

*Report of the 1997 Forum for State Court Judges*

**SCIENTIFIC EVIDENCE**  
**IN THE COURTS:**  
***Concepts & Controversies***



*Sponsored by*

**Roscoe Pound**  
FOUNDATION



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*The Roscoe Pound Foundation  
1050 31st Street, NW  
Washington, DC 20007  
202-965-3500*

*Library of Congress Card Catalog Number: 98-67164  
ISBN: 0-933067-19-5  
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# Executive Summary

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On July 19, 1997, 108 judges from 32 state court systems met with distinguished legal scholars and trial lawyers to discuss the current controversy surrounding scientific evidence in the courts, and especially the question of whether courts should require proof that an expert's opinion is generally accepted in the scientific community as articulated in *Frye v. United States*, 293 F. 1013 (1923) and the developments following the United States Supreme Court's decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579 (1993). The controversy has engendered reams of legal, scientific, and popular commentary, including whole books, a special television program on a major network, and even its own commercial law reporter.

Two legal scholars who have been at the forefront of the scientific evidence issue presented papers addressing different facets of the controversy. A panel consisting of a judge, a legal scholar with a differing viewpoint, and a trial attorney responded to each of the papers with comments. Further comments were then made by the paper presenters. The Foundation made a serious effort to open the conversation to include people of varying opinions.

Professor Sheila Jasanoff, founding chair of Cornell University's Department of Science and Technology Studies, reviewed the nonexclusive criteria identified in *Daubert* for use in evaluating proffered scientific testimony and identified three problems that have made application of these criteria difficult in practice for judges. She then proposed that lay citizens think of "science" as an organized social activity, complete with problems of research funding and sponsorship, relations with industries, and the practical necessity of negotiation of professional standards. After discussing the differences between scientific undertakings with litigation, she outlined five possible models for judges to follow in ruling on questions of scientific evidence: the inquisitor, the gatekeeper envisioned in *Daubert*, the referee, the mediator, and finally the judge, who, she argued, must accept the fact that courts are different from scientific institutions and be prepared to supply "case-specific" judgment in distinguishing between marginal claims of science from legitimate scientific opinions on unsettled or controversial subject matter.

Professor Michael Gottesman, of Georgetown University Law Center in Washington, DC, considered the extent to which state courts should follow the lead of the Court's decision in *Daubert*. He argued that the existence of analogous state rules does not compel state courts to follow the Supreme Court's lead on scientific evidence, and that state courts would be unwise to do so, at least in ordinary civil tort cases, where the effect of *Daubert* in tort cases would be to require the plaintiff to prove causation to a much higher degree of

probability than the traditional standard of a preponderance of the evidence. Finally, Professor Gottesman argued that the four specific reliability factors enunciated in *Daubert* (which are derived from several different scientific disciplines) are not equally appropriate in all cases.

Following each of the commentaries on the papers, the judges divided into six discussion groups. There they gave their own responses to the papers and discussed a number of standardized questions with a guarantee of anonymity.

The luncheon speaker, Arizona Attorney General Grant Woods, described a number of the scientific evidence issues and problems that arose in the course of the tobacco litigation his office pursued in conjunction with other state attorneys general. He described corruption of the scientific literature, use of lawyers to direct and control research and the resulting abuse of the attorney-client privilege, and massive abuse of the discovery process.

At the closing plenary session, discussion group moderators reported that consensus emerged from the dialogue—within individual groups—along the following lines:

*Frye* is still the active case in many jurisdictions, as opposed to *Daubert*.

A number of judges felt that, although *Daubert* was probably intended to liberalize the admissibility of testimony, it has not always had that effect.

Some states have adopted *Daubert* because they feel it is a better standard, and we heard that other states had not adopted *Daubert* because they felt it was “too loose.”

Any imposition of a scientific requirement of 95 percent certainty for expert testimony would revolutionize tort law.

Court-appointed experts were disfavored.

Rather than adhering strictly to any particular model for judging, judges look to the individual circumstances of cases and the context in which the issues arise in judging what happened at the trial court level.

There was a high degree of confidence in cross-examination as a method of testing the credibility of expert opinions.

There was considerable confidence in the jury as a fact-finder and generally in the jury system.

There was significant feeling that the *Daubert* criteria should go to the weight of the evidence, not its admissibility, and that the focus should be on methodology, not on the expert’s conclusions.

Scientific evidence is not a great problem in state courts; “if it ain’t broke, it shouldn’t be fixed.”

This is the report of the fifth Forum for State Court Judges sponsored by The Roscoe Pound Foundation, the purpose of which is to provide an opportunity for state judges to engage in a dialogue on major issues in contemporary jurisprudence with legal scholars and practicing attorneys. In other years we have considered several other crucial topics: the role of state court constitutionalism in protecting individual rights (1992); the independence of the judiciary (specifically, challenges to judicial independence related to inadequate judicial resources and problems with the judicial selection and retention process (1993)); the possible impact on state courts of the *Long Range Plan for the Federal Courts*, which would have shifted a significant portion of the federal caseload to state court benches (1995); and possible state court responses to the American Law Institute's proposed *Restatement of Products Liability* (1996).

After five years, these Forums have become an institution, and with good reason. They recognize the primary role of state courts in our system of justice, and they deal with issues of responsibility and independence that lie at the heart of the judges' work. The number of judges attending each year has increased, and we are proud of that, although we were also delighted in 1997 to welcome back a number of judges who were attending for the third, fourth, or fifth time.

One of the best things we can offer at our Forums is a dialogue between the bench and the legal academy: the interchange that takes place between legal scholarship and theory, on one hand, and the pragmatic, down-to-earth perspective of the judges. The often troublesome gap between what is studied in our law schools—and in other academic settings as well, as we saw—and in the real world, where practicing attorneys and state court judges do their work, is a gap we hope to continue to bridge with our programs.

This kind of dialogue is not achieved very often, and that is everyone's loss. We have learned from experience in our last four Forums that the judges and scholars whom we invite, as well as the trial lawyers who serve as our discussion group moderators, find themselves challenged—and, we hope, also stretched—in the process. In our experience with the Forums, perfect agreement is rare, but sometimes a consensus is reached. Readers of this report will note that it includes examples of both consensus and disagreement emerging from frank exchanges of views.

The topic for 1997, scientific evidence, and particularly the continuing questions of standards of reliability in the wake of the United States Supreme Court's decision in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, has become a watershed issue for the trial bar, for consumers, for scientists, and for judges.

Scientists often, and quite rightly, remind us that what they consider to be “proof” in their work is different from what the legal system considers “proof,” and that their search for objective truth differs from the legal system’s fact-finding in scope, depth, duration, and sometimes even conclusion. Yet the legal system and the scientific community are surprisingly interdependent. Thus, we can all profit from learning more about the nature of scientific standards and the ways in which scientists reach agreement about what constitutes proof.

A related question is how state courts will deal with scientific evidence after *Daubert*. How should they construe their own rules of evidence, which are often similar or identical to the federal rules of evidence, in light of the Supreme Court’s guidelines for the federal courts on determinations of reliability? Considering that *Daubert* is not likely to be the Supreme Court’s last word on this subject, should state courts maintain their traditional independence in questions of scientific evidence, at least for the time being?

These are some of the tough issues judges are facing today. The way in which they are resolved will affect profoundly the adversarial framework within which the courts adjudicate disputes brought by citizens seeking redress. The gravity of the issues, as well as the urgent need to resolve disputes both great and small, made for interesting discussions.

On behalf of The Roscoe Pound Foundation, we want to express our appreciation to Professors Jasanoff and Gottesman, who wrote the papers that set our discussions in motion, and to our panelists, Justice Victoria Lederberg, Judge Mark Bernstein, Dr. Joe Cecil, Professor Margaret Berger, Linda Atkinson, and Tony Roisman. We also must thank the moderators of the small group discussions for their help in facilitating the judges’ discussions and reporting back to the closing plenary session on points of agreement reached in the small groups. And, of course, we are gratified by the participation of the distinguished group of judges, whose interest in this dialogue reflects their commitment to their profession and to our system of justice.

**Roxanne Barton Conlin**  
*President, The Roscoe Pound Foundation*

**Philip H. Corboy**  
*Forum Program Chair*

# I. Background of the Present Controversy over Scientific Evidence

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***First come I, my name is Jowett,  
There's no knowledge but I know it,  
I am the Master of this College.  
What I don't know isn't knowledge.***

The above jingle, composed by undergraduates of Oxford University's Balliol College to spoof the college's Master, Benjamin Jowett,<sup>1</sup> points in sophomoric fashion toward the core questions of the present controversy over scientific evidence:

What is "knowledge"?

What is "known" on any given subject?

Who "knows" it, and where does their knowledge come from?

Who doesn't "know," but mistakenly or intentionally claims to know, and how can we tell the difference?

If no one "knows" everything, at what point can or should we be satisfied that "enough" is known?

How long should we wait for science to achieve certainty before we must move ahead with the timely, just, efficient, and reliable dispute resolution required by modern society?

Historically, intrusions by any branch of government—and by other nonscientific institutions—into questions of scientific knowledge have been viewed with suspicion, and for good reason. The trial of Galileo is probably the most frequently cited (but far from the only) example of the punishment of good science. In more recent days the Soviet regime's official denial of the existence of genes (the "Lysenko" affair of the late 1930s through early 1960s) has served as an important example of irrational abuse of government power. But even in the United States, the temptation to legislate or litigate scientific truth into "correct" configurations has

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1 THE BALLIOL RHYMES (Oxford: Basil Blackwell, 1939). Benjamin Jowett, a 19th century clergyman and renowned classics scholar, presided over Balliol College of Oxford University from 1870 to 1893. Ironically, in his day Jowett was accused of heresy owing to several controversial religious writings. Thanks to Dr. John Jones, dean and archivist of Balliol College, and to Alan Tadiello, assistant librarian of the Balliol College Library, for providing the background of this verse.

occasionally been too strong for lawmakers to resist.<sup>2</sup>

Viewed from the other end, science's devotion to theories that are later discredited has also had troubling impacts on the law. Speakers and participants in the 1997 Forum pointed to numerous historical examples (no doubt many of them closely related to, and accepted by, the culture of their day) of spurious or dubious excursions of "science" that were accepted as conventional wisdom, and were sometimes even held admissible in courts of law: astrology, mind reading, dowsing,<sup>3</sup> spectral evidence,<sup>4</sup> phrenology,<sup>5</sup> and even alchemy.<sup>6</sup> Such examples may explain the infrequent acceptance by modern courts, through judicial notice, of scientific matters as "beyond dispute."<sup>7</sup>

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## THE NATURE OF THE CURRENT CONTROVERSY

Depending on who is asked, the recent flurry of activity by academics, jurists, journalists, and politicians on the subject of scientific evidence represents either a long-overdue correction of courts gone astray or an all-out assault on the civil justice system by entities with great stakes in the outcomes of litigation and no regard for the institution of trial by jury.

This controversy has been under way for at least a decade. Outside of court, it has engendered reams of legal, scientific, and popular commentary, including numerous journal articles and entire books,<sup>8</sup> at least one special television program on a major network, and even a specialized legal newsletter.<sup>9</sup> Outside of the legal profession it

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2 A World Wide Web site devoted to mathematics reminds us that the Indiana State Legislature, in its House Bill No. 246 (1897), once came perilously close to legislating a new value for pi (i.e., *not* 3.1416...). The author of a new mathematical theory expected it to revolutionize mathematics and planned to charge royalties for its use in any textbook, but offered it to the State of Indiana free of charge if it were adopted by the state legislature. The bill passed the Indiana House *unanimously* after the Committee on Education recommended its passage. It was later killed in the state senate only when it came to the attention of a member of the Indiana Academy of Science who happened to be lobbying for funds for scientific research on one of the days when the bill was being debated. More details of this peculiar event are set out in "Frequently Asked Questions in Mathematics" at <<http://daisy.uwaterloo.ca/~alopez-o/math-faq/math-faq.html>>.

3 Dowsing is the search for underground water or minerals with a forked wooden "divining rod" that is expected to dip downward when it is moved over a mineral vein or water supply.

4 "Spectral" evidence was purported evidence of the existence and activities of a ghost or specter. As described by Judge Mark Bernstein, the idea behind spectral evidence, admitted in some early American "witch trials" of the 17th century, was that "a particular witness could be, by virtue of endowed gifts, capable of seeing the interaction between an accused person and a satanic or demonic agent. The proof of the validity of such an expert's testimony was the fact that nobody else could see the demonic or satanic agent, so witness's power was self validating. The courts, as triers of fact, found such evidence admissible and probative."

5 Phrenology is "the study of the conformation of the skull as indicative of the mental faculties and traits of character especially according to the hypothesis of F. J. Gall (1758–1828)." WEBSTER'S THIRD NEW INTERNATIONAL DICTIONARY OF THE ENGLISH LANGUAGE, UNABRIDGED (G. & C. Merriam 1981).

6 Alchemy is defined as "the medieval chemical science and speculative philosophy whose aims were the transmutation of the base metals into gold, the discovery of a universal cure for diseases, and the discovery of a means of indefinitely prolonging life." WEBSTER'S THIRD NEW INTERNATIONAL DICTIONARY OF THE ENGLISH LANGUAGE, UNABRIDGED (G. & C. Merriam 1981).

7 See FED. R. EVID. 201 and its state analogues.

8 See, e.g., SHEILA JASANOFF, SCIENCE AT THE BAR: LAW, SCIENCE AND TECHNOLOGY IN AMERICA (Harvard 1995).

9 MEALEY'S DAUBERT REPORTS, available through WESTLAW's MDAUBREP database.

has also resulted in calls by several medical organizations for the American Medical Association to declare medical testimony in court to be “the practice of medicine,” thereby potentially subjecting the testimony of medico-legal expert witnesses to peer review and disciplinary action should it be deemed “false.”<sup>10</sup> As with other recent controversies, numerous “horror stories” have been used to promote acceptance of purported “reforms” of scientific evidence practice, and the Forum participants, despite their general faith in the existing system and their belief that its problems are limited, were able to offer a few of their own.

Within more formal legal circles, the controversy has become focused particularly on two federal court decisions (*Frye* and *Daubert*<sup>11</sup>) that embody different judicial approaches to scientific evidence, and on the struggles of both federal and state courts over which of the two approaches to follow. Within the judiciary it has led to some renewed interest in court-appointed experts,<sup>12</sup> and to efforts to quantify more precisely the extent of any real “problems” of scientific evidence in the courts.<sup>13</sup>

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## **FRYE VERSUS DAUBERT**

Some court systems have followed *Frye* for over 70 years, and they routinely look to whether scientific evidence offered in court is “generally accepted” within the scientific community—generally meaning whether it results from a conventional, peer-reviewed scientific process.<sup>14</sup> These courts are understandably loath to abandon

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10 See Bill Ibelle, *Doctors May Be Disciplined for “Bogus” Testimony*, LAWYERS WEEKLY USA, May 18, 1998, at B1. The resolution adopted by the AMA’s House of Delegates at its interim 1997 meeting (“[O]ur AMA . . . will not tolerate false testimony by physicians . . .”) is viewable at <[www.ama-assn.org/meetings/public/int97/finalrpt/197-refb.htm](http://www.ama-assn.org/meetings/public/int97/finalrpt/197-refb.htm)>.

11 *Frye v. United States*, 293 F. 1013 (1923) and *Daubert v. Merrell Dow Pharm.* 509 U.S. 579 (1993). The several stages of the *Daubert* litigation are sometimes referred to as “Daubert I,” “Daubert II,” etc.: *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 727 F.Supp. 570 (S.D. Cal. 1989)(Daubert I); *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 951 F.2d 1128 (9th Cir. 1992)(Daubert II); *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579 (1993)(Daubert III); *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 43 F.3d 1311 (9th Cir. 1995)(Daubert IV).

12 For more on this subject as it relates to federal courts, see JOE S. CECIL and THOMAS E. WILLGING, COURT-APPOINTED EXPERTS: DEFINING THE ROLE OF EXPERTS APPOINTED UNDER FEDERAL RULES OF EVIDENCE 706 (Federal Judicial Center 1993).

13 See oral remarks of Dr. Joe S. Cecil, p. 62 *infra*, describing the Federal Judicial Center’s 1991 survey of federal judges on problems involving expert testimony.

14 The *Frye* court reviewed the decision of the then Supreme Court of the District of Columbia to exclude offered testimony on the “systolic blood pressure deception test,” i.e., a lie detector test. Justice Van Orsdel concluded the court’s very brief opinion as follows:

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.

We think the systolic blood pressure deception test has not yet gained such standing and scientific recognition among physiological and psychological authorities as would justify the courts in admitting expert testimony deduced from the discovery, development, and experiments thus far made.

293 F. at 1014.

*Frye*, and the extensive case law that has become rooted in it, often because they believe the *Frye* test is simpler to apply than *Daubert* or because they feel the differences between the two cases are actually minor. Other courts point to the typical judge's limited scientific background. They express reluctance to enter the thicket of scientific dispute to serve as judges not only of law but also of the validity of scientific evidence under criteria laid down by the U.S. Supreme Court in *Daubert*.<sup>15</sup>

Additionally, the precise commands and limits of *Daubert* continue to be a source of debate and litigation. They have resulted in conflicting court decisions on, e.g., whether there is a meaningful distinction between the "scientific" evidence to which *Daubert* applies on its face and "technical" evidence denominated separately in the Federal Rules of Evidence<sup>16</sup> and the important question of the proper standard of appellate review for *Daubert*-based decisions by trial courts.<sup>17</sup>

On a broader issue, other courts, and numerous commentators, have criticized both *Frye* and its progeny, and, to a lesser extent, *Daubert*, for enshrining "mainstream" science ill-advisedly. They point, for instance, to a number of examples of the failure of the peer review system to uncover errors or even outright fraud.<sup>18</sup> Even beyond the most obvious (and presumably infrequent) examples of corruption, recent examinations of the scientific process as a whole (some of them undertaken because

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15 The four criteria are testability, control of error, publication in a peer-reviewed journal, and general acceptance. However, the Supreme Court took care to provide a disclaimer: "Many factors will bear on the inquiry, and we do not presume to set out a definitive checklist or test. . . . The inquiry envisioned by Rule 702 is, we emphasize, a flexible one" (footnote omitted). *Daubert*, 113 S.Ct. 2786, 2796–97.

For a state appellate court decision that added to the four "*Daubert* criteria" a fifth consideration, "whether the expert's research in the field has been conducted independent of litigation," see *McDaniel v. CSX Transp., Inc.*, 955 S.W.2d 257, 265 (Tenn. 1997).

16 *Daubert*, 113 S.Ct. at 2795, n.8. For cases holding *Daubert* inapplicable based on this distinction, see *Thornton v. Caterpillar, Inc.*, 951 F. Supp. 575 (D.S.C. 1997); *Carmichael v. Samyang Tire, Inc.*, 131 F.3d 1433 (11th Cir. 1997); *Compton v. Subaru Am.*, 82 F.3d 1513 (10th Cir. 1996). For cases to the contrary, see *Bogosian v. Mercedes-Benz N. Am., Inc.*, 104 F.3d 472 (1st Cir. 1996); *Watkins v. Telsmith Inc.*, 121 F.3d 984 (5th Cir. 1997); and *Tyus v. Urban Search Management*, 102 F.3d 256 (7th Cir. 1996). See also G. Ross Anderson, *Round Pegs and Square Holes—The Aftermath of Daubert*, SO. CAR. TR. LAW. BULL., Fall 1996 at 9.

