What judges should know about the sociology of science

Daubert v. Merrell Dow Pharmaceuticals *gave judges increased responsibility for reviewing scientific evidence. Some perspectives from the sociology of science provide guidance for fulfilling this obligation.*

by Sheila Jasanoff

In *Daubert v. Merrell Dow Pharmaceuticals,*[1] the Supreme Court firmly rejected the *Frye* test, which had dominated judicial thinking about the admissibility of novel scientific evidence for 70 years. Judges need no longer turn to science to determine whether an expert's opinion has "gained general acceptance in the particular field in which it belongs."[2] Instead, the Court proposed a two-pronged assessment of "whether the reasoning or methodology underlying the testimony is scientifically valid and of whether that reasoning or methodology properly can be applied to the facts in issue."[3]

While *Daubert* seemingly gives judges more discretion than *Frye* did, it also encourages them, as some commentators have already observed, to "think like scientists." Justice Harry Blackmun, writing for the majority, was unwilling to set out a "definitive checklist" for use in reviewing scientific evidence. Yet a determination of "scientific validity" clearly must be central to such inquiry. Where should judges turn for further guidance on how to meet this obligation?

Perspectives from the sociology of science can be applied usefully to the review of scientific evidence in the wake of *Daubert.* It is beyond this article's scope to present the issues in more than outline form, but the approach proposed here promotes deeper reflection about how far the courts can go in determining what constitutes legitimate science.

The practice of science

In recent years, critical studies of science have increasingly focused on the way scientists carry out their work in practice. Investigations into the social structure and operation of science have revealed a picture of scientific knowledge that is distant from the logically coherent but highly abstract accounts constructed by philosophers of science. This new, and in many ways disconcerting, picture of science has particular relevance for the law, because what is at issue in most legal proceedings is precisely the social dimension of science: the matrix of social practices, conventions, institutions, and interests that sustains scientific progress and gives legitimacy to particular scientific "facts."

Below is an abbreviated and highly simplified overview of findings from the sociology of science; these ideas are subsequently brought to bear on some well-known patterns of legal controversy.

Social construction. The most significant insight that has emerged from sociological studies of science in the past 15 years is the view that science is *socially constructed.* According to a persuasive body of work, the "facts" that scientists present to the rest of the world are not direct reflections of nature; rather, these "facts" are produced by human agency through the institutions and processes of science, and hence they invariably contain a social component.[4] Facts, in other words, are more than merely raw observations made by scientists exploring the mysteries of nature. Observations achieve the status of "facts" only if they are produced in accordance with prior agreements about the rightness of particular theories, experimental methods, instrumentation techniques, validation procedures, review processes, and the

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1. 61 U.S. L.W. 4805 (U.S. June 28, 1993).
2. Frye v. United States, 293 F. 1013, 1014 (D.C.Cir. 1923),
3. 61 U.S. L.W. 4805 (U.S. June 28, 1993).
4. *See, e.g.,* Latour and Woolgar, LABORATORY LIFE: THE CONSTRUCTION OF SCIENTIFIC FACTS (1986).

like. These agreements, in turn, are socially derived through continual negotiation and renegotiation among relevant bodies of scientists.

The process of constructing scientific facts normally takes place within familiar scientific institutions such as the laboratory, the specialist journal, the disciplinary society, or the "invisible college"[5] of experts in a given field. At times, however, non-scientific institutions are drawn into the construction of science, such as when a television program publicizes the risks of a pesticide or a court adjudicates the validity of an epidemiological study that has never been published in the peer-reviewed literature. In these cases, what finally counts as "science" is influenced not only by the consensus views of scientists, but also by society's culturally conditioned views of how things work in nature.[6]

