in *Deceit and Denial*, Wiener writes:

Cook's deposition took only an hour, but it was "an hour of battering and legal tricks, and the goal was to trip you up and get you confused," she said. "They kept asking me how long I had known Gerry Markowitz. I said, 'Are you asking if I had an affair?' They said, 'No, why are you asking that?' I said, 'Where I come from, that's the implication of your question.' They said, 'Where do you come from?'"

Id. I admit to being uncertain how asking how long a witness had known a proffered expert constitutes either battering or a legal trick.

118. This point may be moderately naïve. I am aware that many depositions are taken without any real hope of getting something useful. The percentage that fit

This point remains applicable whether or not the research in question is litigation-driven, or in fact whether or not the expert witness was involved in the research in question. If the expert is relying on peer-reviewed research that he did not perform, the opponents of the expert's testimony may well have a legitimate reason to explore the validity of that research, though, as I note below, the burden to show the propriety of the discovery may appropriately be greater.

3. *Discovery into the Peer-Review Process Opens the Black Box of Peer Review and Educates the Courts*

Some commentators have looked at the decade-plus of experience with *Daubert* and concluded that the shift in focus has had relatively minor effects on the outcome of cases, but, importantly, that it has educated judges about the realities of science.¹¹⁹ By providing judges factors to consider, the argument goes, judges will learn more about how those factors relate to reliability and how scientists think about the issues. Similarly, allowing discovery into peer-review processes will inform judges about the realities of those processes, and the many forms that those processes take, as described above.

In lay press coverage of the *Deceit and Denial* depositions, the reviewers and others focused on the perceived oddity of the attorneys asking the reviewers if they had checked all of the footnotes.¹²⁰ This focus misses the point. The point of the questions was almost certainly not to show that the reviewers failed in their job, but rather to point out the limitations of that job and thus of the endorsement, as suggested earlier. If the defendants could later show that those footnotes were not, in fact, accurate, then the reviewers' recommendation of publication could reasonably be called into question, not necessarily because they did not do their job, but because facts on which they (reasonably) relied were inaccurate.¹²¹ Such a showing would lead the courts involved to learn more about what peer reviewers actually do.

We have already seen that some judges have an oversimplified view of the realities of peer review. Through allowing discovery into peer review, judges will learn through seeing the outcome of that dis-

that description rises as the stakes rise. As noted below, I do have some suggestions to deter fishing expeditions, including the possibility of a judge appointing an expert to evaluate the request to take discovery into the peer-review process.

119. Edward K. Cheng & Albert H. Yoon, *Does Frye or Daubert Matter? A Study of Scientific Admissibility Standards*, 91 VA. L. REV. 471, 474 (2005).

120. See Wiener, *supra* note 4, at 21 ("The prevailing practice at university presses is that manuscript reviewers are not expected to check footnotes; Lynne Withey, director of the University of California Press, asked, 'How could you expect people to do that?'").

121. See, e.g., Jennifer Couzin, *Stem Cells: And How the Problems Eluded Peer Reviewers and Editors*, SCIENCE, Jan. 6, 2006, at 23 (Science's editor-in-chief Donald Kennedy says that "[p]eer review cannot detect [fraud] if it is artfully done").

covery that peer review is not binary, but instead a spectrum, and will be better able to evaluate scholarship that is presented to them as peer reviewed, and better able to determine its reliability and admissibility.

V. PROPOSALS FOR MITIGATING POTENTIAL HARM

The reasons I have suggested for developing a level of comfort with discovery into the peer-review process and with litigation-driven scholarship are all related, and they relate to the goals of *Daubert*, a recognition that law and science, while having substantial differences, have overlapping magisteria. "The net of science covers the empirical universe: what is it made of (fact) and why does it work this way (theory)."¹²² Similarly, law—as it relates to expert testimony—has to do with facts (what the expert tells the factfinder jury it should conclude) and theory (why the jury should do so). The relevant criteria for evaluating those asserted facts and theories should largely line up.

Scientists (theoretically) welcome challenges from sources within their magisterium, whether by way of competing publications, challenges in the peer-review process, revisions in the publication process, or otherwise. They should not automatically reject similar challenges from an overlapping magisterium, law, simply because they come from outside their home turf.

Of course, the complaints about discovery into peer review are not baseless. Depositions are unpleasant and expensive, complying with document requests is expensive, and lawyers are largely not scientists' favorite companions (and are, too, expensive).¹²³ Between the inconvenience and expense, the availability of such discovery may be a disincentive to participate in peer review, especially given the fact that most peer review is done as a professional obligation rather than for pay.