17 Following the Forum, the U.S. Supreme Court handed down its decision in *General Electric Co. v. Joiner*, \_\_\_ U.S. \_\_\_, 118 S.Ct. 512, 139 L.Ed.2d 508 (1997) ("abuse of discretion" standard applied to district court's decision to exclude scientific evidence).

18 In 1997, the prestigious *New England Journal of Medicine* published a highly critical review of SANDRA STEINGRABER, *LIVING DOWNSTREAM: AN ECOLOGIST LOOKS AT CANCER AND THE ENVIRONMENT* (Addison-Wesley 1997), a book on health-related concerns about environmental pollution. The reviewer, Jerry Berke, MD, concluded that

*Living Downstream* frightens, at times misinforms, and then scorns genuine efforts at cancer prevention through lifestyle change. The objective of *Living Downstream* appears ultimately to be controversy.

Jerry H. Berke, Book Review, 337 N.E.J.MED. 1562 (1997). When it was later pointed out to the Journal's editors that Dr. Berke was the medical director of W.R. Grace & Co., a company whose plant in Woburn, Massachusetts, was alleged to have polluted a local water system with carcinogenic waste, the *Journal* published an apology for not informing readers of the review writer's conflict of interest. Its editor-in-chief told the Associated Press that it was "laughable that Berke would think that he could write an objective review of the book." *Medical Journal Apologizes for Ethics Blunder*, WASH. POST, Dec. 28, 1997, at A3.

of the current controversy) have exposed troubling patterns of scientific passivity to initial research failures,<sup>19</sup> resistance to new theories,<sup>20</sup> and heavy influence by the industries that provide financial backing for scientific research.<sup>21</sup>

Meanwhile, judges and juries continue to be presented on a daily basis with questions involving science that they cannot leave undecided until science achieves certainty.<sup>22</sup>

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19 The researchers who in 1997 announced their success in developing procedures to clone sheep continued their research well after the possibility of such discoveries had been dismissed by most other scientists. One account of the news of the successful birth of “Dolly,” the first-ever cloned sheep, observed that “Researchers have tried to clone adult mammals for many years with no hints of success, and most had all but given up. ‘It flied in the face of biological dogma,’ said Neal First, an animal biotechnologist and chairman of reproductive biology at the University of Wisconsin in Madison. ‘Work in mice and other animals had convinced us all it couldn’t be done.’” Rick Weiss, *Scottish Scientists Clone Adult Sheep*, WASH. POST, Feb. 24, 1997, at A1.

20 A recent example is the “cosmic snowball” controversy among geophysicists. In an informative article that should be read by everyone interested in the peer review and “general acceptance” criteria for admission of scientific evidence, Professor Louis Frank of the University of Iowa has described his controversial discovery of the “small comet” phenomenon and the resulting “scientific vandalism” with which he charges many of his colleagues in the scientific community. See Louis A. Frank and Patrick Huyghe, *Out There*, WASH. POST, July 13, 1997, at C1; and LOUIS A. FRANK and PATRICK HUYGHE, *THE BIG SPLASH* (Birch Lane Press 1990). A lay person’s guide to the phenomenon is viewable at <<http://smallcomets.physics.uiowa.edu>>.

In another recent example of the same phenomenon, Stanley B. Prusiner, a scientist long derided by colleagues as a “maverick,” was awarded the 1997 Nobel Prize in Medicine. Prusiner advanced the theory that microscopic particles called prions—which are made of protein with no genetic material—could multiply and cause a class of degenerative brain diseases involving dementia. The diseases are called “transmissible spongiform encephalopathy,” and they include human Creutzfeldt-Jakob Disease, “mad cow disease,” and “kuru,” a disease discovered in Papua New Guinea in the 1920s. Prusiner’s work was credited in the Nobel Prize press release as having added a link toward understanding Alzheimer’s disease, and as laying a theoretical foundation for the future treatment of these conditions. See Rick Weiss, *Nobel Prize Vindicates U.S. Scientist*, WASH. POST, Oct. 7, 1997, at A1. Dr. Prusiner’s description of his research is viewable at <<http://keck.ucsf.edu/neuroscience/prusiner.htm>>. See also Gretchen Vogel, *Prusiner Recognized for Once-Heretical Prion Theory*, 278 SCIENCE 214 (Oct. 8, 1997). But see Richard Rhodes, *Pathological Science*, NEW YORKER, Dec. 1, 1997, at 54 (noting that even the award of a Nobel Prize may represent little more than a decision by a renowned institution to take a particular side in a continuing debate, and providing several examples of scientific theories initially accepted widely but later rejected).

21 The paradigm example of the dangers of industry funding, and of the dependence of nonindustry institutions on it, may have emerged in the recent tobacco litigation. In late 1997 the *Dallas Morning News* disclosed that two law firms representing tobacco companies (Shook, Hardy & Bacon of Kansas City and Jones, Day, Reavis & Pogue of Cleveland) were instrumental in secretly funding research projects based at the University of Texas Health Center and elsewhere to the extent of \$1.68 million. The research was carried out by a scientist who specialized in finding weaknesses in studies linking second-hand smoke to diseases. Documents discovered by the newspaper showed that the money was passed through a special account to keep it off the hospital’s books. *Tobacco Lawyers Funded Scientist*, WASH. POST, Nov. 16, 1997, at A22.

22 The often passionate desire of scientists to achieve certainty—a topic of considerable discussion at the 1997 Forum—may be illustrated best by the cautionary tale of “Fermat’s Last Theorem,” the legendary mathematical proof referred to by Prof. Jasanoff at p. 22 of this report. This abstruse problem in number theory was first posed by the French lawyer and amateur mathematician Pierre de Fermat (1601–1665). It has no practical applications, is accessible only to mathematicians, and is said to be understood well by only about a hundred people. This theorem resembles the Pythagorean theorem known to every high school graduate, but projected into three dimensions and beyond. No proof of the problem was ever reached until a Princeton University researcher, Andrew Wiles, published a proposed proof in 1995 (over 350 years after the problem was originally posed), after spending essentially his entire professional life working on it. An interesting history of the problem and its solution may be viewed by the interested reader on the Internet at <<http://daisy.uwaterloo.ca/~alopez-o/math-faq>>. A much more accessible account of the proof and its solution can be viewed at the Public Broadcasting Service’s World Wide Web site: <[www.pbs.org/wgbh/nova/proof](http://www.pbs.org/wgbh/nova/proof)>.

Some examples mentioned during the Forum are as abstruse and difficult as the reliability of testimony on battered woman syndrome,<sup>23</sup> child abuse syndromes,<sup>24</sup> and other psychological syndromes.<sup>25</sup> Some are as mundane as the “Breathalyzer” or horizontal gaze nystagmus tests for intoxication.<sup>26</sup> Some are as complex and revolutionary—with tremendous implications for personal freedom—as the DNA evidence now frequently used to convict or exonerate criminal defendants, to identify the deceased, and to establish family relationships.<sup>27</sup>

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## THE FORUM

More than 100 judges, representing more than 30 jurisdictions, took part in The Roscoe Pound Foundation’s 1997 Forum for State Court Judges. Their deliberations were based on papers written specially for the Forum by Professor Sheila Jasanoff, founding chair of Cornell University’s Department of Science and Technology Studies, and by Professor Michael Gottesman of Georgetown University Law Center in Washington, DC. The papers were distributed to participants in advance of the meeting, and the authors also summarized their views to the audience informally (see section II of this report).

Each paper presentation was followed by discussion by a distinguished panel of commentators. Responding to Professor Jasanoff’s paper were Anthony Z. Roisman, Esquire, of Lyme, New Hampshire; Professor Margaret A. Berger of Brooklyn Law School; and Judge Mark Bernstein of the Philadelphia County, Pennsylvania, Court of Common Pleas. Responding to Professor Gottesman’s paper were Linda Miller Atkinson, Esquire, of Channing, Michigan; Dr. Joe S. Cecil of the Federal Judicial Center; and Justice Victoria Lederberg of the Rhode Island Supreme Court.

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23 See *Soutiere v. Soutiere*, 657 A.2d 206 (Vt. 1995)(testimony on “battered woman syndrome” and post-traumatic stress syndrome offered to show impact of abuse on plaintiff in divorce action). For a brief overview of this subject, see Lenore E. A. Walker, *Understanding Battered Woman Syndrome*, TRIAL, Feb. 1995, at 30.

24 See, e.g., *Steward v. State*, 652 N.E.2d 490 (Ind. 1995)(testimony on “child sexual abuse syndrome,” i.e., evidence of victim’s behavior as consistent with that of victim of sexual abuse, offered to prove that sexual abuse had in fact occurred); *Commonwealth v. Dunkle*, 602 A.2d 830 (Pa. 1992)(testimony on “child abuse accommodation syndrome” offered to explain why victim had not previously complained of abuse); *State v. Tanner*, 675 P.2d 539 (Utah 1983)(evidence of “battered child syndrome,” or physical and other signs of physical child abuse, offered to prove physical abuse of victim); and *State v. Frye*, 461 S.E.2d 664 (N.C. 1995), cert. denied, \_\_\_ U.S. \_\_\_, 134 L. Ed. 2d 526 (1996) (testimony on “child abuse syndrome” exhibited by criminal defendant offered as evidence of impaired capacity as mitigating circumstance in defendant’s murder prosecution).

25 On the general subject of *Daubert* and psychological syndromes of various types, see James T. Richardson et al., *The Problems of Applying Daubert to Psychological Syndrome Evidence*, 79 JUDICATURE, July–Aug. 1995, at 10.

26 Nystagmus is an involuntary jerking movement of the eyes. For an explanation of this test, see *State v. Hill*, 865 S.W.2d 702 (Mo. App. 1993) (HGN test held admissible as evidence of intoxication).

27 For more on the general subject of DNA evidence, see EDWARD CONNORS ET AL., CONVICTED BY JURIES, EXONERATED BY SCIENCE: CASE STUDIES IN THE USE OF DNA EVIDENCE TO ESTABLISH INNOCENCE AFTER TRIAL (U.S. Dept. of Justice, National Institute of Justice 1996).

After each paper presentation and commentary, the judges separated into six smaller groups to discuss the issues raised in the paper, led by Fellows of The Roscoe Pound Foundation. The paper presenters and commentators visited the groups to share in the discussion and respond to specific questions. The discussions were tape-recorded and transcribed by court reporters. However, under the ground rules set in advance of the discussions, comments by the judges were not made for attribution in the published report of the Forum. A selection of the judges' comments appears in section IV of this report.

At the plenary session that closed the Forum, the moderators summarized the judges' views of the issues under discussion (see section V of this report).

This report is based on the papers written and presented by Professors Jasanoff and Gottesman and on the transcripts of the plenary sessions and group discussions.

**James E. Rooks, Jr.**  
Forum Reporter



## II. Papers, Oral Remarks, and Comments

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### A. JUDGING SCIENCE: ISSUES, ASSUMPTIONS, MODELS

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SHEILA JASANOFF

**I**n her paper, Professor Jasanoff reviewed the nonexclusive criteria identified in Daubert for use in evaluating proffered scientific testimony. She identified three problems that have made application of these criteria difficult in practice for judges: the existence of several models of science, which make application of a single set of standards impossible; the difficulty or impossibility of adapting scientists' own standards of proof to legal proceedings; and the duty of courts to do justice, separate and apart from the desirability of achieving certainty to scientific standards.

What lay citizens describe as "science," Professor Jasanoff argued, should be understood as an organized social activity rather than as a monolithic body of knowledge or a universal set of standards. This aspect of science is critical to a full appreciation of the workings of basic research and of the practices of replication and peer review, all of whose capacity to produce definitive, objective knowledge may be limited by problems of funding and sponsorship, relations with major industries, and the practical necessity of negotiation, rather than dictation, of professional standards.

Professor Jasanoff then contrasted the workings of science with the application of its methods and conclusions in the context of litigation. She described the accommodations that must be made to accomplish this within an adversary environment in which partisanship must be expected—indeed, depended upon—both to drive relevant research and to provide an additional level of review of scientific work. Judicial interests in resolution of civil disputes in an efficient and timely manner may necessitate both the acceptance of levels of proof less stringent than those sought by scientists outside of the courtroom and the acceptance of a degree of uncertainty in final judgments in the interest of allowing parties to conclude litigation and proceed with their affairs.

Professor Jasanoff identified five possible models for judges to follow in coming to terms with the separate values and goals of science and litigation: the inquisitor, who seeks control of all evidence and must rely on experts to be unbiased; the gatekeeper envisioned in Daubert, who may inadvertently rely on personal assumptions about science and, in so doing, devise new standards of evaluation of evidence not contemplated by the Supreme Court; the referee, who views closure as the highest goal and so may seek out weaknesses in both sides' cases to propel them toward settlement; the mediator, who prompts parties themselves to recognize their cases' weaknesses in the interest of settlement; and finally the judge. The judge accepts the fact that courts are institutions of justice, not institutions of science, and that case-specific judgment must be applied to the process of identifying marginal claims of science and distinguishing them from legitimate scientific opinions on unsettled or controversial subject matter.

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## I. INTRODUCTION

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*Daubert has been widely interpreted as an injunction that judges should “think like scientists” when assessing the validity of scientific evidence.*

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In its landmark decision on the admissibility of scientific evidence, *Daubert v. Merrell Dow Pharmaceuticals, Inc.*,<sup>1</sup> the U.S. Supreme Court directed federal judges to determine the validity of expert scientific testimony in accordance with standards used by scientists themselves. Specifically, *Daubert* set aside the 70-year-old “*Frye* rule”<sup>2</sup> which stated that scientific evidence could be admitted only if it was generally accepted within the relevant field in which it belonged. Instead, *Daubert* decreed that judges should strive to assess whether proffered evidence rests on scientifically valid reasoning or methodology.

To assist judges in this endeavor, the Court proposed four non-exclusive criteria, cautioning that these should not be regarded as “a definitive checklist or test”: (1) is the evidence based on a testable theory or technique; (2) has the theory or technique been peer reviewed; (3) in the case of a particular technique, does it have a known error rate; and (incorporating the *Frye* standard), (4) is the scientific method underlying the expert testimony generally accepted?<sup>3</sup> The strong message conveyed to lower courts by *Daubert* was that judges should play a more proactive, gatekeeping role with respect to scientific evidence instead of passively allowing virtually all such testimony to reach the jury.

*Daubert* has been widely interpreted as an injunction that judges should “think like scientists” when assessing the validity of scientific evidence. On its face, this requirement seems both sensible and unproblematic. What could be more reasonable than to require that scientific evidence should conform to scientists’ own tests of validity? In practice, however, *Daubert* has given rise to three sets of difficulties that make the decision far from simple to implement: first, there is no single model of “good science” that can be mechanically applied to all offers of scientific evidence; second, the standards used by scientists may be impractical or impossible to achieve in legal proceedings; third, the law’s commitment to doing justice may conflict with full acceptance of scientists’ standards. Each issue opens up areas of uncertainty and discretion for both judges and juries, leading to possible variation among jurisdictions in the admissibility of the same scientific evidence. This paper reviews each issue in greater detail, presenting the major options, alternatives, and pitfalls that may confront courts in making admissibility decisions in the post-*Daubert* era.

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1 509 U.S. 579, 113 S.Ct. 2786 (1993).

2 A rule governing the admissibility of novel scientific evidence was laid down in *United States v. Frye*, 293 F. 1013 (D.C. Cir. 1923). In *Daubert*, the Court held that the *Frye* rule had been superseded by the congressionally enacted Federal Rules of Evidence.

3 *Daubert*, 509 U.S. at 593, 113 S.Ct. at 2796.

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## II. WHAT IS SCIENCE?

The *Daubert* majority seemed to assume that there is a distinct, well-demarcated “scientific method” and that criteria reflecting this method can be objectively applied to determine the validity of scientific evidence. Further, two of the criteria that the Court proposed—testability and error rate—suggest that the majority accepted experimental science as the canonical model of scientific activity. These explicit and implicit assumptions greatly oversimplify the diversity of approaches and methods that characterize contemporary science. They also rest on an idealized conception of the scientific method that pays inadequate attention to the social contexts in which scientific research is conducted, evaluated, and interpreted.

Although the experimental method deservedly occupies a position of importance within science, it is not the only technique by which science is done. To be “scientific,” a theory does not necessarily have to be subjected to experimental testing. The Darwinian theory of natural selection is one very widely accepted scientific theory that does not easily lend itself to such tests. Many theories in the human sciences—including psychology, psychiatry, anthropology, and sociology—are also generally accepted as valid although they cannot be tested through conventional experimentation. Moreover, some types of scientific claims, such as theories of disease causation, cannot be experimentally tested for ethical and practical reasons. These examples indicate at the very least that scientific validity cannot be assessed in court in terms of a single, universal set of criteria.

How then should the validity of scientific methodology be evaluated? Particularly important to judicial decisionmaking is a growing body of scholarship that regards science as a form of organized social activity. Much of this work

illuminates, often in minute detail, the practices through which scientists produce their authoritative understandings of the world.<sup>4</sup> The most significant insight that has emerged from this research is the view that science, like other forms of human knowledge, is “socially constructed.” According to this view, the “facts” that scientists discover about the physical and social worlds are not simple reflections of reality; rather, these facts invariably contain a social component because they are produced by human agency, through the institutions and processes of science. Facts, in other words, are not merely raw observations whose objectivity is secured by the scientific method. Observations achieve the status of scientific facts only if they are produced in accordance with prior understandings about the correctness of

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*Particularly important to judicial decisionmaking is a growing body of scholarship that regards science as a form of organized social activity.*

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4 For an overview of this literature, see SHEILA JASANOFF, GERALD MARKLE, JAMES PETERSEN, and TREVOR PINCH, EDs., *HANDBOOK OF SCIENCE AND TECHNOLOGY STUDIES* (Sage Publications 1995).

particular theories, experimental methods, instrumental techniques, validation procedures, statistical analyses, review processes, and the like. These understandings, in turn, are socially derived through continual negotiation and renegotiation among relevant bodies of scientists.

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*Scientists negotiate over a whole range of issues that are quite central to the conduct of science and hence are part of the “scientific method” .... When these negotiations are successful, the resulting science looks objective because most or all potentially significant conflicts have been resolved.*

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Scientists negotiate over a whole range of issues that are quite central to the conduct of science and hence are part of the “scientific method”: how to interpret variations in experimental approaches and results; determine what constitutes adequate replication and peer review; resolve disagreements over unexpected data points; explain equipment failure and malfunction; apply quantitative measures to observed changes in systems being studied; or establish benchmarks and surrogate measures for phenomena that cannot be examined by direct observation. When these negotiations are successful, the resulting science looks objective because most or all potentially significant conflicts have been resolved. However, cessation of conflict within particular scientific communities does not necessarily guarantee the objectivity of their conclusions, just as the existence of controversy does not in and of itself make scientific evidence unreliable in *Daubert* terms.