Contingency. From a sociological viewpoint, scientific claims are never absolutely true but are always *contingent* on such factors as the experimental or interpretive conventions that have been agreed to within relevant scientific communities. The contingency of scientific facts refers to their dependence on certain background features necessary for their production. In their normal professional interactions, scientists tend to downplay even those contingencies of which they are aware, and they tend to speak of facts as if they were objectively true. When scientific controversies erupt, however, disputing parties regularly focus on the contingencies in each other's accounts of reality. As noted by sociologists Nigel Gilbert and Michael Mulkay, the objective "empiricist repertoire" of normal scientific discourse is replaced in these instances by a more subjective "contingent repertoire" that stresses the indeterminacy of many alleged facts.[7]

Inscription. The noted French sociologist Bruno Latour has called attention to the fact that science as we know it often takes the form of written texts or *inscriptions,* such as a curve on graph paper, a scattering of dots on photographic film, or an X-ray picture that looks like a supermarket bar code. The inscription (and, more generally, its translation into numbers) is regarded as having a direct relationship to the observed substance of science, although extremely sophisticated instruments and practices may in fact underlie its production. Scientific debate generally takes the inscriptions that are reproduced in published articles as the starting point for discussing natural phenomena. The inscription is a substitute for reality, while "the intervening material activity and all aspects of what is often a prolonged and costly process are bracketed off in discussions about what the figure means."[8]

Deconstruction. For sociologists of science, *deconstruction* means nothing more arcane than the pulling apart of socially constructed facts during a controversy. That facts should lend themselves to deconstruction is a corollary of their original construction. The adversarial structure of litigation is particularly conducive to the deconstruction of scientific facts, since it provides parties both the incentive (winning the lawsuit) and the formal means (cross-examination) for bringing out the contingencies in their opponents' arguments.

Experimenters' regress. Harry Collins, a leading British sociologist of science, has observed that the deconstruction of controversial scientific claims commonly follows a pattern called *experimenters' regress.* Experiments, as Collins notes, are always matters of skillful practice, so that "it can never be clear whether a second experiment has been done sufficiently well to count as a check on the results of a first."[9] When scientists wish to contradict each other's findings (as routinely happens in legal proceedings), the indeterminacy of experimentation provides a natural pathway of attack: Were the instruments properly calibrated? Were background conditions stably maintained? Was the experiment adequately controlled? Were the resulting inscriptions correctly interpreted? Was there a valid statistical analysis of the data? There is virtually no limit to the questions that can be asked about experiments as long as scientists have an interest in challenging one another's observations. A consensus develops around particular scientific theories, methods, and claims only when the incentives for attacking them disappear. Claims that no scientist any longer wishes to challenge or unpack are said to be "black boxed"; such claims constitute the expanding and, for the most part, invulnerable core of scientific knowledge.

Boundary work. The stability of science, according to the sociological view, depends upon negotiated agreements within a research community about a host of issues ranging from the applicable theoretical paradigm to norms of peer review and publication. To maintain the stability of its findings, a community of scientists has to be relatively resistant to criticism from outsiders. Solid state physicists, for example, will only brook criticism from other solid state physicists, just as epidemiologists will reject interventions by experts who have no formal training in epidemiology. Studies in the sociology of science have shown that scientists maintain the purity of their communities through what is termed *boundary work.* People whose criticism the community does not wish to accept are dismissed as members of a different field or, if circumstances demand, as misfits, deviants, charlatans, or outsiders to the enterprise of science.[10] Effective boundary drawing insulates scientific work from unexpected and possibly ill-motivated challenge by inadequately credentialed critics. Boundary work is in this sense an indispensable part of the ordinary practice of science, but the boundaries drawn by scientists can be used to deflect meritorious as well as unjustified criticism.

Applications to the law

The foregoing model of scientific practice provides a useful starting point for explicating scientific controversies, whether they arise at the labo-

5. For a definition of this term, see Crane, INVISIBLE COLLEGES: DIFFUSION OF KNOWLEDGE IN SCIENTIFIC COMMUNITIES (1972).