Before addressing some ways to mitigate that disincentive, a reality check may help. Even if peer-review discovery becomes much more widely adopted, depositions and document discovery will almost certainly take place in a very small percentage of instances of peer review. Most scientific scholarship is simply not that interesting to litigators, just like most legal scholarship is not that interesting to scientists (or, sadly, others!). Thus, the universe of cases in which litigation is even on the horizon is small, and that universe is likely identifiable in advance (e.g., articles about drugs, devices, toxic effects of industries, and a handful of other items, plus perhaps some patent-related scholarship). If a publisher chose not to accept for publication

122. Gould, *supra* note 7, at 19.

123. See Wiener, *supra* note 4, at 21 (noting the unpleasant manner in which peer reviewers have been deposed).

what I have described as litigation-driven scholarship,¹²⁴ whether to avoid hassle or because it concluded that the inherent biases militated against such scholarship, the universe would get still smaller.

Alternatively, a journal could switch to an open peer-review process, with the reviewers' comments and identities open to anyone, similarly reducing discovery exposure.¹²⁵ Such a switch would reduce both the likelihood and extent (and thus burden) of peer-review discovery.

Journals also should require the disclosure of litigation relationships. By their terms, at least some journals' disclosure policies do not require such disclosure at all.¹²⁶ In contrast, the *Federal Rules of Civil Procedure* require expert witnesses to disclose the compensation to be paid for their testimony.¹²⁷ In at least those cases involving litigation-driven scholarship, authors should be obligated to disclose not just the fact of their litigation work but also, at minimum, an estimate of the payments received. In an era of expert witnesses on all sides being paid up to \$500 or more an hour for work and testimony, consumers of that scholarship should receive at least the same information that a jury would receive.¹²⁸

Further, the number of individual cases that would go beyond document discovery to depositions is even smaller, because, if the documents indicate thorough and unbiased review, the attorneys are likely to conclude that a deposition is not worth it. The litigation and hassle exposure, in other words, would be small even if those involved in the

124. Though I think that litigation-driven scholarship is not inherently untrustworthy, a journal could rationally conclude otherwise, just as some have refused to accept articles funded by industry. If anything, I think litigation-driven scholarship is less trustworthy than industry-funded work (as the intended outcome is explicit in the former and less so in the latter), and so a policy rejecting litigation-driven work but accepting industry-funded work could be consistent.

125. See *supra* note 47 and accompanying text.

126. See Jeffrey M. Drazen & Gregory D. Curfman, *Financial Associations of Authors*, 346 NEW ENG. J. MED. 1901, 1901 (2002), available at <http://content.nejm.org/cgi/content/full/346/24/1901> ("Because the essence of reviews and editorials is selection and interpretation of the literature, the *Journal* expects that authors of such articles will not have any significant financial interest in a company (or its competitor) that makes a product discussed in the article."). "Significant" is defined in that article as \$10,000 or more in a year; insignificant interests are to be disclosed in the article. *Id.* Notably, the policy makes no mention of involvement in litigation, though the disclosure forms do note the relevance of expert work. See, e.g., Letter from The New England Journal of Medicine Editorial Staff to Potential Authors, Financial Disclosure & Authorship Form, <http://authors.nejm.org/misc/disclosRev.pdf> (last visited Jan. 5, 2007). The form asks only for a "brief description," not for dollar figures or the like. As noted above, even that is not always disclosed. See *supra* note 6.

127. FED. R. CIV. P. 26(a)(2)(B).

128. Such information may well be valuable information in work that is not litigation driven as well.

peer-review process had no protection beyond the ordinary protections in third-party discovery. This sort of discovery simply does not happen that often today and is unlikely to happen much more often in the future.¹²⁹

And yet there is legitimate concern, and therefore some ways to mitigate the risks may be valuable, both in the publication process and in the courts. As for reviewers and publishers, in those cases where litigation seems possible (or in all cases, to better spread costs), peer reviewers could demand that the publishers indemnify them for expenses associated with discovery, including the cost of counsel and the reviewers' time.¹³⁰ (Often, of course, the parties to litigation end up paying for the time of deponents, especially if the deponent is outside the scope of the subpoena power.) The additional costs to the publisher will be minimal at most, especially if its peer-review process is solid and well documented (which is a good thing to encourage in all events). The financial impact on the availability of qualified peer reviewers should be minimal, with those costs able to be spread by the publisher.

Yet peer-review discovery does create dangers not created by the average discovery process, and concerns about academic freedom and disincentives are not baseless. I therefore propose certain protections that balance those legitimate concerns against the litigants' legitimate interests in testing assertions.

When a party seeks discovery into research performed by a testifying expert and cited or relied upon by that expert—in other words, the party seeks drafts of the work, reviewers' comments in the expert's possession, editorial suggestions, and the like—that discovery should be granted as a matter of course under the ordinary discovery protections.¹³¹ Thus, if a party's discovery is not calculated to lead to the discovery of relevant materials, it can properly be quashed, but no special protection is necessary. The expert has, after all, submitted him-

129. A further reality check: While discovery into peer review is relatively new, research subpoenas have been around for at least a decade. See Jasanoff, *supra* note 87. I am aware of no evidence, even anecdotal, that any researchers or reviewers have in fact been deterred from getting involved in potentially litigation-related research. The absence of declarations about it is not proof of its actual absence, of course, but it is worth noting.