Even in relatively well-established areas of scientific research, the line between proper and unacceptable methodology is not always clearly drawn. For example, a lengthy investigation of alleged misconduct in the laboratory of Robert Gallo, the noted co-discoverer of the AIDS virus, led to inconclusive results which ultimately exonerated Gallo.

The investigation oscillated for a long time between charges that Gallo and his assistants had used scientifically unacceptable methods to appropriate the virus from colleagues, and the counterargument that their admittedly unorthodox methods were characteristic of cutting-edge science.

Familiarity with the social organization of research and with the socially negotiated aspects of standard scientific practices should enhance judges’ and juries’ ability to assess the objectivity and reliability of scientific evidence. The following observations are especially important:

**Basic research.** Basic scientific research, usually conducted in universities with the aid of public funds, is commonly thought to be a source of objective scientific findings and unbiased expertise. Basic research contrasts in this respect with applied or industry-sponsored research. Disinterestedness—that is, the lack of ties to particular social concerns or political agendas—is part of the ethos of basic science and contributes to the appearance of objectivity. Since the 1950s, the disinterestedness of basic research has been encouraged primarily through merit-based federal funding, allowing research programs to be guided as far as possible by scientific considerations alone. More recently, however, reductions in federal support

for academic science, together with policies fostering closer university-industry relationships, have compromised the detached character of basic research, especially in the biomedical sciences and biotechnology.

**Replication.** In the experimental sciences, the validity of findings depends critically on the replication of one researcher's results by one or more others. Results that have been replicated are generally accepted as true or valid, while those that cannot be replicated are more likely to be viewed with distrust. Studies of scientific practices have disclosed, however, that exact duplication of experiments is virtually impossible. In the real world, there will always be some differences in the background conditions under which the original experiment was conducted and those under which efforts were made to replicate it. Thus, different bioassays to test the effects of a particular chemical may differ in the animal species selected, the dose and pathway of exposure, and the number and nature of controls. Whether one bioassay replicates another may therefore be a matter of dispute. In order to accept any study or experiment as an adequate replication of an earlier one, scientists have to agree which differences they consider important enough to question and which they are prepared to dismiss as trivial. Such agreements are least likely to exist in new, rapidly moving, or contested areas of science.

In general, agreements about standards for replication are negotiated on a continuing basis within particular experimental communities. These internal dynamics of science are generally not apparent to outside observers unless a controversy erupts and makes them public. What counts as proper replication within one scientific subculture may not have comparable standing in another. Even such fundamental practices as the selection of controls may vary from one field to another.<sup>5</sup> The validity of replication therefore has to be judged within particular fields of scientific endeavor, much as the *Frye* rule implied.

**Peer review.** As the *Daubert* majority noted, review by peer scientists is an extremely important mechanism for helping to ensure the validity of scientific claims. Peer review is known to be an imperfect device for detecting fraud and misconduct, but it does serve a number of other valuable functions: to refine the analysis of scientific data, point out inadequacies in argument, sharpen the focus of conclusions, and improve the clarity of written presentations. Social choices involved in the conduct of peer review include, first and foremost, the identification of reviewers. Journal editors and funding agencies can promote, modify, delay, or kill scientific submissions simply by choosing referees with known disciplinary or methodological biases. Peer review practices and standards also vary widely across journals, disciplines, and professional societies. In sum, while peer review improves the overall quality of research, as well as the likely integrity of individual publications, it does

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5 In some scientific fields, such as research on the effects of cancer drugs, it is not uncommon to use "historical controls," a method that compares patients in the study group with comparable patients studied in the past. In other research areas, such as molecular biology or environmental toxicology, the use of historical controls is much more controversial.

not mechanically guarantee reliability. Courts therefore should neither be wholly dismissive of peer review<sup>6</sup> nor accord it more respect than it deserves.

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### III. SCIENCE IN THE LEGAL CONTEXT

Scientific evidence prepared for courtroom presentation is a special kind of applied science, designed to help the trier of fact to understand and determine the facts in issue. Such science is subject to institutional constraints that go well beyond the normal social influences on scientific research described above. The requirements for truth and objectivity are different in legal and scientific settings, and there are substantial differences as well between the goals, methods, and processes of legal and scientific fact-finding. A review of the most salient differences follows (see Table 1 for a summary).

**Goals and objectivity.** In common law litigation contexts, scientific evidence is generated by and for the parties and thus is robbed of its usual mantle of disinterestedness. The rationale for permitting this departure from the norms of basic science is that the parties have the strongest incentives to uncover and represent the scientific knowledge most favorable to their claims. Placing the evidentiary burden on the interested parties is a relatively economical way to ensure that relevant facts or bodies of knowledge will not be overlooked or disregarded. The price paid is that the science strategically brought forward by the parties may be of marginal quality, utility, and relevance. This consideration led the Ninth Circuit Court of Appeals, entertaining *Daubert* again on remand,<sup>7</sup> to posit that (presumably disinterested) pre-litigation scientific research is inherently more reliable than (presumably interested) research initiated in response to litigation.

A blanket rule of this kind seems problematic for several reasons. First, given the developing links between universities and industry, the assumption that pre-litigation research is necessarily more objective or disinterested than post-litigation research may not be warranted. Second, in many legal controversies that depend on science for their resolution, the relevant research base may not preexist litigation in a meaningful way; not until litigation develops may researchers find the issues worth investigating, and there is nothing inherently illegitimate about such motivation. Third, even when relevant scientific data are available prior to litigation, crucial assessments or statistical interpretations of the data, possibly using novel methodologies, may be undertaken only in response to litigation. Fourth, science produced in response to litigation does not have to be unreliable, provided that care is taken to weed out bias and ensure adequate review.

For example, disputes over the technique of DNA typing (or “DNA fingerprinting”)

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6 For an extreme example, see *Valentine v. Pioneer Chlor Alkali Company, Inc.*, 921 F.Supp. 666, 674–76 (D.Nev. 1996) (in *dictum*, distinguishing pre-publication or “editorial” peer review from post-publication or “true” peer review that includes attempts at replication, and declining to accept publication in peer-reviewed journal, by itself, as sufficient evidence of good scientific methodology).

7 *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 43 F.3d 1311, 1318–19 (9th Cir. 1995).

8 NATIONAL RESEARCH COUNCIL, *DNA TECHNOLOGY IN FORENSIC SCIENCE* (1992) and *THE EVALUATION OF FORENSIC DNA EVIDENCE* (1996).

led the National Research Council, one of the nation's most authoritative scientific advisory bodies, to produce two reports on how to validate DNA evidence.<sup>8</sup> Litigation, in other words, is a significant social driver of scientific research in the United States today. Such work cannot be ruled out of court wholesale. Rather, in evaluating its results, judges and juries need to consider not merely the timing and context but also, more importantly, the methods and procedures employed to ensure its validity.

**Review.** Science is subjected to different forms of review in research and litigation contexts. Peer review, as discussed above, includes a remarkably diverse cluster of practices, but they all seek to hold scientific claims to professional standards that reviewers have been socialized into as members of their scientific communities. By contrast, in admissibility proceedings, judges review science in accordance with their personal understandings of scientific methodology, although these may be informed by numerous external sources, such as briefs from the parties, briefs by *amici curiae*, representations by court-appointed experts or special masters, judicial precedents, and pretrial hearings. As citizens of a scientifically advanced society, American judges cannot help acquiring tacit assumptions about the nature of the scientific method and its relationship to lay perceptions. Yet, since these assumptions are seldom made explicit or subjected to critical scrutiny, judges attempting to implement *Daubert* may unwittingly end up functioning as “hidden experts” in the philosophy and sociology of scientific knowledge. Greater self-awareness and formal exposure to the social studies of science provide two possible safeguards against this risk.

At trial, scientific evidence is subjected to still another form of review, through cross-examination. Unlike scientific peer review, cross-examination is an intrinsically unfriendly procedure. It aims to discredit both the testimony and its presenter; it therefore tends to blur distinctions between personal and scientific credibility and may confuse the fact-finder by focusing attention on fairly trivial shortcomings in the quality or presentation of the evidence. Juries (and judges) with relatively little training in or familiarity with scientific research methods may lack the analytic resources to discriminate between serious and superficial contradictions revealed through cross-examination. At the same time, precisely because it is a hostile technique, cross-examination of expert witnesses may reveal biases and assumptions that were invisible to members of their peer communities. Thus, litigation over DNA typing evidence helped to disclose numerous messy practices and questionable methodological assumptions adopted by private testing companies.<sup>9</sup>

**Closure.** Outside the litigation context, scientists may reach closure on particular factual claims for a variety of reasons. Most or all of the relevant research community may believe the claim to be true. Alternatively, they may stop questioning it for a variety of social reasons, for example, because they find it uninteresting, unrewarding, impractical to challenge with existing resources, or temporarily acceptable as a basis for further research. Scientific claims may remain in

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9 See Eric S. Lander and Bruce Budowle, *DNA Fingerprinting Dispute Laid to Rest*, NATURE 371 (1994) 735–738.

a domain marked “provisional” or “not proven” for long periods of time.<sup>10</sup> Within the framework of a lawsuit, however, the pressure to end disputes efficiently may lead courts to reach decisions on the basis of science still deemed provisional by the wider scientific community. This, too, is a form of socially sanctioned closure, but it may give rise to the impression that courts are willing to rely on “bad science.”

**Proof.** It is often said that standards of proof are fundamentally different in science and the law. Thus, proof for scientists generally amounts to something like a 95 percent certainty that a presumed cause-effect correlation is not due to mere chance. Proof in civil litigation, by contrast, requires only a showing that the harm alleged was more probably than not caused by the defendant’s conduct. Overly stringent restrictions on admissibility could imperceptibly ratchet up the standard of proof in civil litigation. It should be noted that the *Daubert* majority viewed the older “general acceptance” test from *Frye* as too restrictive.<sup>11</sup> Nevertheless, some commentators and courts have apparently interpreted *Daubert* as setting a higher threshold for admissibility than *Frye*.

**Uncertainty.** In science, uncertainty is a measure of the range of probabilities that a given causal statement or explanation is in fact the right one. Uncertainty may be reduced by defining a phenomenon more exactly, by accumulating additional data, by ruling out alternate causes, by persuading dissenters, or by formulating new causal models. None of these possibilities may be feasible, as a practical matter, within the constraints of litigation. Moreover, the methods by which science reduces uncertainty in pristine laboratory conditions may be inapplicable in the complex, real-world situations out of which many contemporary mass tort claims arise. New methods of measuring, monitoring, and statistically interpreting health or environmental effects may have to be devised in such cases, and these may prove quite controversial.<sup>12</sup> In practice, then, uncertainty in the litigation context is frequently irreducible.

Uncertainty about scientific findings does not render them invalid for use in litigation. It affects the weight rather than the admissibility of evidence. The choice for courts is to decide where to place the burden of irreducible scientific uncertainty. One approach, in cases where current science is incapable of producing definitive evidence, is to lower the burden of proof for plaintiffs below standards of scientific certainty. This, however, is a choice where the values of the judicial process inevitably come into play; it is not a mere matter of scientific gatekeeping.

**Norms.** The commitment to truth is central to the ethos of science, just as the commitment to justice is central to the ethos of the law. This does not mean that false, unreliable, or misleading scientific evidence has any place in legal proceedings.

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10 Two examples that come to mind are the early findings supporting the theories of global warming and plate tectonics (movement of continent-size segments of the earth’s crust).

11 “That austere standard, absent from, and incompatible with, the Federal Rules of Evidence, should not be applied in federal trials.” *Daubert*, 509 U.S. at 589, 113 S.Ct. at 2794 (footnote omitted).

12 For a controversy of this kind, see STEVEN EPSTEIN, *IMPURE SCIENCE: AIDS, ACTIVISM, AND THE POLITICS OF KNOWLEDGE* (1996) (describing how clinical trial methodologies were contested, and eventually changed, so as to accommodate patients with preexisting disease and under multiple medications).

It does mean that the law may appropriately settle for something less than scientifically accepted truth in order to further the interests of justice. In practical terms, *Daubert* does not exclude the admissibility of studies that are still provisional—that have not, for instance, undergone scrutiny over a period of years or been formally replicated. Needless to say, studies that have been validated in these ways (for example, studies of cancer caused by asbestos or tobacco) should legitimately command greater respect in legal settings than those which have not (for example, studies of neurological effects of chlorine inhalation).<sup>13</sup>

**TABLE 1: SCIENCE IN CONTEXT**

	<b>Research Science</b>	<b>Evidentiary Science</b>
<b>Goals/ Objectivity</b>	basic research/ disinterested	party driven/ interested
<b>Review</b>	peer review	admissibility review cross-examination
<b>Closure</b>	scientific consensus	legal judgment
<b>Proof</b>	>95 percent certainty	more probable than not
<b>Uncertainty</b>	reducible in principle	irreducible in practice
<b>Norms</b>	commitment to truth	commitment to justice

#### **IV. RETHINKING THE JUDICIAL ROLE**

*Daubert*, I have suggested, sets forth not so much a clear standard as an approach that courts should follow in evaluating the admissibility of scientific evidence. Although the majority opinion provides some criteria, it does not spell out in detail—either substantively or procedurally—how scientific validity should be determined, thus leaving considerable scope for judicial discretion. The standards by which judges review scientific evidence, and the procedural steps they take to test and assure the validity of science, will ultimately depend on their underlying conception of the role they should play in resolving knowledge-based disputes. A rehearsal of the major possible judicial roles, together with the assumptions that each makes about science, may assist judges

<sup>13</sup> This was the issue in *Valentine*, cited in n.6 above.

in better positioning themselves with respect to the evaluation of scientific evidence.

**Inquisitor.** This is the classic position of the judge in civil law systems. In this model, experts are appointed by and are answerable to the judge, who also questions witnesses and conducts formal fact-finding. This approach rests on a presumption that neutral or unbiased experts exist and that they can be identified by impartial judges. Its chief virtues are efficiency and the removal of the particular kinds of bias that characterize scientific evidence produced by the parties, such as a preference for extreme opinions and repeat witnesses. Its major drawbacks are the uncritical acceptance of mainstream views, both judicial and scientific, and the failure to acknowledge or question the socially constructed aspects of scientific testimony.

**Gatekeeper.** This is the role envisioned by *Daubert*. It presumes, as discussed above, that science operates according to objective standards that can be clearly understood and applied by judges. The standards may vary from case to case, but judges are seen as capable of identifying science that is so substandard as to merit exclusion. This approach encourages judges to construct their own rules and procedures for screening scientific evidence, building on their tacit assumptions regarding the nature of scientific inquiry. Thus, the Ninth Circuit's decision in *Daubert*, on remand from the Supreme Court,<sup>14</sup> applied criteria that were not considered by the Supreme Court (e.g., the distinction between pre-litigation and post-litigation research). Similarly, a federal judge in Oregon appointed his own panel of supposedly impartial experts to assess the validity of scientific claims in breast implant litigation.<sup>15</sup> A major risk of the *Daubert* approach therefore is inequity, as litigants with similar complaints are subjected by gatekeeping judges to substantially different evidentiary standards and validation processes. Another risk is the temptation to defer to a mainstream scientific approach that may tend toward exclusion of theories the Supreme Court, in *Daubert*, called "well-grounded but innovative" or "too particular, too new, or of too little interest to be published."<sup>16</sup>

**Referee.** The pressure to settle cases may make trial judges adopt the role of referee with respect to offers of scientific evidence. In this role, judges are likely to view the parties' scientific claims as driven by interests and contaminated by bias. Instead of screening the evidence according to "objective" criteria of scientific validity, however, a refereeing judge will attempt to use perceived weaknesses in the parties' scientific arguments to steer the litigants toward settlement. This, arguably, was the strategy adopted by Judge Jack Weinstein in settling the Agent Orange litigation.<sup>17</sup> One risk, as evidenced in that case, is that judges may assess the validity of scientific testimony,

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14 See n.7 above.

15 Hall v. Baxter Healthcare Corp., 947 F.Supp. 1387 (D.Ore. 1996).

16 Daubert, 509 U.S. at 593, 113 S.Ct. at 2797.

17 See In re "Agent Orange" Product Liability Litigation, 597 F.Supp. 740, 795 (1984): "Many of those who testified at the Fairness Hearings were of the opinion that further studies and a full trial would reveal more evidence supporting their causality claims. Defendants' predictions are to the contrary. . . . But based on present data it appears unlikely that such proof will develop in time to affect this litigation. . . . In conclusion, all that can be said is that persuasive evidence of causality has not been produced."

and the reliability of particular experts, in accordance with their own intuitive understandings of science and with little or no reference to the actual views or practices of the scientific community. Another risk is that they will apply markedly different standards of skepticism toward expert testimony offered by plaintiffs and that offered by defendants.

**Mediator.** This is a variant of the refereeing role and it also fits well with the desire to settle cases. However, the mediating judge does not decide for herself how much weight to accord to different offers of scientific testimony. Instead, she may shape the discovery process and other pretrial proceedings so as to promote a sharpening of the scientific issues and, where possible, a negotiated resolution of significant scientific disputes. This procedural approach has the merit of allowing the parties to assess the strength or weakness of their own positions and to make decisions accordingly. Its principal demerits are that it arguably rejects any attempt to establish scientific truth and that it makes no allowance for gross resource differences between the parties.

**Judge.** Ultimately, there is no substitute for exercising judgment in reviewing scientific evidence, as in all other aspects of judicial decisionmaking. Judgment is needed because, despite *Daubert's* suggestions to the contrary, there are no hard and fast rules for evaluating scientific validity. Judges need to reject mythical versions of both “pure science” and “junk science.” They have to develop a sixth sense for ways in which bias creeps into scientific inquiry and for the differences between legitimately different viewpoints and truly marginal forms of inquiry. Most of all, they need to retain the conviction that courts are not a forum for resolving scientific disputes definitively, but rather for doing justice on a case-by-case basis with the aid of all available scientific knowledge that meets threshold tests of relevance and reliability.

## **B. ADDITIONAL ORAL REMARKS OF PROFESSOR JASANOFF**

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I was looking for an anecdote to begin this morning’s discussion with, and the unlikely pages of the *New York Times Book Review* provided one to me. About two weeks ago, there was a review by a distinguished federal judge of a book called *Reason and Passion*<sup>1</sup>, which is about Justice Brennan’s jurisprudence. In the course of that review, the author—who needless to say, did not approve of Justice Brennan’s jurisprudence—said, “Recently, during a discussion of a difficult criminal case, one of my law clerks suggested a rule that might make sentencing more rational. After hearing him out, I responded, ‘That might be a good rule, but where are you getting it from?’ I meant, where in the Constitution, in statute or in case law.”

The distinguished author of that passage was Alex Kosinsky, who was also the author of

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1 E. JOSHUA ROZENKRANZ & BERNARD SCHWARTZ, EDs., *REASON AND PASSION: JUSTICE BRENNAN’S ENDURING INFLUENCE* (Norton, 1997).

the decision in *Daubert IV*.<sup>2</sup> The position he takes in that review is admirable in its understanding of judicial restraint. But one might ask Judge Kosinsky and indeed, all judges in the wake of *Daubert*, “When you construct rules of reliability for science, where are you getting those rules from?” The Constitution, statute, or case law work less well in these contexts. Accordingly, the courts seem to have been thrown into a kind of free-floating parachute fall with regard to constructing rules of reliability after *Daubert*.