6. The U.S. public, for example, tends to blame diseases such as cancer on chemicals in the environment. For a study of the impacts of culture on risk perception, see Douglas and Wildavsky, RISK AND CULTURE (1982).

7. Gilbert and Mulkay, OPENING PANDORA'S BOX: A SOCIOLOGICAL ANALYSIS OF SCIENTISTS' DISCOURSE (1984).

8. Latour and Woolgar, *supra* n. 4, at 51.

9. Collins, CHANGING ORDER: REPLICATION AND INDUCTION IN SCIENTIFIC PRACTICE 2 (1985).

10. Gieryn, *Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists,* 48 AM. SOC. REV. 781(1983).

ratory bench, in the pages of scientific journals, or in the courts. Recent legal disputes about the reliability of DNA fingerprinting, for example, illustrate both the "constructedness" of scientific claims and some commonly recurring patterns of deconstruction.

DNA fingerprinting was initially greeted by forensic scientists as the ultimate solution for problems of identification. The technique is solidly grounded in biological theory, empirically tested, technologically feasible, and far more discriminating than other widely employed tests of identity. It can be used not only to convict the guilty but to exonerate the falsely accused, to establish paternity, and to reunite families separated by political terror. Between 1986, when DNA identification was first introduced into U.S. criminal trials, and 1990, the congressional Office of Technology Assessment identified 185 cases in which such tests had been admitted into evidence.[11] Given the technique's rapid spread, it is easy to understand why two 1989 decisions to exclude DNA evidence in New York[12] and Maine[13] raised agitated questions about the capacity of courts to deal with complex scientific testimony.

The arguments that led to the exclusion of DNA evidence in these lawsuits, however, seem entirely predictable when observed through the lens of sociology of science. Let us consider first the issue of "bandshifts," which first reached national prominence in a sexual molestation case in Maine. Lifecodes Corporation, the commercial testing laboratory that had prepared the evidence in this case, identified a match between two DNA samples. The bands that constituted the two "fingerprints," however, did not quite line up; the pattern was the same in both prints but was displaced in a way that suggested the DNA fragments in one sample were slightly larger than in the other.[14] On what basis, then, did Lifecodes read the two prints as identical?

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The defense attorneys' attempts to investigate this question led to a classic case of deconstruction, in which the validity of a particular scientific interpretation unraveled under critical pressure. The experts from Lifecodes revealed at trial that they had employed a previously unvalidated methodology to reconcile the difference between the two DNA inscriptions. A device known as a monomorphic probe had been used to tag a particular fragment of DNA that is the same in every person. Based on the relative displacement of this tagged fragment, the Lifecodes experts had concluded that all the bands in the seemingly displaced sample should be corrected by a factor of 3.15 percent, a calculation that enabled them to explain away the bandshifting as immaterial—in other words, to declare a match between the two samples.

Subsequent discussion both inside and outside the courtroom led to further deconstruction and experimenters' regress. Once other scientists became aware of the technique used by Lifecodes, they not only found fault with it on methodological grounds, but began proposing alternative techniques they asserted would work better. In a direct attack on the validity of the Lifecodes approach, an expert working for the defense said, "The whole experiment wasn't done with the kind of rigor you would expect."[15]

An exchange of letters in *Science* went even further. One writer criticized Lifecodes for using Southern blotting when "other more powerful techniques" were available, such as the use of "internally tagged" DNA samples and substitution of a sequencing gel for an agarose gel. Another writer suggested that the two samples should be "co-injected" or "co-spotted" in addition to being compared in separate lanes. A third complained that it seemed "overly simplistic to apply a single percentage correction to all the bands in a given lane"; a better approach, he suggested, would be "to spike each DNA sample with a set of marker fragments."[16] Together, these criticisms underscored the fact that Lifecodes' identity determination was contingent on the scientific acceptability of a particular, still contested, interpretive technique: the use of monomorphic probes. Once defense experts and other scientists began systematically attacking this technique, the identity finding that it supported also lost its credibility.