130. In fact, the book's co-publisher paid for the reviewers' legal fees in the depositions related to *Deceit and Denial*. See Wiener, *supra* note 4, at 21.

131. See, e.g., FED. R. CIV. P. 26(b)(1) ("Parties may obtain discovery regarding any matter, not privileged, that is relevant to the claim or defense of any party, including the existence, description, nature, custody, condition, and location of any books, documents, or other tangible things and the identity and location of persons having knowledge of any discoverable matter. For good cause, the court may order discovery of any matter relevant to the subject matter involved in the action. Relevant information need not be admissible at the trial if the discovery appears reasonably calculated to lead to the discovery of admissible evidence.").

self to the process voluntarily and the courts' general protections against harassment are sufficient.

When the party seeks to go further and take discovery into the peer-review process, courts should follow two additional steps. First, limited document discovery, including document subpoenas, limited interrogatories, and similar methods, should be permitted, again under the same processes used for ordinary third-party discovery. The potential for intimidation and inconvenience is relatively small in this context and can be mitigated, as noted above, through indemnification agreements. The identities of the peer reviewers, if previously anonymous, could be redacted at this stage in the process as well.

If, upon the completion of this limited document discovery, the party wishes to take the depositions of individuals not involved directly in the litigation or to take further document discovery, including seeking the names of the reviewers, the court should require a showing that the party has a reasonable basis to suspect that such discovery will be pertinent to the litigation. Such a showing might include a showing that the reviewer was not informed about the fact that the author was paid by attorneys involved in the litigation (if the scholarship is litigation-related), an indication to suspect that the reviewer was either lax or not disinterested, or other arguments that the process did not work as expected, such that the court should discount or disregard the peer-review factor under *Daubert*. In the event that the party opposing discovery can show a need, courts could use protective orders to maintain the anonymity of the reviewers, at least for the time being.

As part of this evaluative process, a court may find it beneficial to have an expert who works for the court.¹³² The expert could be chosen by agreement of the parties, with the party seeking discovery responsible for his fees, and would be there essentially as a reality check for the judge. Thus, the expert might be a researcher from a local university who could provide some perspective on whether the peer-review process in question deviated significantly from the ordinary process and whether a deposition was likely to provide something useful, rather than being harassment.

If these safeguards are adopted with an eye to the potential harms they seek to prevent, researchers will be protected from unnecessary or harassing research, while parties will not have material evidence blocked from view.

132. Cf. William Schulz, *Judging Science*, CHEMICAL & ENGINEERING NEWS, Feb. 27, 2006, at 36–39 (noting proposals for court-appointed experts).

VI. CONCLUSION

Daubert and the *Federal Rules of Evidence* have, commendably, sought to incorporate more of the reliability criteria from science into courts' evaluation of expert testimony. This incorporation recognizes the overlapping magisteria of law and science. Law and science alike rely on testing and challenges to pursue reliable evidence-based conclusions. While the respective disciplines obviously have many differences, the bases on which they reach conclusions about the reliability of methodologies and conclusions are strikingly similar.

These similarities have, as described above, led to both litigation-driven scholarship and to efforts to take discovery into the process of obtaining publication in scientific literature, in particular the peer-review process. These two phenomena should not be surprising in light of *Daubert* and, as I have argued above, should not be rejected as necessarily negative for either law or science.

Litigation-driven scholarship, while subject to many caveats, is not necessarily any less trustworthy than scholarship driven by those who fund it, whether that means pharmaceutical companies, government agencies, or universities spending discretionary grant money. The source of funding for studies, and the reasons for doing those studies in the first place, are all considered relevant but not dispositive in other contexts. No reason exists to treat the litigation relation of scholarship differently.

Discovery into peer review has the potential to strengthen both law and science. As for strengthening law, judges will better understand the peer-review process once they have seen the results of this discovery, and will understand the truly complex and varied activities that all fall within the umbrella term "peer review." More importantly, the information received through that discovery is directly relevant to the *Daubert* evaluation and will better filter expert testimony. As for what science gets out of it, the possibility, even remote, of litigation discovery will create incentives for authors and reviewers to be careful, especially when they are authoring or reviewing litigation-driven scholarship (where such discovery is most likely).

Allowing for the cross-fertilization of law and science in this context, then, will help both and, if appropriate protective measures are used, will hurt neither. As in all aspects of litigation, courts must be vigilant to avoid harassment or intimidation, especially with the additional concerns of academic freedom. But those concerns are not so great as to justify denying access to highly relevant materials. Similarly, concerns about the biases inherent in litigation-driven scholarship are not so great as to justify ignoring potentially important scholarship. With proper care, the overlapping magisteria of law and science can provide additional support for each other.