The paper that I wrote, and the highlights from it that I want to pick out, essentially go to this question. When judges are trying to decide what science is like, and therefore import into legal decisionmaking rules about what makes some science reliable and other science not, they are no more free to construct their own rules than if they are constructing rules of law. Anybody who is in the business of lawyering or judging has had a lifetime of training about where to look for rules of legal import. Similar kinds of training with regard to science obviously are not part of our legal training.

*Daubert* puts this problem rather starkly, because the case has been widely interpreted as saying that judges do have a place to turn for rules of reliability. To be sure, they don’t have the Constitutional, statute, or case law, but all they have to do is look to science itself and to “think like scientists.”

That is a rather risky kind of position to start off from, for a number of reasons, and I want to go through those.

1. First, to say judges should “think like scientists” suggests that science is a monolithic kind of activity subject to one clearly defined set of rules by which reliability is judged. Even the common-sense notions and understandings of science that we all share immediately suggest that that is a highly misleading view of the scientific enterprise.

Obviously, all scientific inquiry has some properties in common. Science prizes intellectual rigor, consistency, coherence, and logical thought, and findings are constrained by evidence. But it could be pointed out that, in any society that sees itself as rational (and ours eminently does), it is not just science that is subject to those constraints. We are capable of judging the plausibility of all kinds of stories by criteria that science is held to.

To the extent that science does appear to have universal criteria of reliability attached to it, those criteria are universally applicable to rational thought, whether or not it is science to start with.

One of the points worth keeping in mind when one judges scientific reliability is that perhaps science is accorded too much specialness by the legal system. Some of the rules to which we must hold science are the same rules to which we must hold any other kind of evidence, not just scientific activity.

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2 See Background section, n.10.

Further, all science obviously does not operate according to the same sort of progression from observation to finding to conclusion to fact. To some degree, Justice Blackmun seemed to have in mind the textbook example of the scientific method that all of us have been exposed to from grade school onward and that runs something like this: Reliable science or valid scientific inquiry begins with a theoretical framework or paradigm. Within that framework, one can construct hypotheses. In order to test those hypotheses, one develops experiments, and one carries out those experiments. If those experiments can be replicated, then the experimental results are entitled to weight. And we can believe in general experimental results that have repeatedly been replicated in some way or another.

In the litigation context, however, science rarely progresses in that way, partly because the kinds of issues that are litigated scientifically in courtrooms do not start from preordained theory. Typically, the kinds of issues that come up in a scientific context in the courtroom begin with some kind of phenomenon that has been observed, and a *post hoc* attempt is made to construct a plausible explanation for why that phenomenon seems to operate in the way it does.

The typical phenomena in personal injury cases are occasioned by an alleged exposure to a toxic substance or a pollutant or whatever. The phenomenon is there before the causal story is there. To a large extent then, the kind of science we deal with in courts is a science that attempts to construct a theoretical explanation *after* the fact of observation.

In the scientific world, there are areas of inquiry that proceed much in the same way as well. The last time I made this point, a scientist from the audience said, "But isn't that true of something like, say, AIDS, where we noticed the phenomenon first and found the cause afterwards?" Or "Isn't that true of a phenomenon like superconductivity, where we noticed materials behaving in a certain way, and then went about finding the theoretical explanation afterwards?"

My answer to that is yes, but all this bears on the rather general, legally significant point that all scientific activity is not monolithic. Therefore, the idea that *Daubert* seems to propose, that there is a single coherent set of rules of reliability against which judges can hold scientific evidence, simply does not work. One has to look at the nature of the science that is being proposed and being done, and to some degree fit the rules of reliability to the nature of the scientific activity itself.

2. The second problem with the "thinking like scientists" injunction is that scientists don't operate according to a set of abstract norms. They operate within a social context, which has a lot to do with the determination of the way science works.

The social context in which science is done for purposes of litigation and the production of evidence differs in some notable ways from the social context for university-based, academic, so-called pure or basic science. Let me just run through those contextual differences, dwelling a little more on some of them than on others.

a. To begin with, the goals and the criteria for determining objectivity in research science are fundamentally different from the goals of evidentiary science, as I have called it in the paper. Basic research, university-based basic research, is at least in theory supposed to be disinterested. It is not supposed to be geared to producing a particular kind of argument in support of a particular set of results. Evidentiary science is not that. Evidentiary science is supposed to present the strongest possible argument for a causal assumption that somebody wants to represent in court. To be sure, it has to be plausible, but nevertheless, it is not from the beginning a disinterested kind of scientific activity.

b. Secondly, in research science, the reliability of findings is supposed to be assured to some degree through a process called peer review. Since *Daubert*, and since the scrutiny of peer review that was initiated by *Daubert*, I think most people in the legal profession are aware that peer review is an imperfect kind of screen. It doesn't assure scientific reliability. It is a cover term for a lot of different kinds of review procedures that operate in the grant awarding system and in the journal publishing system.

Still, the nature of peer review is very different from the kinds of review that courts carry out, either at the stage of admissibility hearings or through cross-examination.

The general point I want to make is that one can't have a blanket notion that peer review is or is not stronger or more reliable as a filter for legitimate science than admissibility review or cross-examination. There are some respects in which peer review may be a more reliable filter; there are some respects in which cross-examination may be a more reliable filter.<sup>3</sup>

c. Thirdly, the **closure mechanisms** by which one reaches final conclusions in research science are very different than those by which we reach final results in a legal context. Scientific consensus is thought to be the normal way by which scientific controversies and debates within science reach closure, and that can take enormous lengths of time—for instance, several hundred years in order to show whether the mathematical hypothesis called “Fermat's Last Theorem” really was a valid theorem or not.<sup>4</sup> Legal judgments don't have that luxury. When we do afford ourselves the luxury of lots of time, we are accused of monumental inefficiency and delay, as in *Bleak House*.<sup>5</sup> Therefore, the concluding event in evidentiary science is the compelling need to produce some kind of legal judgment.

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3 I'll just give two examples. Peer review on the whole does better at holding science to communal standards that are applicable within a scientific community. Cross-examination on the whole does better at eliciting personal biases such as ties to funding sources and possible inconsistencies in the positions held by scientists in different contexts of work.

4 See the Background section of this report.

5 In this novel by Charles Dickens, a dispute over inheritance drags on in the courts for decades until the inheritance is completely used up in legal costs.

d. **Proof** is another area where the social organization and practice of science is carried out under different presumptions from the presumptions that govern evidentiary science. On the whole, scientists tend to say that they have proof when there is a higher than 95 percent likelihood that the connection that science observes is not accidental.

In evidentiary science, in civil litigation, the standard is “more probable than not.” If one insists on attaching numbers, that is something like a more than 50 percent likelihood, not a more than 95 percent likelihood, that the connection is not accidental.

A question that I just want to throw out for the moment, and that will be discussed throughout the day, is what happens with an injunction to “think like scientists” when the evidentiary standards in civil litigation and in science are so different from one another.

e. **Uncertainty** is the next point that I want to dwell on. This is a rather important point, because here there really are entry points for the normative concerns of the law.

Scientists always say that all science is uncertain, all science is provisional. Even claims that are considered to be facts today may be altered in the light of large-scale shifts in the scientific paradigm within which people are working, as new findings emerge from unexpected sources. In any case, scientists do believe that scientific uncertainty can be reduced through systematic inquiry.

With evidentiary science, however, the notion that more research will ultimately lead to the correct answer is not necessarily right. This is a place where scientists and lawyers, I think, have very different points of view. It is therefore worth underscoring this.

Why do I say that uncertainty in the evidentiary context may be irreducible in practice? The reason is that the circumstances in which laboratory-based science is produced are really fundamentally different in many cases from the conditions out of which evidentiary science is generated. All of us saw illustrations of this in the course of the O. J. Simpson trial, where the difference between DNA testing carried out in a lab or a medical context and DNA testing carried out in the forensic context did indeed become an important issue.

But there are more subtle ways in which uncertainty in the evidentiary context looks rather different from uncertainty in the lab context. Usually when we are dealing with causal claims involving mass torts, we are dealing with conditions that do not lend themselves to replication in the lab. We are dealing with people with multiple exposures, people who are very variable in the nature of their exposures, and in the nature of their responses to such exposures. We cannot overlay a kind of lab-based controlled academic study on the kinds of situations out of which mass tort claims arise. Therefore, the notion that one can do research and thereby reduce the

uncertainty of the situation out of which a tort action has arisen is sometimes a misplaced kind of confidence. It may not be the case that lab research actually replicates the situation out of which the tort claim grew.

f. The final point leads into the issues about the judge's own conception of the judge's social role. This is **the normative point** that we repeatedly see mentioned in writing about law and science. One often sees statements to the effect that "science is committed to truth, law is committed to justice."

I think that this is a very stark way of putting it. It must be clear from the very first comments I made that it is not possible to have truth without also paying attention to certain norms of justice. We tend to think on the whole that giving all sides of an issue a fair hearing is a way of getting at both truth and justice. So I don't mean to suggest that the norms of science and the norms of law are totally different from one another.

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*There are two major problems with "thinking like scientists." One is that not all scientists think alike: science is not monolithic. The second is that scientists don't think in an abstract realm of ideas. They think in a social context.*

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But the way in which the issue of justice relates to uncertainty is that justice may need to be done even when the scientific story cannot be closed off, cannot be answered definitively. This is a point that I think many scientists writing about the legal process tend to forget.

I have suggested that there are two major problems with "thinking like scientists." One is that not all scientists think alike: science is not monolithic. The second is that scientists don't think in an abstract realm of ideas. They think in a social context, and the social context is different for evidentiary science from the way it is for research science. There is a third point. That is, that for judges to "think like scientists" might require judges to give up certain other kinds of role conceptions that are more appropriate for judges. In the last part of the paper, I call attention to this third set of issues.

*Daubert*, again, has been widely interpreted as requiring judges to be gatekeepers. First of all, the idea of gatekeeping is founded on a notion of the legal process that takes the trial as its model. Gatekeeping into what? Gatekeeping into letting issues get in front of the jury. But as we all know, something upwards of 95 percent of cases actually don't get to that point. Therefore, in that vast majority of cases, it becomes very significant to wonder about the ways in which judges will be conceptualizing their own role in looking at the reliability of scientific evidence.

Although *Daubert* says, "Be a gatekeeper," my sense is that carrying *Daubert* out to the fullest would push American common law more in the direction of the European civil law model, where the judge acts as **inquisitor**. Some of the reforms that have been proposed, both immediately before and in the wake of *Daubert*, would actually say to judges, "Increase your powers that you already enjoy under the Federal Rules of Evidence or their analogs in the states. Go ahead and appoint your own experts

and create a neutral space for expertise somewhat similar to what people do in the civil law system, where the judge's role is that of inquisitor."

One of the problems with that kind of approach is that it tends to replicate mainstream, dominant views of science within the legal process and does not subject them to adequate questioning. I am sure we will hear more about this in the course of the day.

### THE CUTTING EDGE

You will recall that one of the points I am making about the difference between evidentiary science and research science is that evidentiary science, almost by necessity, is at the cutting edge. It is asking about phenomena that have not been studied in detail. So a court that appoints its own experts in an inquisitorial role runs the risk of finding experts who are not really doing the kind of science that is before the courts.

The second role, the *Daubert* role, is that of **gatekeeper**. I think the problems with that role have been adequately discussed.

In the settlement context, judges may wish to conceptualize their roles in rather different ways. The two that I have called attention to are either **referee** or **mediator**. Both have costs and benefits attached to them. On the whole, what I am calling the referee role is the more judicially active. The judge, as referee, takes an active role in seeing which kinds of evidence deserve more weight or less weight. The judge also takes steps to construct the procedural framework in which scientific issues are discussed so as to privilege the issues that the judge considers more reliable and to underprivilege the others. The mediator, on the other hand, is a more laissez-faire kind of role. A mediating judge leaves it up to the parties to decide which issues they themselves want to hammer out and which ones they really want to fight for. With Judge Bernstein as one of the commentators, I am sure that we will hear more about this role.

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*Even though Daubert suggests that judges can separate themselves from the difficulty of exercising discretion with regard to scientific evidence, they cannot rely on science itself to generate and produce its own criteria of reliability.*

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### JUDGING

The final role, of course, is the one that actually has brought most of the judges to this Forum, and that is judging. In the end, even though *Daubert* suggests that judges can separate themselves from the difficulty of exercising discretion with regard to scientific evidence, they cannot rely on science itself to generate and produce its own criteria of reliability.

Once one undertakes the kind of analysis I have proposed, it becomes clear that one cannot look to science for the solutions to legal complexity and legal problems. Therefore, ultimately, the social role that I think the judiciary will be thrown into, like it or not, is that of judging. Judges will not be able to shrug off detailed analysis of the scientific enterprise that has to be done in order to decide, case by case, whether particular offers of evidence are reliable or not.

### C. COMMENTS BY PANELISTS

ANTHONY Z. ROISMAN, PROFESSOR MARGARET A. BERGER, AND  
JUDGE MARK BERNSTEIN, RESPONSE BY PROFESSOR JASANOFF,  
AND EXCERPTS OF FLOOR DISCUSSION

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ANTHONY Z. ROISMAN, LYME, NEW HAMPSHIRE

I too thought it might be good to start with something light. It occurred to me that there is a scientific-type question with which we are all familiar, that has evolved over the years. It is the same question, and like all good abstract science, there has been no answer. The initial question was “If a tree falls in the forest and there is no one there, does it make a noise?” In the modern era, that question has evolved into another one: “If a tree falls in the forest and the press isn’t there to cover it, did it happen?” Then recently, across my Internet screen came this: “If a husband expresses an opinion in the forest and his wife isn’t there to hear it, is he still wrong?” We have in the law similar abstract issues to which we never get an answer. The one that always puzzled me most was “What exactly is a *scintilla juris*,<sup>1</sup> and does it exist?” I remember it from something that I heard in a first-year law class.

But I want to talk today about the more practical sides of science and the law, because I think what Professor Jasanoff has done is to lay a foundation for thinking about science as a real-world activity—not an abstract activity, but something that is in the real world.

There will be pieces of the discussion this afternoon about Professor Gottesman’s paper, in which the intersection between the real world of science and the more abstract kind of science becomes even more important. The doctor who treats a patient is doing a very different kind of scientific process than the doctor who is working in the research labs, writing the papers that are published in the peer-reviewed journals that the doctor reads and uses in deciding what to do with the patient.

I think for purposes of the law, the law is almost always concerned with that practical side, the practical aspects. I will attempt to follow the scientific process and prove a thesis. I submit that the scientific thought process and the legal thought process are essentially the same. I further submit that, if that initial thesis is correct, it makes the

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1 “In real property law, [a *scintilla juris* is] a spark of right or interest. By this figurative expression was denoted the small particle of interest, which, by a fiction of law, was supposed to remain in a feoffee to uses, sufficient to support contingent uses afterwards coming into existence, and thereby enable the statute of uses (27 Hen. VIII, c. 10) to execute them.” BLACK’S LAW DICTIONARY 1207 (5th ed. 1979).

process of evaluating the admissibility of scientific evidence much easier.

First, essentially both processes, scientific and legal, involve these steps: gather all the relevant data, organize it, review it, apply deductive and inductive reasoning, reach a judgment, and explain the basis for the judgment. That is the scientific method and it is also the legal method. It is what we generally call a “rational thought process.” It excludes jumping to conclusions without a basis, speculation, guessing, coming up with something because you had a bad dream, or any of those other things that we would say are not acceptable. I think we have some examples of that in the world that we see all the time.

Let’s look at the law side. On the law side, how does the judge decide what the legislative intent is of a piece of legislation? The judge goes through a process that has a set of rules. The rules say that a committee report has more weight than the floor debate. They say that the majority opinions in the report have more weight than minority opinions. However, even when the rules are followed, we are really allowing certain fictions to exist and making certain assumptions. Anyone who has had any experience at all with the legislative process knows that the idea that what is written in a committee report actually reflects the legislative intent of any significant number of the legislators is one of the greatest fictions in the world. Yet it is used all the time to decide what a piece of legislation means.

The scientist also has rules for how much weight to give each piece of data, uses assumptions to compensate for imperfect knowledge and, in the last analysis, uses judgment and experience to reach conclusions based on the data. For example, it has been a bedrock of Euclidian geometry that parallel lines do not meet. Of course, no one has ever been able to extend parallel lines to infinity to test that hypothesis. For decades scientists accepted Einstein’s theory of general relativity in conducting scientific experiments although there had not been a scientific test of its validity.

One rule that is equally applicable to science and the law is that the outcome of an inquiry is substantially influenced by the precise question that is asked. *Frye* and *Daubert* are good examples of the problems created when the wrong question is asked. *Frye* was a case about the admissibility of the results of a polygraph test based upon an analysis of the ability of a machine to reliably determine whether a person was telling the truth. *Daubert*, on the other hand, was a case about the admissibility of opinions concerning the causal connection between exposure to a drug and an adverse health outcome.

Although the general principles of science noted above are applicable to both cases, the specific scientific concepts that apply are very different. For example, in testing the reliability of a machine, the error rate of the machine—i.e., how often the machine produces the wrong result—is not only relevant but easily ascertained. A test can be conducted with persons answering questions for which the true answer is known but not always given, and the rate at which the machine detects the “lies”

can be ascertained. No similar test of an error rate can be used where the question is whether exposure to toxic substance A produces adverse health outcome B. Controlled tests would require deliberate exposures of significant populations to toxic substances. Unfortunately, the Court in *Daubert* did not fully appreciate that difference, apparently mistakenly thinking that the question before it about cause and effect was no different from the question before the *Frye* court.

Despite this failure to fully recognize the difference between the specific scientific questions relevant to the admissibility of polygraph evidence and the admissibility of evidence about cause and effect, the underlying principles of *Daubert*—to identify the methodology appropriate to the inquiry involved in the case and determine whether the expert followed that methodology (including the use of scientific reasoning) in forming an opinion—are the appropriate starting points for the admissibility inquiry. However, one vestige of *Frye* that persists, albeit with much less emphasis in *Daubert*, is the issue of general acceptance.

Again there are major differences between determining whether the output of a machine is sufficiently reliable to be admitted in a criminal case and whether a scientific opinion about cause and effect is admissible. In the former, the widespread use of the machine or, conversely, the very limited use of the machine bears directly on the reliability of the machine. If the machine were reliable and efficacious there would be no reason not to use it. However, in tort litigation we are often dealing with a product or substance that has been blessed by mainstream science or at least has apparently passed muster based on mainstream scientific analyses. If mainstream science or its processes had concluded that the product or substance was dangerous, it would not have been marketed by any reputable company. Thus, in the tort context, the proof of the hazard will inevitably come from persons who are less likely to be part of the mainstream and whose approach is less likely to be “generally accepted.” In such cases, such as litigation over Bendectin or widely used chemicals, the emphasis should be much more on the scientific method that underlies the opinion, not on whether there is widespread scientific consensus about the approach used to render the opinion.