Boundary work has also emerged as a significant factor in determining the validity and courtroom acceptability of DNA tests. Urging caution in the use of the new technique, some have asked whether acceptance by research scientists provides a sufficient guarantee of its acceptability for use in criminal identification.[17] Critics have noted that conditions in forensic laboratories may make the tests less reliable than when they are done in research laboratories. This critique elevates the boundary between research science and forensic science into a legally significant issue. It is worth recalling that a similar boundary proved to be legally persuasive in earlier disputes involving the reliability of blood typing by gel electrophoresis. Courts in Michigan and California divided the "relevant community" of experts into two groups to whom they assigned differing credibility: "scientists" from university research laboratories were deemed more reliable than "technicians" working in

11. Office of Technology Assessment, Genetic Witness: Forensic Uses of DNA Tests 14 (1990).
12. People v. Castro, 545 N.Y.S.2d 985 (Sup. 1989).
13. State of Maine v. McLeod (1989).
14. For details, see Norman, *Maine Case Deals Blow to DNA Fingerprinting*, 246 Science 1556 (1989).
15. *Id.* at 1557.
16. *See Letters*, 247 Science 1018-1919 (1989).
17. Thompson and Ford, *DNA Typing: Acceptance and Weight of the New Genetic Identification Tests*, 75 Va. L. Rev. 45, 56-57 (1989).

forensic laboratories.[18]

Statistical challenges to DNA typing exemplify a different kind of boundary issue that will have increasing relevance following *Daubert*: the difference between a test's general validity and its validity in a particular case. As the OTA study noted,[19] the *validity* of forensic DNA tests does not hinge upon our knowledge about the frequency of various DNA markers in the U.S. population. Yet information from population genetics can be highly relevant to a scientifically reliable application of the tests in specific cases—in particular, to calculating the probability of an accidental match. Hence, a finding that the test is scientifically valid will not alone be sufficient to justify the reliance on DNA typing. The applicability of these tests has to be evaluated case by case, with both forensic DNA experts and population geneticists being given the opportunity to interpret the data from their respective disciplinary perspectives.

In sum, the DNA fingerprinting cases suggest that the courts are a better forum for articulating than for definitively resolving deconstructive questions about scientific evidence. The issues of technique, standardization, and statistical interpretation that first arose in trials involving DNA tests eventually were addressed by expert scientific bodies whose work may help to standardize scientific practices in this area. Thus, in April 1992 a panel of the National Academy of Sciences issued a report recommending that DNA testing laboratories should meet stricter quality control standards to ensure the reliability of their results.[20] Interestingly, Eric Lander, a member of the NAS panel, was one of several experts who had testified on some of these issues several years earlier in *Castro*,[21] the New York case where the reliability of DNA fingerprints first came to public attention.

Judicial assessments

Many of the insights from the sociology of science will seem familiar to judges and lawyers skilled in the interpretation of expert testimony. The community of trial lawyers and judges knows perhaps better than any other professional group just how unruly science often is in practice. Their daily experience confirms that scientists are often sloppy, that they use covert assumptions and untried techniques, and that they sometimes manufacture data points or gloss over results that do not quite make sense in the light of theory. Yet even legal practitioners who are well-versed in the ways of science and scientists can benefit from a systematic account of scientific practice. A more serious engagement with the sociology of science can provide legal analysts with at least three forms of enlightenment.

First, familiarity with sociological accounts of science should help dispel unrealistic and overly romanticized views of the legal process. Is cross-examination really "the greatest legal engine ever invented for the discovery of truth"?[22] The social constructivist perspective suggests the answer may be more complex and ambiguous than lawyers generally admit. Adversary procedures are indeed a wonderful instrument for deconstructing "facts," for exposing the contingencies and hidden assumptions that underlie scientific claims, and thereby preventing uncritical acceptance of alleged truths. The adversary process is much less effective, however, in reconstructing the communally held beliefs that reasonably pass for scientific truth. Cross-examination, in particular, privileges skepticism over consensus. It skews the picture of science that is presented to the legal fact finder and creates an impression of conflict even where little or no disagreement exists in practice.