The importance of defining the question in order to approach answering that question properly is also relevant to the basic question that now faces all courts. What should be the test that is used to determine how deeply a court probes in deciding the admissibility of an expert opinion? Scientists would look for analogies in their scientific experience to find situations similar to the one presented. Given the “liberal thrust” of the Federal Rules, favoring admissibility of expert opinions, and the Supreme Court’s emphasis on that principle in *Daubert*, an analogy should be sought in an area of the law where courts generally provide substantial deference. One such area is the deference given by appellate courts to the factual findings of juries.

I propose, as a hypothesis for further discussion, that the standard to be used by a trial court in deciding how far it should probe in order to determine whether an

expert opinion is admissible could be the standard an appellate court would use in deciding whether the factual determinations of a jury should be overturned. I would be interested in hearing from all of you during the discussion session whether this standard of deference, or another one, best captures the dual role of being a “gatekeeper” and assuring the broad admissibility of all relevant evidence mandated by the Federal Rules and by *Daubert*.

So I submit as a *thesis* that science and law think the same way. The *hypothesis* is that we have some analogs in the law that will help us understand how to determine when an expert’s opinion is admissible and when it is not, and that we should look for those analogs and use them as a way of testing the admissibility of expert opinions.

**PROFESSOR MARGARET A. BERGER, BROOKLYN LAW SCHOOL**

I would like to make a few comments that pick up on both what Professor Jasanoff said and what we have just heard from Tony Roisman.

The first thing I would like to point out is that when we speak of “the *Daubert* test,” and even to some extent “the *Frye* test,” we are talking about too broad a generalization. The *Daubert* test works in different ways, depending on the issue and depending on the kind of case we have.

As was said at the beginning, the Supreme Court in the *Daubert* opinion seemed to be thinking of science as some kind of an empirical endeavor, where one could test and see whether things could be falsified or not. I think that model works quite well, for instance, in the forensic science area. I think that *Daubert* in the federal courts has had some interesting impact on forensic science. For one thing, people have told me that some of the forensic specialties are really going back and re-looking at work they have done in the past in areas like ballistics, handwriting, and a whole variety of fields, because those are fields where one can do some testing to see whether we can really get results that can be replicated, whether we have a theory that works, whether we can really identify something.

I think that the DNA controversy was helped by the *Daubert* test and by what was done in response to it, including what was done by a committee of the National Research Council. I think that controversy has moved toward better science. I suspect that even some of what we have heard about of late, of problems with FBI labs, is perhaps some of the feedback that is coming from taking a more intense look at what these forensic sciences can really produce and offer.

Now, that is a very, very different kind of science from the science we are talking about with regard to toxic tort cases. I think it is the toxic tort cases that are really causing us such enormous difficulties about what to do with science and the courts. That, I think, is so because, as Professor Jasanoff’s paper makes so very evident, there are really no ways of answering conclusively—or even getting a good handle on—

what happens in cases when somebody comes in after the fact and says, “I have a disease—a disease which exists in people who have not been exposed to the substance I have been exposed to, a disease that has a background rate, and we don’t know all that much about what the background rate really is, but I think my disease is due to the fact that I was exposed to the defendant’s substance.”

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*What we are talking about here are not evidentiary issues at all. We are really talking about who should bear the risk in a complicated society where we are all exposed to potentially toxic substances that we do not understand.*

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Why does this issue of causation cause such difficult problems in terms of the science? Well, first, most of the time no science has been done on this issue. The studies that have been done show that, basically, the science starts with the litigation, that the litigation drives the science, which means that there is a terrible time gap. To do any kind of scientific studies takes time, especially if, as *Daubert* suggests, we’re going to try for some replication. We need lots of time. And, of course, there are statutes of limitation running, there is pressure on judges to decide cases, as we know, and here is this science that is just beginning.

Second, it is expensive to do science, very expensive. Where is this money going to come from? Obviously, the group that has control of the money, that has control of the data, tends to be the defendants. How do we get around this very unequal kind of allocation of resources?

Finally, Professor Jasanoff was talking about the “judge” model of dealing with science in judging. I think that in these toxic tort cases, more than in any other kinds of cases involving science, we really have to come to grips with whether we want these cases decided by an evidentiary model, which deals with issues like burden of proof, and how one deals with a problem when we don’t have enough evidence, or whether we want to deal with this as a substantive law matter. I think that the latter is really what we have to do.

What we are talking about here are not evidentiary issues at all. We are really talking about who should bear the risk in a complicated society where we are all exposed to potentially toxic substances that we do not understand. What is fair in our society, given the cost of science, the inconclusiveness of science, the difficulty in doing science, the likelihood that even when science is done it may take 20 years to get an answer? What, under those circumstances, is going to be fair?

I really don’t see that as an evidentiary issue. I really see that as an issue that we are going to have to get to by developing more fully the substantive law of torts, which is what is supposed to ultimately decide how people ought to behave in our society. That, I think, is really the central issue for us here.

So I think that it is ironic that the *Daubert* case comes up in a toxic tort situation. But I find that the *Daubert* test, with its talk about falsifiability and peer review and

other kinds of factors, works much better in areas of science where one could perhaps get a consensus about what scientists can do—testing two substances, or other kinds of issues—than in the area of toxic torts. So I think that we are still at the threshold of seeing how ultimately all of this will play out.

**HONORABLE MARK BERNSTEIN, JUDGE, COURT OF COMMON PLEAS,  
PHILADELPHIA COUNTY, PENNSYLVANIA**

I don't think we can address these issues without maintaining a sense of humility, and a sense of history, and a sense of awe at our responsibility in our jobs as judges.

I have done a lot of speaking to lawyers and a lot of speaking to trial judges. But I speak to this many appellate court judges with great humility, because they are professionals at judging what trial judges say. On the other hand, I also approach it as I approach writing an opinion. Appellate judges can't talk back until later. I hope that some of the things I have to say will stimulate further discussion.

Judges should not consider themselves amateur scientists. We have to retain the proper respect for the jury's fact-finding function. I believe that juries are at least as good at sniffing out the phonies as judges are.

We have to keep in mind our role of conveying the legal concepts and the seriousness of the judicial function to the juries, and specifically the function of the burden of proof. Rather than five words dashed off in the middle of an otherwise incomprehensible charge, we have to find a way of convincing jurors that their oath of office is centered around that concept of burden of proof.

I believe that a trial has to do with education and findings of fact. If counsel is incapable of educating the jury so that the science presented is all a muddle, then the burden of proof dictates that the plaintiff loses. I believe that, if counsel for the defense is incapable of explaining the difficulty, the complexity, the awesomeness of the science, the only reason is that they don't have an adequate lawyer or adequate teachers.

We have to consider ourselves protectors of the jury system, not door guardians for the scientific community and their temporal version of scientific purity. We are asked in *Daubert* to make a subtle shift, but one that has dramatic implications for the concept of burden of proof. The law says the plaintiff's case must be proved by a preponderance of the evidence. They call an expert. That expert must establish legal (or proximate) cause, which in Pennsylvania is defined as "a substantial factor."

Our standard for who they can call as an expert is absolutely unbelievable. It is "any reasonable pretension to specialized knowledge in a field." I say that that means, in

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*We have to consider ourselves protectors of the jury system, not door guardians for the scientific community and their temporal version of scientific purity.*

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effect, “anybody who is reasonably pretentious in a specific area can testify to opinion evidence.” I suggest we should take a more productive approach—trying to come up with a standard that ensures that anyone who is going to be permitted to testify as an expert truly is an expert. If we had a standard like that, we could rely on their testimony to be at least sincere and scientific.

So we give plaintiffs these standards to meet to be able to prove their cases: (1) preponderance of the evidence; (2) proximate cause, or “substantial factor”; and (3) reasonable pretention to specialized knowledge. Then we say, “As an evidentiary matter, the substance of what this pretentious expert has to say cannot be presented to the jury unless the threshold question of scientific certainty is established.” In some fields, it has been proposed it has to be established to a 95 percent of confidence interval. This fundamentally changes the burden of proof! The standard should be that the expert needs to testify to only a reasonable degree of scientific certainty. In some jurisdictions, experts can just offer comments without even having to offer an opinion. But if we use the standard of “a reasonable degree of scientific certainty,” are we grafting onto that definition concepts of scientific *conformity*, scientific *uniformity*, or scientific *certainty*? And if so, are we not then letting the gods of science determine what is legitimate and illegitimate for presentation in the courts?

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*We as judges are in the business of keeping society together. We are in the business of helping people resolve their disputes justly, and we are in the business of making self-help unnecessary.*

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I believe that, if we do that, we are abdicating our judicial function. Science deals in ultimate truths, however long it takes to get there. The history of science is one of constantly reappraising and constantly changing and overthrowing the orthodoxy that new thoughts, new observations determine no longer matches the observed facts. Scientists, after all, work in an environment knowing that, within a hundred years, they will be superseded. Ptolemy’s eternal truth was superseded by Copernicus’s, and then by Kepler’s, and finally by Einstein’s. As we look back over 2,000 years of the progress of science, it seems absolutely natural. But would we have permitted Copernicus to testify against the established scientific knowledge of Ptolemy when he first got his novel thought?

We as judges are in the business of ascertaining truth, but for a very different function. *We are in the business of keeping society together.* We are in the business of helping people resolve their disputes *justly*, and we are in the business of making self-help unnecessary. We have malpractice litigation so that the father of the baby who died in the hospital doesn’t get a shotgun and go back to find the doctor. Since justice is our business from both sides, we also have a statute of limitations that, in effect, says, “Merck drug company, if they can’t prove a case within two years, you can destroy your records. You don’t have to worry. You can close your books on those ingestions of drugs.”

I think these considerations say we can’t let “establishment” science decide when

the courtroom doors will be open and shut. Epidemiological proof might be the only absolute “scientific” proof of causation, but epidemiology requires human guinea pigs. The truth about thalidomide could not be established by epidemiological evidence until 8,000 babies were born with stunted arms and legs.<sup>2</sup>

In closing, let me give two examples that I find compelling:

The first is a letter by Dr. Smithels to Merrell Dow,<sup>3</sup> in which he says

Much clearly depends upon the value of this publication to Merrell Dow National Labs. If it may save the company large sums of money, large sums in the California court, which is rather what I thought when we undertook this study, they may feel magnanimous. If, with the passage of time, the study is of no great significance, I can only regard the figure you suggest as generous and welcome.

Needless to say, I should appreciate any gesture Merrell felt inclined to make. But I imagine that if we are able to give Debenex<sup>4</sup> a clean bill of health with regard to teratogenesis, this would be of substantial help in the courtrooms of California.

The second is an item that concerns peer review. To those who say that peer review is the *sine qua non* of proper science, I suggest they look at the peer-reviewed journal *Spine*. It is an international journal for the study of the spine. It has an editorial board made up of scientists from across the world. In the April 15, 1995, supplement, Volume 20, they report on a study of a Quebec task force,<sup>5</sup> saying

The report of the Quebec task force on whiplash-associated disorders scores a victory for spine science. It will serve as one milestone applying clinical epidemiology to clinical practice, the rules that distinguish truth from fashion.

This report is an indictment of the literature. From an inception pool of more than 10,000 publications, the task force found only 346 worthy of consumption. The task force found the literature wanting. It could not even complete a table of the number of good studies per topic, as it did for low back pain.

How can we explain a peer-reviewed journal that says that peer-reviewed, reported studies are not good science? Well, if we subject the article to cross-examination, we can find that their conclusion is to recommend a temporary moratorium on all medical payments for physiotherapy for patients, because that would provide the funds needed to pursue the research called for in the report.

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2 See Professor Gottesman’s paper *infra* n.28.

3 Presented as an exhibit in the case of *Blum v. Merrell Dow Pharmaceuticals, Inc.*, Philadelphia County, Pa., Court of Common Pleas, Civ. Div., No. 1027 (Sept. Term 1982). For a later decision in this case, see 705 A.2d 1314 (Pa. Super. 1997).

4 Debenex was the name under which Bendectin was marketed in Great Britain.

5 Walter O. Spitzer et al., *Scientific Monograph of the Quebec Task Force on Whiplash-Associated Disorders: Redefining “Whiplash” and Its Management*, SPINE, Apr. 15, 1995 (supp.).

If we subject it to a little more cross-examination, we find that it was funded by the *Société d'Insurance Automobile de Québec*! So we know who paid for the study and the article.

We must be very cautious when we decide that we can interpret the science well enough to say the jury doesn't get to hear the evidence. I think we have to approach our duties in this area very, very cautiously.

#### **RESPONSE BY PROFESSOR JASANOFF**

Those were extremely interesting comments. I would like to begin with what **Professor Berger** said, because I think she expressed a wish that many of us who have been observing the toxic tort realm for the last 20 years have also come to feel.

In the ideal world, we might indeed have a change in substantive law that would suddenly make sure that the third of our citizenship that is not subject to appropriate health care coverage would indeed get covered, and that there would be fair monetary allowances made for people with long-term disabilities, and that these compensation systems would not necessarily have to be tied to an evidentiary determination in a court of law, which all of us know is a relatively inefficient way of transferring money from one source to another. I say "relatively," because there are some doubts about that.

Unfortunately, we don't live in a world where that sort of substantive law change appears to be likely to happen. I'm sure that Professor Berger will agree with me that the difficulty that people in this audience and on this panel face at the moment is that one will have to continue to treat these disputes on an evidentiary basis unless and until substantive law changes, such that new incentives or disincentives are created against people bringing toxic tort claims into the courts.

So the question for us is, How should evidentiary disputes be handled during this interregnum when our society seems incapable of altering substantive law in a way that fully matches the complexities of the technological universe that we live in?

My problem with *Daubert* is that in a sense it appears to usher in a change in substantive law—that is, a change in decisions about who should bear the risk and what is the fair way to allocate burdens—under the guise of a change in evidentiary assessments. That, I think, is the real crux of the *Daubert* dilemma. What it does is to insulate the power to make evidentiary judgments at one level rather than another level of the legal system. It is a decision that pushes toward the elimination of diversity, even though I think all of our three panelists have eloquently spoken to the need for case-by-case discretion in the evaluation of evidence, because the context out of which the science arises really is very different from one case to another.

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6 The story of the Woburn toxic tort case has been told comprehensively in JONATHAN HARR, *A CIVIL ACTION* (Random House 1995). See also Background section, n.18.

7 See remarks of Attorney General Woods on this subject, section III of this report.

The Société d'Insurance Automobile de Québec is a different kind of entity for generating science from, let's say, a victims' group in Woburn, Massachusetts<sup>6</sup> or the tobacco company trade associations of America.<sup>7</sup> So the discretion to preserve some space for judging the quality of science case by case is something that we ideally would want to preserve in the legal system. *Daubert* seems to suggest that one should narrow that range of discretion, and that, I think, is a risk that all the members of this panel would worry about.

**Tony Roisman** talked about lawsuits as a challenge to mainstream science. There is one respect in which I would disagree, but another respect in which I would very profoundly agree, and that would make a bridge then over to Judge Bernstein's comments as well.

When Tony says that the Bendectin litigation is a challenge to mainstream science, of course, like any accomplished and brilliant trial lawyer, he has prejudged the issue for us. We now know that Bendectin stands inculpated at the court of law, and has indeed caused these injuries, and the mainstream science (or technology, if you prefer) that gave rise to products like this is to blame. If that is the interpretation that Tony Roisman intends, then of course I have to part company with him, because of course it is not the case that mainstream science and technology, without further ado, have necessarily produced a defective product. This is the thing that the court of law is actually wishing to ascertain.

On the other hand, there is another way in which one might interpret his comments about challenges to mainstream science so as to make a provocative and important point. That is, when Bendectin has been administered to masses of unwitting subjects—experimental subjects, in effect—we have had an uncontrolled *social experiment* or study conducted, not under the kind of rubric that science normally would use for the carrying out of these kinds of tests.

If we think about drug regulation at the Food and Drug Administration, there is an extraordinarily careful, controlled set of premarket regulatory procedures, clinical trials—several phases of them—before we actually release a drug on the market. Those studies are extremely limited. They are limited as to the study populations. The study populations have to be carefully selected so that they don't include the most vulnerable members of the population, so that they don't represent the full variability of people who will actually be subjected to a drug once it is released on the market. So, in effect, after the release of any pharmaceutical product, we have a kind of uncontrolled experiment that is going on. Mainstream science is rather bad at interpreting the results of the kind of study that was done, in effect, without a protocol and without some kind of controlled approach to studying it.

So in the toxic tort situation, what we have is a kind of uncontrolled social study. People have been exposed, they have lived in complex and difficult surroundings, and then we turn to something called mainstream science to come and sort this out for us

and help us to understand the question: Is it appropriate to recognize cause-and-effect relationships in this setting? That, I think, is a challenge to mainstream science. If that is a fair reading of Tony's statements about Bendectin, then I think we are in total accord.

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*I have heard mainstream science referred to as that which large corporations think is good to protect them. On the other side of it, I might say that mainstream science is what the majority of practicing scientists think is right. How do we know in a court of law if the phrase "mainstream science" really is what a majority of people think is right?*

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That also brings us to **Judge Bernstein's** remarks, because of course Judge Bernstein eloquently pointed out the kind of mainstream science that swings into operation after a terrible event such as a toxic tort has occurred. This is a point that Professor Berger also underscored. It is a very special kind of mainstream science. It is mainstream not just because "mainstream" scientists happen to do it, but because most of the money spent on science is devoted to research that is already labeled as mainstream.

Peer review, about which we have heard a great deal, both before and after *Daubert*, is classically not a very good way of reaching into the social and political and economic interests of the investigators who are carrying out research. Peer review is quite good at getting into the consistency between a scientific approach or procedure and the background scientific orthodoxy against which the findings of research ought to be judged.

The strongest reason that Judge Bernstein has proposed—and here I thoroughly agree with him—for preserving quite a lot of power in the cross-examination process is that it is the only process we have for getting into the social presuppositions of mainstream science—for asking questions like, Was there even the effort made to generate a different kind of story from the one that appeared in the *New England Journal of Medicine*? How is it that the *Journal of the American Medical Association* and the *New England Journal of Medicine* have published only certain kinds of stories? Where might stories of a different sort, scientific stories, have gone? Are there outlets,

are there peer-reviewed places where these kinds of alternative science papers might appear? Then we would find that mainstream science is often a cover term for, among other things, a *social power structure* that needs to be questioned as well. To that extent, I am completely in agreement with Judge Bernstein.

#### QUESTIONS FROM THE FLOOR

**Question by Barry Nace, Esq., Washington, DC:** One of the things that I would like to address to Professor Jasanoff is this question of mainstream science. On one end, I have heard mainstream science referred to as that which large corporations think is good to protect them. On the other side of it, I might say that mainstream science is what the majority of practicing scientists think is right. How do we know in a court of law if the phrase "mainstream science" really is what a majority of

people think is right? How do we handle that?

**Professor Jasanoff:** Well, I have been foremost among the people who have argued that mainstream science is the product of a particular group of people who wish to use that label to characterize something or other as mainstream science. The terminology “mainstream science” carries an incredible amount of authority in our social system because it suggests that this is the closest we can get to truth and therefore is something that we ought to adopt as the basis for making decisions.