At the same time, the sociological perspective on science alerts us to be cautious about statements like the following from "junk science" critic Peter Huber:

> Some will always insist that all truth is relative and subjective, that anyone should therefore be allowed to testify to anything, that science must be viewed as a chaotic heap of unconnected and contradictory assertions, and that the best we can do is invite the jury to decide scientific truth by majority vote. But anyone who believes in the possibility of neutral law, as many fortunately still do, must at the same time believe in the existence of objective fact, which ultimately means positive science. The only real alternative is nihilism.[23]

Neither the neutrality of the law nor the positivism of science has stood up well enough to tests of empirical research to justify uncritical belief. Fortunately, however, the alternative is not nihilism, at least as long as we remember that the ultimate goal of the courts is the attainable one of dispensing justice, not the impossible one of finding objective truth.

The second conceptual benefit that the sociology of science can offer to judges and lawyers is to provide a more principled basis for evaluating the validity and applicability of scientific evidence. Seventy years of judicial experience showed how difficult it is to implement the seemingly straightforward dictates of the *Frye* test. For example, courts reached inconsistent results in trying to assign scientific techniques such as polygraph tests or DNA fingerprinting to one or more unambiguous "fields." Work in the sociology of science puts these difficulties into context, revealing them to be special instances of the more general phenomenon of boundary drawing in science. The boundaries around fields, as we now know, are themselves contingent: a scientific "field" is intrinsically a moving target, for its boundaries are defined in relation to particular scientific, historical, cultural, and even political circumstances, all of which may change over time. A technique, moreover, can "belong" to more than one field, and, as in the case of DNA tests and population genetics, courts may discover through experience that a technique that has gained general acceptance in one field may not yet have done so in

18. People v. Young, 391 N.W.2d 270, 274-5 (Mich. 1986) (because a theoretical understanding of science is essential, the relevant community of experts is scientists, not technicians); and People v. Brown, 40 Cal.3d 512, 533 (1985) (forensic technicians' lack of formal training and background in the applicable scientific disciplines made them unqualified to state the view of the relevant scientific community). For a different approach to the same boundary issue, however, see People v. Reilly, 242 Cal. Rptr. 496, 503-4 (Cal. App. 1 Dist 1987).

19. OTA, GENETIC WITNESS, *supra* n. 11, at 8.

20. Kolata, *Chief Says Panel Backs Courts' Use of a Genetic Test*, NEW YORK TIMES (April 15, 1992), at A1.

21. *Supra* n. 12.

22. Richardson v. Perales, 402 U.S. 413, 414 (1971).

23. Huber, *Junk Science in the Courtroom*, FORBES (July 8, 1991), at 72.

another—for reasons that are in themselves scientifically valid.

Daubert did well to recognize that "peer review" should not be adopted as a blanket prerequisite for admissibility, replacing *Frye*'s even less workable criterion of "general acceptance." At the same time, the analytic approach outlined above suggests that *Daubert*'s criteria of testability and falsifiability will in their turn prove difficult to implement in courts of law. Whether or not a theory or technique has been adequately tested is as much a social as a scientific question. If an issue is not contentious within a given community of experts, members will readily agree on whether it has been properly tested. For issues in rapidly moving or frontier areas of science, however, experts will be more inclined to question the adequacy of scientific testing, following the well-trodden paths of experimenters' regress. Trial courts may therefore soon discover that Chief Justice William Rehnquist was not alone in his confusion over how to interpret the *Daubert* majority's criterion of falsifiability.