In a court of law, I think that the major ways in which people can ascertain mainstream science is to ask questions of the experts who have come into court about the extent to which their ideas have been subjected to other forms of scrutiny. On the whole, it is not a bad rule of thumb to use the term “general acceptance.” This rule points to something more like mainstream science than to the views of an isolated expert operating out of association with any kind of scientific community. The problem is, in many of the kinds of cases that litigators deal with much more directly than I do, it is mythical to think that there might even be a mainstream science, because there likely was not a science on that particular issue until the case came to court. Once the case came to court, all the evidence was being generated in the pursuit of one interest or another that is connected with the litigation. So I think we should be aware from the start that it may be impossible, it may be mythical, it may be misleading to have a category called mainstream science against which we can judge the findings that are presented in an evidentiary context.

**Follow-up Question by Barry Nace:** Is mainstream science something that deals only with the way to get to a conclusion? Or is it dealing with the conclusion itself? I would throw that out for any of the panel. And if we come to the conclusion that mainstream science applies only to the means to get to the conclusion, then what is the function of the judiciary?

**Professor Jasanoff:** The second half I will leave to the work groups, or the co-panelists may wish to address that. But as to the first half, *Daubert* of course is quite clear that mainstream science is not about the conclusions, but about the way of getting to the conclusions. What I have suggested both in my Forum paper and elsewhere is that it may be misleading to talk about mainstream science even as a way of getting to conclusions.

**Judge Bernstein:** I’m not sure if this is responsive at all, but I think the issue for a judge should be, Is this testimony science or superstition or religion or magic? If the judiciary comes to the conclusion that it is science, then I don’t think we should be terribly concerned whether it is mainstream, sidestream, or little creek running into the stream. If it is magic, superstition, or religion, then perhaps it has no place in the courtroom.

**Professor Berger:** I would like to be a little more specific and concrete and put this in the context in which it arises in toxic tort cases. I see the question as asking, in part, What kind of people have the expertise to tell us whether or not there is a

causal link between this exposure and this disease? There is a crucial issue there, for instance, as to whether a physician on the basis of clinical experience can say, "I can tell you based on my experience that the cause of this must be the exposure, because I know this person's history, I have done a differential diagnosis, there is no other reason that this person would have gotten the disease."

I think, against that, there certainly are people who will tell us that that is not something that a clinician can do. A clinician is someone who can observe, a clinician is someone who can perhaps rule out some causes, but there is nothing that a clinician knows about cause and effect. That kind of determination has to be done with other kinds of science—epidemiology, toxicology, and a variety of different kinds of tests within those regimes. That is a very crucial question that comes up in these cases. I think that when the courts are talking about "mainstream science," some of their concern is about how to determine which discipline has the appropriate expertise. I don't think that is an easy question to answer. But I think that is the question, at least.

**Tony Roisman:** First, since Professor Jasanoff threw down the gauntlet, let me say that that isn't what I meant. All I meant to say was that, in the typical causation case, the plaintiff's burden is inevitably one of challenging a mainstream conclusion. The mainstream conclusion is that the product was safe, the drug was safe, the bridge was safe, the substance was safe, the disposal process was safe. If the test for whether or not the plaintiff's expert gets to testify is whether the mainstream agrees with you, that is the Catch-22.

So it cannot be that the test of admissibility is whether or not the experts' opinions are in the mainstream. There are almost always, in the plaintiff's case in a causation context, going to be people whose opinions don't appear in the standard textbooks and aren't the things that are generally accepted within the community.

That, I think, is not a problem if *Daubert* is read as Professor Berger has just indicated she would read it, and as I would read it. The whole focus is on methodology. When they talk about replication, when they talk about testing, when they talk about peer review, none of those should be considered "opinion." They should all be considered matters of methodology.

So is the scientific methodology that the expert is using a methodology that appears in the peer-reviewed journal? Or, if we went to a journal, would we find that that is what the peer-review people are using when they try to reach judgments about whether A causes B? The difficulty in that area of medical causation is that there is either no clear definition or the definition is so obvious that everybody follows it.

I urge you to take a look at the 11th Circuit's opinion in *Joiner v. General Electric*,<sup>8</sup> in which the court had absolutely no problem finding that the experts were following the standard methodology. The plaintiff's experts testified to what it was, and the

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8 *Joiner v. General Electric Co.*, 78 F.3d 524, 532 (11th Cir. 1996).

defendant's experts confessed that that is what they did to reach the same conclusion. It was that process that I laid out: gather all the relevant information, look at it logically, reach logical conclusions, explain what you did.

That is the kind of methodology that is "mainstream." Knowing that doesn't answer the next question. The next question, the one that is so troublesome, is: In that kind of a case, is the role of the judge to then peer behind the methodology to see whether the judge agrees that the expert used the methodology the way *the judge* believes the methodology should have been used? That, I think, is the point of greatest controversy.

If as I submit, the judge should not go past that because the judge is then into the area of opinion—and that is what the 11th Circuit said in *Joiner*, the Second Circuit has said in similar cases,<sup>9</sup> and the D.C. Circuit said in *Ambrosini II*<sup>10</sup>—then the judge should stop with the knowledge that this is an expert who is qualified in the relevant field and can address the subject matter of the factual dispute. Once the judge has done that, the court is done under *Daubert*. Everything else is left to the jury.

Professor Berger suggests that it is very, very difficult in these contexts to be able to determine what kind of expert is the right expert for this field. I submit that, at least in the field of medical causation, that is not true. Occupational environmental medicine is a field of medicine the subject matter of which is the environmental cause of physical disease. The whole purpose of that field of medicine is to advise industries on (1) whether or not their workers are being subjected to risks; if so, (2) what the risks are; and, if so, (3) what the protections are. Occupational environmental medicine is a "cause" profession. All the other professionals who tend to speak on this are people who engage in diagnosis and treatment. I agree with Professor Berger that those people don't fit well because they don't spend their time trying to understand cause. They are interested in it, but it is not what they do.

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9 In re Joint Eastern & Southern Dist. Asbestos Litigation, 52 F.3d 1124 (2nd Cir. 1995); and McCulloch v. H.B. Fuller Co., 61 F.3d 1038 (2nd Cir. 1995).

10 *Ambrosini v. Labarraque*, 101 F.3d 129 (D.C. Cir. 1996).



## D. SHOULD STATE COURTS IMPOSE “RELIABILITY” THRESHOLDS ON THE ADMISSIBILITY OF EXPERT SCIENTIFIC TESTIMONY RESPECTING CAUSATION IN TORT CASES?

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Professor Gottesman considered the extent to which state courts should follow the lead of the Court's decision in *Daubert*, which instructed trial judges to evaluate the reliability of the methodology of witnesses who offer scientific testimony, in addition to their qualifications. *Daubert*, he pointed out, amounted to a sub silentio reversal of two decisions of the Court that were less than 10 years old: *Barefoot v. Estelle* and *Beech Aircraft Corp. v. Rainey*, both of which construed the identical Federal Rules of Evidence (FRE) construed in *Daubert*.

Professor Gottesman observed that most of the states have rules of evidence that follow the FRE, while others look to the FRE for analogues. He argued that the existence of analogous state rules does not compel state courts to follow the Supreme Court's lead on scientific evidence, and that state courts would be unwise to do so, at least in ordinary civil tort cases, where proof is by a preponderance of evidence. In essence, he argued, the effect of *Daubert* in tort cases is to require the plaintiff to prove causation to a much higher degree of probability than the traditional standard of a preponderance of the evidence.

He found a reliability threshold especially inappropriate in cases where the expert in question is highly credentialed, does not work exclusively as an expert witness, and uses the same methodology in other work outside the courtroom. Under those circumstances, he argued, the expert's own self-interest provides additional assurance of reliability. Even when the witness is a full-time litigation expert, or is testifying outside of the usual area of expertise, the Court's own language in *Daubert* acknowledges that the adversarial nature of the process provides its own checks through opposing counsel's cross-examination. Given that, he argued, it is a gross usurpation of the jury's fact-finding role for judges to foreclose expert scientific testimony because of doubts about the reliability of the witness's methodology.

Even should a state court conclude that a reliability threshold is desirable, Professor Gottesman argued that the four specific reliability factors enunciated in *Daubert*, which are derived from several different scientific disciplines, are not equally appropriate in all cases.

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In *Daubert v. Merrell Dow Pharmaceuticals, Inc.*,<sup>1</sup> the U.S. Supreme Court construed the Federal Rules of Evidence (FRE) as assigning a “gatekeeper” role to federal judges. Henceforth, federal trial judges are to screen expert scientific testimony not merely to assure that the expert is qualified, but also to assure that the methodology employed by the expert is “reliable.” If the judge concludes that the methodology is not reliable, the expert's opinion will not be admitted into evidence. In toxic tort cases, expert testimony is often the only way to establish that a product caused the plaintiff's injury. If a judge bars the testimony, the plaintiff is non-suited.

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1 509 U.S. 579, 113 S.Ct. 2786 (1993).

The holding in *Daubert* was a reversal (although the Court did not acknowledge it) of two of the Court's prior decisions,<sup>2</sup> which had construed the FRE *not* to impose a reliability threshold, but instead to leave to juries the determination whether a qualified expert's opinion is persuasive. Nothing in the language of the FRE had changed since those prior opinions; only the make-up of the Court had changed.

A majority of states have adopted evidence rules patterned after the FRE. In those that have not, courts look to the FRE for analogues in shaping their common law evidence rulings. Thus, the decision in *Daubert* will likely fuel efforts by some litigants to persuade state courts to adopt similar "reliability" thresholds for admission of expert scientific testimony. This paper suggests that state courts would be unwise to do so, at least in ordinary civil cases.<sup>3</sup> State courts should adhere to the traditional view that it would be a usurpation of the jury's function for judges to keep experts off the stand because they (the judges) doubt the reliability of the experts' methodology.

**I. STATE LAW ANALOGUES TO RULE 702 OF THE FEDERAL RULES OF EVIDENCE DO NOT COMPEL A HOLDING THAT THERE IS A RELIABILITY THRESHOLD UPON THE ADMISSIBILITY OF EXPERT SCIENTIFIC OPINION.**

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The Federal Rules of Evidence were enacted by Congress in 1975, and have not changed in material respects since. At the time of their enactment, no federal court had ever imposed a reliability test for expert testimony in civil cases.<sup>4</sup> Some Circuits had adopted the *Frye*<sup>5</sup> test for criminal cases, holding that expert testimony was inadmissible if based upon a methodology that lacked general acceptance in the scientific community. This test was used principally for excluding testimony based on lie detectors and other scientific apparatus. However, the use of the *Frye* test, even in criminal cases, was controversial. Some federal courts refused to adopt any limit on relevant expert testimony,<sup>6</sup> and the *Frye* test was under severe attack from prestigious commentators when the FRE were adopted. "[T]he [*Frye*] general acceptance test has been rejected by an increasing number of courts and attacked by commentators, who have labelled the test 'infamous,' 'a sport,' 'archaic,' and 'antiquated on the day of its pronouncement.'"<sup>7</sup> Charles McCormick, in his famous *Handbook on the Law of*

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2 Barefoot v. Estelle, 463 U.S. 880 (1983); Beech Aircraft Corp. v. Rainey, 488 U.S. 153 (1988).

3 As explained in section V hereof, a case can be made for insisting on a greater degree of certainty before allowing expert testimony in areas, like criminal law and civil commitment, where the burden of proof is higher than preponderance of the evidence.

4 The first federal appellate opinion approving application of a reliability threshold to a civil case was *Barrel of Fun, Inc. v. State Farm Fire & Casualty Co.*, 739 F.2d 1028, 1031 (5th Cir. 1984).

5 *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923).

6 The cases are described in Paul C. Giannelli, *The Admissibility of Novel Scientific Evidence: Frye v. United States, A Half Century Later*, 80 COLUM. L. REV. 1197, 1206–28 (1980).

7 P. Giannelli & E. Imwinkelried, *Scientific Evidence* 13–14 (1986).

*Evidence*, had railed against the *Frye* test, insisting that relevance and proper credentials should be the only predicates for admitting expert testimony.<sup>8</sup>

The FRE were adopted to consolidate what until then had been separate civil and criminal evidence rules. As the Supreme Court explained in two pre-*Daubert* opinions, the intention of Rule 702 was to liberalize the admissibility of expert testimony by eliminating any use of a reliability threshold.

The Supreme Court first discussed the FRE expert testimony rules in 1983, in *Barefoot v. Estelle*.<sup>9</sup> That case involved a constitutional challenge to the receipt of psychiatric testimony on future dangerousness in a state criminal proceeding. Because it was a state proceeding, the FRE technically were not applicable, but the Court drew on them by analogy to explain why there was no constitutional impediment to admitting this psychiatric testimony. The mainstream of the psychiatric profession regarded predictions respecting future dangerousness to be totally unreliable—a point emphasized in an *amicus curiae* brief filed in *Barefoot* by the American Psychiatric Association (APA). Without challenging the accuracy of the APA's assessment that the experts were offering crackpot opinions, the Court responded:

If [the four psychiatrists whose testimony is at issue] are so obviously wrong and should be discredited, there should be no insuperable problem in doing so by calling members of the Association [to testify to that effect]. . . . *We are unconvinced, however, at least as of now, that the adversary process cannot be trusted to sort out the reliable from the unreliable evidence.* . . .

. . . All of these professional doubts about the usefulness of psychiatric predictions can be called to the attention of the jury. Petitioner's entire argument . . . is founded on the premise that the jury will not be able to separate the wheat from the chaff. We do not share in this low evaluation of the adversary process.<sup>10</sup>

The Court emphasized that neither the FRE nor most state evidentiary rules put reliability constraints on the admissibility of expert testimony:

[T]he rules of evidence generally extant at the federal and state levels anticipate that relevant, unprivileged evidence should be admitted and its weight left to the fact finder, who would have the benefit of cross-examination and contrary evidence by the opposing party.

. . . [T]he purpose of the jury is to sort out the true testimony from the false, the important matters from the unimportant matters, and, when called upon to do so, to give greater credence to one party's expert witnesses than another's. Such matters occur routinely in the American judicial system, both civil and criminal.<sup>11</sup>

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8 CHARLES T. MCCORMICK, HANDBOOK OF THE LAW OF EVIDENCE §171, at 363-64 (1954).

9 463 U.S. 880 (1983).

10 *Id.* at 900, 901, n.7 (emphasis added).

11 *Id.* at 898, 902 (citation omitted).

Five years later, in 1988, in *Beech Aircraft Corp. v. Rainey*,<sup>12</sup> the Supreme Court sounded the same theme again, this time in a case directly involving the FRE. The Court noted “the liberal thrust of the Federal Rules” with regard to expert testimony, and reaffirmed that “the ultimate safeguard” in the FRE is “the opponent’s right to present evidence tending to contradict or diminish the weight of [the expert’s] conclusions.”<sup>13</sup>

But then, five years after that, came the turnabout in *Daubert*. Professor Paul Giannelli, a distinguished evidence scholar, has observed that *Daubert* cannot be reconciled with *Barefoot*.<sup>14</sup> As the Supreme Court has issued diametrically opposite interpretations of the same words in the FRE, there is no reason why state courts should feel compelled to construe *their* evidentiary rules, even if worded similarly to the FRE, as the Court did in *Daubert*. And, as will be demonstrated in the remainder of this paper, there is every reason not to do so, for it is bad law and bad policy.

## II. IMPOSING A RELIABILITY THRESHOLD ON EXPERT TESTIMONY IS A USURPATION OF THE JURY’S FUNCTION.

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Why would courts wish to bar a qualified expert from expressing his opinion? There are only two possible explanations: a fear that some experts will give dishonest opinions in order to earn fees, or a belief that experts giving their honest opinions are sometimes so wrong (*i.e.* their science is so bad, despite their credentials) that juries should not be allowed to hear them lest the jurors mistakenly find these wrong opinions persuasive. On either explanation, barring the expert from testifying is a gross usurpation of the jury’s function.

If the motive for screening testimony is a fear that some experts may lie about what they truly believe in order to collect a fee, that is obviously a distortion of the respective roles of judge and jury. It is the jury’s job to decide whether a witness is telling the truth or not.

Not surprisingly, judicial advocates of imposing a reliability threshold do not proffer this reason (although I suspect it is what *really* motivated the turn-around in *Daubert*). Rather, the justification proffered in the *Daubert* opinion is that sometimes experts have used a methodology that *they* sincerely believe is sound, but that the judge can determine is not. But how can a judge presume to know better than an honest, qualified scientist, whether the scientist’s methodology is reliable? After all, an expert knows everything about the subject that the judge knows (including, where applicable, that most scientists hold views that differ), yet still believes the methodology is reliable. Plainly, the judge cannot be better positioned than the expert to make the assessment. Of course, it is also true that the jury cannot “know”

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12 488 U.S. 153 (1988).

13 *Id.* at 169, 168 (1988).

14 Paul C. Giannelli, *Daubert: Interpreting the Federal Rules of Evidence*, 15 CARDOZO L. REV. 1999, 2020–21 (1994).

whether the expert's methodology is reliable. But the choice between conflicting experts' views on a topic that is beyond the capacity of lay people to understand (both judges and juries) is the kind of choice between conflicting versions of the "truth" that juries ordinarily are called upon to make.

If an expert's methodology is outside the mainstream, or unsupported by the scientific literature, those points surely will be put before the jury by the opposing party's evidence. It is then for the jury, not the judge, to decide whether the expert's approach is unreliable. Judges are allowed to usurp the jury function, and direct a verdict on a factual issue, only when the evidence is such that no reasonable juror could believe one side's version on the basis of the record evidence. But, by definition, an honest expert's opinion provides a reasonable basis for a juror's belief in that opinion.

**III. IN TORT CASES, IMPOSING A RELIABILITY THRESHOLD UPON THE RECEIPT OF EXPERT TESTIMONY ON CAUSATION REQUIRES THE PLAINTIFF TO PROVE THE CASE BY MORE THAN A PREPONDERANCE OF THE EVIDENCE.**

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The Supreme Court's opinion in *Daubert* was premised on an incorrect view of the relationship between scientific inquiry and the litigation process. Here is the Supreme Court's view of that relationship:

It is true that open debate is an essential part of both legal and scientific analyses. Yet there are important differences between the quest for truth in the courtroom and the quest for truth in the laboratory. Scientific conclusions are subject to perpetual revision. Law, on the other hand, must resolve disputes finally and quickly. The scientific project is advanced by broad and wide-ranging consideration of a multitude of hypotheses, for those that are incorrect will eventually be shown to be so, and that in itself is an advance. Conjectures that are probably wrong are of little use, however, in the project of reaching a quick, final, and binding legal judgment—often of great consequence—about a particular set of events in the past. We recognize that in practice, a gatekeeping role for the judge, no matter how flexible, inevitably on occasion will prevent the jury from learning of authentic insights and innovations. *That, nevertheless, is the balance that is struck by Rules of Evidence* designed not for the exhaustive search for cosmic understanding but for the particularized resolution of legal disputes.<sup>15</sup>

In this passage, the Court appears to be saying that expert opinions, to be admissible in lawsuits, must have *greater* reliability than those which scientists would find valuable in their quest for a currently unknown "truth." The consequence, as the

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15 *Daubert*, 113 S.Ct. at 2798–99 (emphasis added, footnote omitted).

Court draws it, is that juries will occasionally be prevented from learning of “authentic insights and innovations.” That, the Court explains, “is the balance that is struck by [the Federal] Rules of Evidence.”

That vision, however, has it backwards. The burden state law imposes on plaintiffs in tort cases—to prove that the defendant’s product “more likely than not” caused the plaintiff’s injury—is a lesser burden than laboratory scientists typically impose upon themselves. Scientists want to know whether causation is sufficiently proved that truth can be declared and further scientific investigation suspended—in effect, whether it has been shown to be correct beyond a reasonable doubt. As Harvard Law School Professor Charles Nesson has stated:

An imaginative scientist exploring the hypothesis that a given toxic agent causes cancer is very likely to suspend scientific judgment on the ultimate question of causation until more testing or study can be done to eliminate alternative hypotheses. A doctor or lawyer or judge, on the other hand, often does not have the luxury of postponing a decision. Often he must make a decision once he reaches a reasonable working conclusion. Likewise, we ask juries to come to conclusions without insisting on or waiting for scientific demonstration.<sup>16</sup>

It follows that the litmus tests scientists impose to determine “truth” are likely to be far *more* stringent than those required to satisfy one’s burden of proving that causation is “likelier than not” (and not *less* stringent, as *Daubert* supposed). Scientists have erected high thresholds to identify those findings that are so likely to be correct that other scientists ought to rely upon them in shaping their own research. But it should not require such a demanding protocol to show in a tort suit that causation is likelier than not. Charles Nesson has demonstrated this nicely with a hypothetical of the treating physician’s dilemma:<sup>17</sup>

A patient has symptoms that will be fatal unless immediately treated. The symptoms are caused either by toxin X or by something else. If toxin X is the cause, drug A must be prescribed or the patient will die. If something else is the cause, drug B must be prescribed or the patient will die. The doctor cannot prescribe both drugs, because in combination they are always lethal.

What is the physician to do? Will he reason that, as the possible causative effect of toxin X on this patient has not been proven to the high standards demanded by pure science, the automatic answer is to prescribe drug B? Hopefully not! In this situation, the doctor, recognizing that the truth about causation is not knowable to the degree of certainty ordinarily required by science, will ask which cause is *likelier* than the other. In Nesson’s words:

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16 Charles Nesson, *Agent Orange Meets the Blue Bus: Factfinding at the Frontier of Knowledge*, 66 B.U. L. REV. 521, 529–30 (1986).

17 Nesson, *supra*, at 527–31.

This standard of the treating doctor is the typical juridical standard of proof necessary for the doctor's opinion to serve as a basis for a jury's conclusion about what happened. To be sure, the doctor is not saying he is absolutely or *scientifically* certain of the diagnosis, but to expect him to be so would discourage treatment in most medical situations. A qualified medical diagnostician is familiar with the scientific and medical literature. He assesses the significance of experiments and studies, not in the technical scientific sense of the statistician, but in an intuitive way. He anticipates what the scientist would be able to prove if he could structure the perfect study, the perfect experiment. *Lacking complete information, the diagnostician gives his best judgment. By its nature, this judgment is not, of itself, scientific proof, but it may nonetheless constitute legal proof.*<sup>18</sup>

An elevated threshold for introducing expert testimony would of course apply to both parties to a lawsuit. But the plaintiff has the burden of proof. It follows, as Nesson concludes, that if courts insist upon the “lab scientist’s” standard of proof of causation—proof at a high degree of certainty—they are weighting the rules against recoveries in tort cases. As Nesson explains, imposing that standard as the threshold for finding causation in toxic tort cases “*is tantamount to institutionalizing an objectively determinable probability greater than .5 for cases in which proof of causation involves a disputed supposition about the working of nature.*”<sup>19</sup> This point has been made widely in the literature, by both legal scholars and scientists.<sup>20</sup>

#### IV. THE “FACTORS” SUGGESTED IN DAUBERT FOR MEASURING RELIABILITY ARE ILL-SUITED TO THE TASK.

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As we have seen, *Daubert* imposed a “reliability” threshold based on a misconception of what plaintiffs need to prove to establish causation in a tort case, i.e., that they must prove truth as a laboratory scientist would. That misconception also led the Court to suggest inappropriate “factors” for making that reliability assessment. The Court began by stating that “[m]any factors will bear on the inquiry, and we do not presume to set out a definitive checklist or test . . . . The inquiry . . . is, we emphasize, a flexible one.”<sup>21</sup> But the Court went on to suggest four factors under the

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18 *Id.* at 528–29 (emphasis added).

19 *Id.* at 538–39.

20 See, e.g., Michael D. Green, *Expert Witnesses and Sufficiency of Evidence in Toxic Substances Litigation: The Legacy of Agent Orange and Bendectin Litigation*, 86 NW.U.L.REV. 643, 667–68, 682–94, 697 (1992); V. Brannigan, V. Bier, and C. Berg, *Risk, Statistical Inference and the Law of Evidence: The Use of Epidemiological Data in Toxic Torts Cases*, 12 RISK ANALYSIS 343 (1992); D. H. Kaye, *Is Proof of Statistical Significance Relevant?* 61 WASH.L.REV. 1333 (1986); Steve Gold, *Causation in Toxic Torts: Burdens of Proof, Standards of Persuasion, and Statistical Evidence*, 96 YALE L.J. 376 (1986); Neil B. Cohen, *Confidence and Probability: Burdens of Persuasion in a World of Imperfect Knowledge*, 60 N.Y.U. L.REV. 385, 413–14 (1985).

21 *Daubert*, 113 S.Ct. at 2796, 2797.

guise of offering “some general observations.”<sup>22</sup> Predictably, some lower courts have converted these into a rule, the “four factors of *Daubert*.”

#### A. TESTABILITY AND ERROR RATE.

Two of the factors can be discussed together: “testability” and “control of error.” Here is the Court’s description of testability:

Ordinarily, a key question . . . will be whether [a theory or technique] can be (and has been) tested. “Scientific methodology today is based on generating hypotheses and testing them to see if they can be falsified; indeed, this methodology is what distinguishes science from other fields of human inquiry.” . . . “[T]he statements constituting a scientific explanation must be capable of empirical test.” . . . “[T]he criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.”<sup>23</sup>

And here is what the Court said about error rate control:

Additionally, in the case of a particular scientific technique, the court ordinarily should consider the known or potential rate of error . . . and the existence and maintenance of standards controlling the technique’s operation.<sup>24</sup>

The Court in these two passages selected observations that pertain to particular branches of science, and purported to generalize them (subject only to the qualifier “ordinarily”). The “testability” concept comes from the pure sciences, such as physics and astronomy, where scientists are seeking enduring truths and will settle for nothing less than certainty. The “error rate” idea comes from the other end of the spectrum, where explorations are so mechanical that it is actually possible to measure the correlation between the methodology’s findings and the observable world. (Spectrographic analysis, the example the Court cited, is in this category.)

But many fields of science do not lend themselves to this kind of precision and measurement. How, for example, could psychiatric testimony about a defendant’s mental state at the time of a crime be “tested,” or its “error rate” measured? Plainly, courts are going to have to ignore *Daubert*’s factors in that area, or else *Daubert* will have achieved an unintended elimination of all psychiatric testimony from federal courts. And the same is true for tort cases, once it is recognized that in a tort case the plaintiff doesn’t have to prove causation to a degree of scientific certainty but only that it is likelier than not.

The world of science includes more than lab scientists seeking ultimate truths. It also contains scientists who make probabilistic assessments of the *likelihood* that a

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22 *Id.* at 2796.

23 *Id.* at 2796–97 (citations to books omitted; last two sentences are quoted from cited books).

24 *Id.* at 2797 (citations omitted).

substance is causing harms, when the state of scientific knowledge does not permit a determination with certainty. The federal government has published protocols describing the methodology to be employed by such scientists in advising it with respect to the safety of food additives, drugs, cosmetics, medical devices, industrial chemicals, pesticides, and a wide range of other potentially toxic substances. In those protocols, *in vitro* studies (laboratory studies of biochemical processes), *in vivo* studies (studies of the effect of such suspected toxins on laboratory animals), and epidemiological studies (studies, where available, that compare humans who have been exposed to the substance with those who have not), are all expressly recognized as useful in predicting the effect of various substances on humans.

*In vivo* and *in vitro* data are regularly relied upon by federal regulatory agencies to predict causation even though their predictive reliability is not susceptible to testability and/or error rate control. Indeed, the federal government has learned that it is rare that causation can be established through a human epidemiological study to the degree of certainty scientists require, and that, in consequence, with respect to most substances the best indicators of probability of causation are *in vivo* and *in vitro* data. The EPA Final Guidelines for Carcinogen Risk Assessment<sup>25</sup> explain, “[e]pidemiologic studies are inherently capable of detecting only comparatively large increases in the relative risk . . . . Negative results from such studies cannot prove the absence of [risk] . . . . [Human epidemiologic studies] are useful in generating hypotheses and providing supporting data, but can rarely be used to make a causal inference.”<sup>26</sup> Virtually all federal regulations dealing with toxic risk assessments stress the importance of experimental data based on studies of animals and of chemical structures. For example, the EPA’s Guidelines for Developmental Toxicity Risk Assessment state:

[I]t is assumed that an agent that produces an adverse developmental effect in experimental animal studies will potentially pose a hazard to humans following sufficient exposure . . . . [I]n almost all cases, experimental animal data are predictive of a developmental effect in humans.<sup>27</sup>

But the assessment whether an animal study in a particular case is, likelier than not, predictive of impact on humans is not one that can be reduced to measurement. In consequence, a literal application of the *Daubert* “testability” and “error control” factors

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25 51 FED. REG. 33992 (1986).

26 *Id.* at 33995–96. *Accord*: Office of Science and Technology Policy, *Chemical Carcinogens: A Review of the Science and its Associated Principles*, 50 FED. REG. 10372, 10378 (1985). For general discussions of why epidemiology so rarely provides reliable answers, see Howard Latin, *Good Science, Bad Regulation, and Toxic Risk Assessment*, 5 YALE J. REG. 89, 103 (1988); Brief amicus curiae of American Society of Law, Medicine and Ethics, in *Daubert*, No. 92–102 (October Term, 1992).

27 Environmental Protection Agency, *Final Guidelines for Developmental Toxicity Risk Assessment*, 56 FED. REG. 63798, 63801 (1991). See also, Environmental Protection Agency, *Proposed Guidelines for Assessing Female Reproductive Risk*, 53 FED. REG. 24834, 24836 (1988); Occupational Safety and Health Administration, *Final Standard for Occupational Exposure to Ethylene Oxide*, 49 Fed. Reg. 25734, 25742–43 (1984); Federal Insecticide, Fungicide, and Rodenticide Act, 7 USC §§136–137, and accompanying regulations, e.g., 40 CFR §154.7(a)(2)(i); Federal Food, Drug and Cosmetic Act, 21 USC §348(c)(3)(A). This is but a partial list of the numerous federal regulations dictating regulatory action on the basis of animal studies.

would non-suit plaintiffs in all toxic tort cases except for the “rare” substance whose toxicity is so enormous that it manifests itself in human epidemiological studies.<sup>28</sup>

The mismatch between these *Daubert* factors and the government’s protocols (which are not based on these factors and do not satisfy them) has confounded the lower courts in toxic tort cases. A few courts have literally applied the *Daubert* factors, issuing opinions that would effectively spell the end to toxic tort litigation in this country except for products whose toxicity can be demonstrated to a scientific degree of certainty.<sup>29</sup> But others have chosen to ignore testability and error rate control in toxic tort cases—invoking for their authority the Supreme Court’s statement that the inquiry is to be “flexible”—in order to permit scientists to testify to the results of the methodology which, as we have seen, the government uniformly employs for assessing risk of causation.<sup>30</sup>

#### **B. PUBLICATION AND GENERAL ACCEPTANCE.**

The other two factors expressly mentioned in *Daubert* as bearing on the reliability of an expert’s methodology were publication in a peer-reviewed journal and general acceptance:

The fact of publication (or lack thereof) in a peer-reviewed journal . . . 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. . . .

. . . “[G]eneral acceptance” can yet have a bearing on the inquiry . . . .

Widespread acceptance can be an important factor in ruling particular evidence admissible, and “a known technique that has been able to attract only minimal support within the community,” may properly be viewed with skepticism.<sup>31</sup>

These two factors will not have a significant effect in tort cases, so long as courts adhere to the *Daubert* Court’s insistence that only the methodology used by the

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28 Even thalidomide was not identified as toxic in the early epidemiological studies. The FDA declined to approve its use in the United States (on the ground that it had not been shown to be safe) without epidemiological evidence. The FDA exercised its judgment on the basis of animal studies and on early reports of birth defects and side effects to users that were not sufficiently numerous to register in epidemiological studies. Only later did epidemiological studies begin to register the toxicity of thalidomide, as evidenced by about 8,000 cases of birth defects in 46 countries worldwide (less than 20 of them involving American mothers). See Cindy Pearson, *Doctor Who Stopped Thalidomide Celebrates 80th Birthday: Her Work Led to Strengthened Drug Laws*, NETWORK NEWS, Sept. 1, 1994, 1994 WL 13449243; *Centennial Journal: Thalidomide Tragedy Spurs New Drug Laws, 1962*, WALL ST. J., Sept. 6, 1989, 1989 WL-WSJ 477241; Amy Linn, *Researcher’s Meticulous Manner Halted Thalidomide*, DALLAS MORNING NEWS, May 29, 1988, at 12A, 1988 WL 5303068.

29 See *Daubert* following remand, 43 F.3d 1311 (9th Cir. 1995); *Pestel v. Vermeer Mfg. Co.*, 64 F.3d 382 (8th Cir. 1995).

30 See *Ambrosini v. Labarraque*, 101 F.3d 129, 137–41 (D.C. Cir. 1996); *Joiner v. General Electric Co.*, 78 F.3d 524, 532–33 (11th Cir. 1996), cert. granted on question of appellate standard of review, 117 S.Ct. 1243 (1997).

31 *Daubert*, 113 S.Ct. at 2797 (citation omitted).

expert is to be tested for reliability, not the conclusions he or she draws from that methodology.<sup>32</sup> There surely can be no stronger indicium of peer approval and general acceptance than the federal government's adoption of a protocol as its basic operating procedure. But if courts become tempted to wander beyond that limit, and begin assessing the reliability of experts' conclusions, these factors, too, have potential to reform substantive tort law to plaintiffs' disadvantage. The reason is inherent in the difference between pure science and litigation.

Publication in a peer-reviewed journal is an academic pursuit. It is not the normal province of academia to survey whether commercial product "X" causes injuries.

Thus, many tort cases pose causation issues that would not naturally find their way

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*Publication in a peer-reviewed journal is an academic pursuit. It is not the normal province of academia to survey whether commercial product "X" causes injuries. Thus, many tort cases pose causation issues that would not naturally find their way into scientific or medical journals.*

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into scientific or medical journals. But, under *Daubert*, the absence of such publication is a demerit for the expert witness seeking to testify. Of course, this is not to say that it would be impossible for experts to publish their opinions in peer-reviewed journals. There are a plethora of scientific and medical journals, just as there are of law journals, and the less prestigious among them are ever in need of articles. We law professors know how hard it is to publish in some journals, and how astonishingly easy it is to publish in others. The same is true in other disciplines. And courts are not likely to calibrate the test of admissibility to the quality of the journal containing the publication. So, if it needs to be done, publication can be accomplished. But notice the advantage that defendants in tort cases have. They have staffs of scientists, and can merely assign them, as a cost of doing business, the task of publishing their anticipatory expert opinions exonerating their employer's product. Plaintiffs, on the other hand, are one-time participants in litigation, and a similar litigation-motivated flurry of publication will emerge on the plaintiffs' side only if the plaintiffs' tort bar elects to

finance it. Because "publication in a peer-reviewed journal" does not have the magical quality of venerability that the *Daubert* Court mistakenly assumed, it seems a silly endeavor to induce lawyers to sponsor publication merely to keep their clients' feet in the courthouse door.

"General acceptance" will be the more dangerous factor for plaintiffs in toxic tort cases if courts depart from *Daubert* and begin assessing the reliability of conclusions, rather than methodology. For here, defendants by sheer numbers will have an advantage over plaintiffs when the causation issue involves a product that is not the

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<sup>32</sup> "The focus, of course, must be solely on principles and methodology, not on the conclusions that they generate." *Daubert*, 113 S.Ct. at 2797.

subject of widespread governmental interest. A large percentage of the non-government scientists in this country are employed by industry. Whenever there is less than certainty about a causation question, it is inevitable that a large cadre of industry-employed scientists will believe that causation does not exist. Litigation does not ordinarily spawn an army of comparable size on the plaintiffs' side. Ironically, the reliability inquiry *Daubert* initiated would then become a reliability that is purchased, rather than derived from the methods of science.

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## V. CONCLUSIONS.

*Daubert's* assignment to trial judges of a "reliability" gatekeeping function was ill-advised. State courts would be wise to steer clear, at least in ordinary civil cases such as torts. (Because the burden of proof in criminal cases, and in a few civil contexts such as civil commitment, is higher than preponderance of the evidence, a stronger case for insisting upon reliability can be made in those contexts. Indeed, that is why *Frye* developed as a criminal-case-only threshold prior to *Daubert*.<sup>33</sup>)

It is especially inappropriate to employ a reliability threshold when the expert is highly credentialed, is not a full-time witness, and is using the same methodology that she uses in her "real world" job (e.g., academia or government employment). That scientist's need to protect her reputation, and the fact that she uses the same methodology in her non-litigation-related work, provide all the assurance that should be needed that the methodology is "reliable." When these factors are present, there is no warrant for judges inquiring further. Not surprisingly, courts have been disinclined to challenge the reliability of highly credentialed experts' methodology, and instead have cited high credentials as an additional and very important factor in applying *Daubert*.<sup>34</sup>

When these factors are not present (because the witness is a full-time expert, or is testifying outside his area of real-world expertise), there is more justification for concern that the testimony is being tailored to accommodate a client's interest. Nonetheless, those very grounds for concern can be pointed out by opposing counsel, and, as the pre-*Daubert* Supreme Court said, "[t]he purpose of the jury is to sort out the true testimony from the false . . . and, when called upon to do so, to give greater credence to one party's expert witnesses than another's."<sup>35</sup>

If the courts in a state remain unpersuaded, and do decide that a judge-controlled threshold is appropriate in some or all situations, they would be wise to refrain from universal adoption of the particular factors mentioned in *Daubert*. For, as shown above, those factors are not sensible in assessing when expert testimony about

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33 Paul C. Giannelli, *The Admissibility of Novel Scientific Evidence: Frye v. United States, a Half-Century Later*, 80 COLUM. L. REV. 1197, 1244 (1980).

34 See, e.g., *Ambrosini v. Larroque*, 101 F.3d 129, 140 (D.C. Cir. 1996); *Joiner v. General Electric Co.*, 78 F.3d 524, 532 (11th Cir. 1996); *Hopkins v. Dow Corning Corp.*, 33 F.3d 1116, 1125 (9th Cir. 1994).

35 *Barefoot*, 463 U.S. at 902.

causation in toxic tort cases is reliable. Courts should stop simply with recognition that an expert who consults the data the federal government regards appropriate is entitled to state the conclusions he draws from that data—conclusions that, inevitably, will not be susceptible to arithmetic measurement for reliability.