The skeptical reader may wonder at this point whether the sociology of science will help the courts or whether it will lead them into even deeper trouble. When courts previously faltered in their efforts to apply the *Frye* rule, commentators confidently blamed the vagaries of the legal process for the problem, emphasizing in particular the unrealistic constraints that the adversary process places on inquiries about science. Few thought to question whether concepts like general acceptance in the scientific community—and now *Daubert*'s concepts of testing and falsifiability—made good sense in the light of the true internal workings of science. The prevalent assumption was that scientific truth or consensus were always "out there" for the law to find and that any failure to accomplish this goal was due to imperfections in the law's machinery. Social studies of science pose a fundamental challenge to this relatively comfortable assessment. The difficulty of locating facts, truth, or consensus now seems to be embedded in the way science works. The problem of fact finding originates within science itself, although the law's halting approaches to determining what science has to say on a given issue often add layers of doubt and uncertainty to an undertaking that scientists themselves cannot entirely master.

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Courts as participant-observers

This brings us to the third and possibly most significant intellectual contribution that sociology of science can make to the legal process: to provide a more complete accounting of what really takes place when courts engage in scientific fact finding. Clarity on this point will not necessarily bring comfort, especially to those who would like simple rules for solving complex problems. It will, however, help educate the practitioners of both law and science about the limitations of each other's disciplines when it comes to fact finding. An accurate perception of these limits may, in turn, lead to more realistic expectations about what can be achieved in courtroom inquiries into scientific evidence.

Frye, and to a lesser extent *Daubert*, are based on a positivist image of science that does not stand up to sociological, and indeed historical or philosophical, scrutiny. The positivist view presumes that science creates pictures of the real world that the law should merely seek to recover. When courts "find" the facts and opinions of science, or seek to determine the validity of evidence, their role is either to defer to what science already knows or to mimic as far as possible the dynamics of scientific inquiry within the courtroom. A sociologically informed analysis suggests, by contrast, that scientific claims are intrinsically provisional, contingent, and subject to deconstruction under critical scrutiny. Scientific claims, in short, are inherently open-ended, although this property may be clearly apparent only when science is embroiled in controversy. Legal fact finding accordingly reproduces at best a still frame out of the continually unfurling motion picture of science, with all the distortions that such compression entails. Worse yet from the standpoint of scientific positivism, the sociologically open-ended view of science suggests that it is impossible for the legal fact finder to maintain an objective distance from the "facts." In seeking to "find" them, the finder necessarily becomes not just an observer of, but a participant in, the social construction of science.

A recent Fifth Circuit decision, *Christopherson v. Allied Signal Corp.*,[24] vividly illustrates this participant-observer role of the courts in deciding questions about disputed science. Christopherson died of a rare cancer of the liver and colon allegedly caused by exposure to nickel and cadmium fumes generated during battery production at his place of work, Marathon Manufacturing Company. The plaintiff sought to establish this causal link through the evidence of a single expert witness, Dr. Miller, whose testimony was deemed inadmissible by the district court. The case came to the Fifth Circuit on appeal from the district court's grant of summary judgment in favor of Marathon. In an en banc hearing, the appellate judges held that Miller's testimony had been properly excluded, in part because of gross deficiencies in the facts and data upon which Miller had based his opinions.

Whether the trial court in *Christopherson* usurped the jury's fact finding role remains an unresolved issue. For our purposes, however, it is instructive to focus on the reasons for the court's ruling that Miller's testimony was fundamentally flawed. Miller had relied

24. Christopherson v. Allied-Signal Corporation, 939 F.2d 1106 (5 Cir. 1991). 25. 939 F.2d at. 1113.

on the affidavit of a co-worker, Edgar Manoliu, who had described Christopherson's (and presumably his own) exposure to fumes in the workplace. The court noted that Manoliu's affidavit contained numerous gaps and inaccuracies: it contained no information about the type of fumes breathed by Christopherson or, more generally, produced during Marathon's manufacturing process; it failed to state the chemical composition of the fumes or of the contents of the soak tanks; and it apparently misstated both the number of times Christopherson visited the manufacturing area and the average duration of his stays. Moreover, neither Manoliu's affidavit nor any other source provided Miller with information about "the physical facilities at the Marathon plant, including the size of the plant or the impregnation and soak area, or the ventilation available in these areas or in Christopherson's office."[25]

Our brief foray into the sociology of science tells us that the exercise the court undertook here was a kind of deconstruction very similar to experimenters' regress. Confronted with Dr. Miller's statement about causation, a common type of claim in science, the court delved back into the basis for the statement's production and identified various points at which the chain of inference seemed weak or nonexistent. But the criteria of sufficiency that the court applied to the proposed testimony were of the court's own making, reflecting a quite possibly limited understanding of the nature of causation and proof in cases involving health claims. For example, the court clearly felt that "objective" standards (the chemical composition of fumes, numerical evidence of plant size and exposure) should take precedence over a co-worker's subjective testimony that all was not well in the Marathon workplace. This is a conclusion that would not necessarily win support from all members of the medical community. In *Christopherson*, then, a scientific claim about causation was deconstructed according to standards articulated by judges, who thus became active participants in determining what evidence was sufficient. The plaintiff's evidence was excluded on the basis of legal or common-sense notions of validity rather than of criteria emanating from the testimony of other experts.

The procedures for truth seeking in science and the law are profoundly antithetical.

Some practical conclusions

The hardest task for an academic observer of the legal process is to demonstrate that theoretical analysis has useful practical consequences. Will the framework outlined in this article make judicial practice any more reflective or improve the handling of expert evidence by courts? Three general observations on this score are offered, each of which has important consequences for future procedural development.

First, the sociological study of science suggests that science is as much to blame as the law for the seemingly indiscriminate deconstruction of scientific authority in the courts. The contingencies that the law exposes are inherent in the production of science, and, as we saw in the DNA fingerprinting cases, the law may serve a socially valuable function by revealing previously hidden contingencies to both scientists and the public. Yet the procedures for truth seeking in science and the law are profoundly antithetical to one another. Science successfully creates facts because scientists operate in a framework of incremental adjustments and carefully bounded negotiation within communities who share a commitment to closure. Legal fact finding, by contrast, treats all facts as equally contingent in a forum where adversaries have every incentive to overstate the weaknesses in each other's positions. To assess scientific opinion fairly, then, the law may well have to experiment more actively with panels, pretrial hearings, and other non-polarizing approaches to fact finding, including procedures that increase the incentives for negotiation and closure.

Second, it follows from the previous point that legal proceedings should be structured with a clearer sense of the costs and benefits of alternative procedural formats. The panoply of a full-scale pretrial hearing may be appropriate for a mass toxic disaster, where millions of dollars can potentially change hands, or a scientific issue, such as DNA fingerprinting, which is likely to recur many times in the same jurisdiction. At other times, however, the adversary system may be the preferable method for scientific fact finding, both because it is most efficient and because it best safeguards the interests of the parties. The accounts of scientific reality produced by these means may be approximate and incomplete, but the methods of science may do no better in most cases, and they may in any event entail substantially higher costs.

Finally, the analysis proposed here supplies a theoretical basis for the misgivings that the legal community has always entertained about an overly active judicial role in scientific fact finding. When judges exclude expert testimony, appoint their own expert witnesses, or render summary judgments, they inescapably give up the role of dispassionate observer to become participants in a particular construction (or, as in *Christopherson*, deconstruction) of scientific facts. They help shape an image of reality that is colored in part by their own preferences and prejudices about how the world should work. Such power need not always be held in check, but it should be sparingly exercised. Otherwise, one risks substituting the expert authority of the black robe and the bench for that of the white lab coat—an outcome that poorly serves the cause of justice, or of science.

25. 939 F.2d at 1113.

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