## **E. ADDITIONAL ORAL REMARKS OF PROFESSOR GOTTESMAN**

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I'd like to continue the conversation that was started this morning, touching a little bit on what I said in the paper, but also more generally describing what has happened to *Daubert* in the lower federal courts.

I want to suggest that this is a cautionary tale that ought to inform courts both in deciding whether they are going to have a reliability filter and, if so, and perhaps more importantly—because I think most of them are going to have one or do have one—in how to shape it so that it doesn't produce some of the consequences that *Daubert* has to date, at least in the lower federal courts.

I also want to describe briefly the *Joiner* case, which is coming up to the Supreme Court this term and which will be its next opportunity after *Daubert* to expound about the role of the courts in monitoring this, and in particular to do so in the context of what the appellate standard of review is of district court rulings excluding or admitting evidence, and what the implications of that may be.

I want to begin this, however, by emphasizing that what I am going to talk about (and, really, what was talked about this morning as well) is the role of judges in screening expert testimony in tort cases, and even more narrow than that, in toxic tort cases. What is said here may not really have applicability to how you deal with criminal cases or perhaps other issues.

### **DISTINCTIONS BETWEEN CRIMINAL AND CIVIL CASES**

Let me just cite some ways in which criminal law poses different considerations for the determination of the role of expert testimony than tort law does. The *Frye* case itself was a criminal case. Until at least 1984, which means the first 60 years of *Frye*, no federal court ever applied a similar screening device with respect to civil cases. It was quite a recent phenomenon when courts began even thinking about demanding more of an expert than that the expert be qualified and that the subject be one that is relevant to the issues in the case.

There are probably four ways in which the issues in criminal cases are different:

1. First of all, in most cases, when an expert's testimony is challenged in a criminal case, it is with respect to some kind of *forensic device*. As Professor Berger observed this morning, when you are talking about DNA or lie detectors—or, for that matter, any other kind of machinery that can produce information—you

are talking about something that does lend itself easily to the kinds of tests that the Supreme Court talked about in *Daubert*: Does it work? Is it reliable? And so on. You are not talking about the world of scientific uncertainty, but rather about whether particular forensic devices are proving reliable in practice.

2. Second, in criminal cases you are dealing with a different burden of proof. The government has to prove its case beyond a reasonable doubt. That doesn't necessarily dictate a different view about expert testimony as such, but if you're dealing with an area of scientific uncertainty, which is really what this is all about, where science doesn't yet have a conclusive answer, the willingness to allow that uncertain scientific opinion into a case where the government has such a high burden of proof may be different than in a case where the question is simply, Is something likelier than not? Is it likelier than not that this substance caused the injury that the person has?

3. A third difference has to do with the imbalance between the parties. In criminal cases, at least when the government is proposing to produce scientific testimony, there is reason to be concerned about whether cross-examination alone and contrary evidence alone will be sufficient to rebut it in the eyes of the jury, because the defendant may not have the resources to mount counter experts or to do an effective job of cross-examination. In the torts cases, where it is the plaintiff proposing to prove causation like this, there is no problem of a defendant lacking the capacity to point out the inadequacies of the expert testimony.

4. Finally, it is usually the case in criminal law that the evidence that the government wants to introduce is not dispositive of the case. That is, if the government is denied the right to put this evidence in, usually it still has a case to put on. Remember, the *Frye* decision itself turned on the admissibility of lie detector evidence. Essentially, what the courts said in that case was, "Juries have been deciding for centuries whether people are telling the truth or not.

We don't think we should allow novel scientific devices to cloud the jury's thinking about that if we don't have some reason to think they are reliable." But in the toxic tort cases, it is usually the case that if the plaintiff can't introduce expert testimony the case is lost, because these are not issues that lay people can decide without the benefit of expert testimony. So the stakes are different.

So in all of those ways, what I am going to talk about may not be the same kind of analysis that should apply to criminal cases, and my focus will then be on tort cases.

#### **ELEVATED BURDEN OF PROOF**

My thesis, which is also the thesis of the paper, is that in practice, *Daubert*, by making judges gatekeepers of the reliability of expert testimony, has, in effect, raised the

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*In practice, Daubert, by making judges gatekeepers of the reliability of expert testimony, has, in effect, raised the standard of proof the plaintiffs must meet in order to get to a jury.*

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standard of proof the plaintiffs must meet in order to get to a jury. It is no longer enough in a toxic tort case to prove that causation is likelier or not. As a practical matter, you have to prove to a virtual certainty that causation exists, because if you don't have that kind of proof, the judge won't let you present your evidence to the jury.

### CARICATURES OF EXPERT TESTIMONY

Let me describe how that has come about. That is not what *Daubert* said, but that is what it has turned out to mean in the lower courts. *Daubert* was dealing at one level with a kind of a caricature. The Court had a vision, which is probably founded in some truth: "There are charlatans out there who happen to have gotten a degree and who are happy to take money to get on the witness stand and say whatever it is that is in the interests of the lawyer who retained them to say it, and we can't trust juries to recognize when that is the case."

That was a big turnaround from what the Court had said in two previous decisions, where they said, "If somebody is up there saying ridiculous things, the other side is going to point out to the jury that they are ridiculous, and point out that every other expert in the world disagrees with them, and we can trust the adversarial system to expose this and rely on the juries to decide."<sup>1</sup>

By the time of *Daubert*, the Supreme Court had lost its confidence in the jury system as a mechanism for determining that. One way to think about this is as follows. The plaintiff puts an expert on the stand:

Q. Did Substance A cause my client's illness?

A. Yes, it did.

Q. How did you determine that, Doctor?

A. Well, here is my way of making those determinations: I get myself a fresh deck of cards, I shuffle them, and I start turning them over. If I get a seven before a three, that means there was causation. But if I get a three before a seven, there wasn't.

That is what Judge Bernstein this morning called "magic," as the mechanism for this. The Court clearly said in *Daubert*, "We don't want that testimony going to a jury." I don't know if juries have decided cases in favor of experts like that, but of course none of us wants cases going to juries like that; we want testimony that is grounded in the methods of science, not in some crackpot approach that does not have that grounding.

If that is all that *Daubert* said and meant, it would not be occasioning the angst that you are hearing from the plaintiff's bar here today. What has led to *Daubert* having a more negative impact in practice is that the Court went on to articulate, almost

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1 Barefoot v. Estelle, 463 U.S. 880 (1983); Beech Aircraft Corp. v. Rainey, 488 U.S. 153 (1988).

offhandedly, some factors that it said judges should take into account in deciding whether the experts' testimony is based on a reliable methodology.

The factors that the Court listed happen to be the factors by which science arrives at closure—which Professor Jasanoff said this morning is when science says, “Okay, you have convinced us that we as a profession should all now accept the idea that this has been established.”

The Court talked about such things as “Is this testable?” That is, can you replicate your results, can you identify the error rate of this? Now, if we are talking about DNA, those things make sense. We know how reliable DNA is—that is, we can test those things. We can't do that with respect to most toxic tort cases. It is very rare that science ever gets to closure on whether a substance causes illnesses.

To take a case where I suppose most people would now agree science has arrived at closure, tobacco causes lung cancer. But it was 25 years after doctors first began saying that they believed that their patients are contracting lung cancer from

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*There were 8,000 thalidomide babies before the first epidemiological study confirmed that thalidomide was in fact causing these limb malformations.*

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cigarettes, it was 25 years before the first human epidemiological study confirmed that. As was said this morning, there were 8,000 thalidomide babies before the first epidemiological study confirmed that thalidomide was in fact causing these limb malformations.

Why were they able to make that finding in tobacco? They were able to make the finding because they began studying it relatively early. And 25 years later, they had a massive amount of data—so massive that it was able to meet the tests of statistical significance. You have to have enormous amounts of data to do that. This was a product that was used by tens of millions of people, so there was an enormous amount of data, and people were consuming this potentially dangerous product in great volume, so that they were, sadly, replicating the illness that flowed from it in great volume. It

was a highly toxic substance. That is, it did harm to a lot of the people who used it.

When all of those things are in combination and you wait long enough, you will get what science regards as the kind of evidence that enables us to say with scientific confidence, “Yes, A causes B.”

But with respect to most things that are thought to be toxic substances, you never do get that closure. You never get it for a combination of reasons that go together. We suddenly discover the possibility that “this substance” causes “this harm.” A latency period of 20 or 25 years is how long it takes to be exposed before the illnesses show up. And the lawsuits begin, as was said this morning, only when the phenomenon first begins to manifest itself.

If we want to arrive at scientific closure, having made the discovery that this is a hypothesis worth testing, we have to begin 20 to 25 years' worth of studies, by

taking people who are only now going to begin to be exposed to the substance, following them, and comparing their experience with that of people who are not exposed to the substance.

But meanwhile, the product is going to be removed from the market. Unlike tobacco, which hung in there, most products—when people begin to suspect that they are having toxic consequences—are either banned by the government or are withdrawn by the manufacturer because they don't want to expose themselves to the potential of liability down the road. So we are never able to conduct the kind of long-term epidemiological study that would be necessary to enable scientists to arrive at that level of certainty.

Now, the question may occur, why don't they work backwards? From the moment they discover that there is a problem, why don't they say, "Okay, now let's go back and look at the last 30 years and compare the people who are exposed to the substance to those who weren't, and develop our data that way"? Well, as the government has explained repeatedly in its studies, you can't do it backwards because human beings have too many variables. First of all, how do we know which people were exposed in which amounts? The amount of exposure is going to vary from person to person.

Which people in our society were exposed to the particular substance and which were not? A lot of the people are now gone; we can't even ask them whether they were exposed. How do we know what other substances they exposed themselves to that are also suspected to be toxic? We can't control for that when we work backwards. We can control for it, however, when we do a study prospectively. We exclude people from the study who are going to be around other substances. We don't allow smokers, for example, to be in the study groups if we are studying something that we suspect might cause lung cancer. How do we know which of those people had genetic predispositions to these symptoms (which would be another possible cause)?

So it has been found in practice (and the government has said this in its federal regulations) that it is virtually impossible with respect to a toxic substance to conduct a human epidemiological study that will give you the kind of scientific confidence that a laboratory scientist would want before saying, "I am satisfied that substance A causes symptom B."

But there is a scientific methodology for making assessments of how likely causation is in areas where we don't yet have certainty. If you think about it, there are all kinds of government agencies that are all the time making predictions about whether substances are harmful or not, the Environmental Protection Agency and the Food

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*With respect to most things that are thought to be toxic substances, you never do get [the kind of] closure [we've had for tobacco and thalidomide].*

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and Drug Administration being two obvious examples. There is a developed science for prediction—i.e., saying, “We think it is likely that this substance is causing harm”—which is, after all, the legal question. The methodology for making that prediction consists of three components:

1. First (the one that is used most expansively because it is the easiest to use) are the **animal studies**. We expose animals that, for whatever reasons, based on their physiology, we think may have some predictive value for humans. We expose them to the suspected substance. We give them massive doses of it, because we don’t want to take 20 years. So we give them what would be for humans a 20-year accumulated dosage, and we give it to them in a short period of time. Then we look at these animals to figure out whether they are in fact experiencing reactions to this. That is not a guarantee that it is going to do the same thing to humans by any means, but it is a body of evidence that experience has shown is a methodology that is predictive, particularly when we combine it with . . .

2. The second category of evidence: **chemical analysis**. The substance is itself a chemical of whatever composition, and the human body responds to chemicals of particular descriptions in certain ways. We have experience from other, similar chemicals as to what they do to the human body. Therefore, we can examine this substance that is having impact on animals with respect to its chemical consequences.

3. Finally, the third category is such **human epidemiological evidence** as exists. It may not be up there at that very high 95 percent level of confidence that creates scientific certainty. Yet, it may be enough to constitute data that, in conjunction with the other two categories of evidence, enable a predictive scientist to say, “I can bring my expertise to bear, and I can answer the question, ‘Is it likelier than not that A causes B?’” Sometimes the answer will be yes and sometimes it will be no.

If that is done, that scientist has used the methodology that is generally accepted for making the kinds of judgments. But for a couple of reasons, the lower courts have misapprehended the way this is supposed to play out, so they are not allowing that kind of testimony in, at least substantial numbers of them are not, even though the scientists are doing exactly what the government does, and even though there is a significant body of scientific evidence that would say, yes, reasonable experts can arrive at the conclusion that this is likelier than not.

## F. COMMENTS BY PANELISTS

LINDA MILLER ATKINSON, DR. JOE CECIL, AND JUSTICE VICTORIA  
LEDERBERG, AND RESPONSE BY PROFESSOR GOTTESMAN

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LINDA MILLER ATKINSON, CHANNING, MICHIGAN

In traditional trial lawyer mode, I am going to raise more questions than answers, I hope. In fact, I hope that all my remarks will be questions that you can take to your groups and answer.

In this community project that all of us call the law, the project really is justice. But some of the premises that have not yet been mentioned come from, for example, the dissent in *Palsgraf*.<sup>1</sup> It is interesting to revisit, because of Justice Andrews's often-quoted remark that "What we do mean by the word 'proximate' is that, because of convenience, of public policy, of a rough sense of justice, the law arbitrarily declines to trace a series of events beyond a certain point. This is not logic. It is practical politics."<sup>2</sup> Practical politics determines how far the court will go to impose the duty, how far it will extend causation.

It is a question of practical politics. That is really what we have been talking about all day so far—practical politics. One of the most expressive Supreme Court cases demonstrating practical politics at work is *Sentilles v. Inter-Caribbean Shipping Corp.*,<sup>3</sup> in which the Court in 1959 indicated that it is the jury's function to weigh the evidence, and not the Court's function at all to weigh the evidence. That is part of the practical politics at issue that raise the questions that we are dealing with.

The questions that scientific evidence in tort cases pose to you, I suggest, are really no different from the questions that evidence has all along raised for the courts and for lawyers before the courts. What has added to the volume of these issues of scientific causation in tort cases is the dollars and the energies expended on trying to convince courts that this kind of evidence is somehow different from the other kinds of evidence on which you rule.

The first question I would ask you is, Are reliability thresholds imposed on other evidence? The paper this afternoon asks if a reliability threshold should be imposed. There already is a reliability filter, so I would amend the question slightly to ask if an additional threshold of reliability should be imposed in front of—or ahead of—the fact-finder. Are reliability thresholds imposed on other evidence—such as photographs, videos, documents, real evidence—in the same way? Scientific evidence is not significantly different from those types of evidence.

On what do we rely to point out the defects or the problems or the bias in other evidence so that the jury can weigh it? We rely on cross-examination, and we rely on the proponent of the evidence to explain its significance further. But, primarily, the filter for

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1 *Palsgraf v. Long Island R.R.*, 162 N.E. 99 (1928).

2 *Palsgraf*, 162 N.E. at 103 (Andrews, J., dissenting).

3 361 U.S. 107 (1959).

other evidence is cross-examination. Is there any reason at all why scientific evidence should be treated any differently, other than a social consideration that has to do with the distribution of the cost of the risk?

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*Has anyone bought a used car recently? Anyone bought a vacuum cleaner recently?... In doing that, were you able to tell by dealing with that person whether they were trustworthy, reliable, going to do the job, carry through as they said they would? When we place a witness on the stand we expect the jurors to be able to tell whether the person is telling the truth—all the truth, or part of the truth—or shaving the truth. If jurors cannot tell that, then the whole system fails.*

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When you look back at the early cases in the development of the products liability doctrine and the development of strict liability, the question was, Who should bear the cost of the injury? That is, I suggest, what Justice Andrews was also suggesting in the *Palsgraf* dissent: that the law cannot wait for certainty. The law must make the decision now, in trying to balance and implement a social policy of who should bear the risk and the cost of that risk, as between the injured individual on the one hand and, on the other hand, the private manufacturer, the distributor, seller, owner, or contractor involved in the event, and society at large (which roughly translates into the taxpayer).

Has anyone bought a used car recently? Anyone bought a vacuum cleaner recently? Any of you hired a building contractor or hired a plumber? In doing that, were you able to tell by dealing with that person whether they were trustworthy, reliable, going to do the job, carry through as they said they would?

When we place a witness on the stand we expect the jurors to be able to tell whether the person is telling the truth—all the truth, or part of the truth—or shaving the truth. If jurors cannot tell that, then the whole system fails. People all over the planet would die to have access to a system that allows the ordinary citizen, whose experience every day in evaluating a person's truth and trustworthiness particularly qualifies them to be the fact-finders, to be the sole weighers of the evidence.

Another question is, Is there any evidence that is per se scientific? The answer to that is no. A study, a document, a test, a demonstration is not either scientific or unscientific. What is scientific is what judgment or decision is made or what conclusion is based on that. That is where the human ingredient determines whether it is scientific or not. You cannot take something and say, "This is scientific and that is not. Therefore, this filter applies here, but not here."

I would like to mention what has happened in Michigan. The Court of Appeals recently decided a case involving "scientific" evidence.<sup>4</sup> In this case, the Court of Appeals initially concluded that the trial court committed error in its exclusion of the testimony of the expert for the plaintiff, where the trial court found that the expert's methodology, which relied on animal studies, was not reliable, and therefore the case was dismissed. Initially, the Court of Appeals reversed.

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<sup>4</sup> Nelson v. American Sterilizer Co., 212 Mich. App. 589, 538 N.W.2d 80 (1995).

The case went to the Michigan Supreme Court. Michigan has not adopted either *Frye* or *Daubert*. The Supreme Court denied leave to appeal and said,

in lieu of granting leave to appeal, we vacate the judgment of the Court of Appeals. . . . and remand this case to the Court of Appeals for further consideration in light of this order. The issue of the admissibility of the testimony of [two experts] must be determined under [Michigan Rule of Evidence] 702. On remand, the court shall review the propriety of the trial court's exclusion of the expert evidence under MRE 702. . . . In making that determination, the Court shall specifically address defendant's argument concerning the appropriateness of the witnesses' reliance on animal studies in preference to existing epidemiological studies. If the court determines that the evidence is admissible, the court shall provide a summary of the recognized scientific principles supporting that testimony. Following a determination of the admissibility of the evidence, the court should consider the propriety of the trial court's order dismissing the complaint.<sup>5</sup>

So what happened? The case went back to the court of appeals, which reversed itself. It cited *Daubert* four times, although we have not really adopted it in Michigan. The court said,

on remand, we conclude that the trial court correctly barred plaintiff's experts from testifying on the issue of causation with regard to plaintiff's liver disease. Where, as here, no epidemiological study has found a statistically significant link . . . and the results of animal studies are inconclusive at best, the expert testimony fails to exhibit the level of reliability required by [Michigan Rule of Evidence] 702.<sup>6</sup>

That is an interesting interaction between courts. Are there significant reasons for that? Can the law insist on scientific certainty? If it does, can it do justice? If you insist on scientific certainty, can you do justice? Is it ethical, is it just, to insist on human testing or other epidemiological studies? Doesn't a ruling such as this, in essence, require human testing? Does a negative epidemiological study prove anything? Should courts weigh evidence? Is it just to allow judges to weigh evidence? What is the purpose of cross-examination? And, finally, does the imposition of yet another judicial filter give unreasonable privilege to established industry and established defendants for whom mainstream science works?

**JOE S. CECIL, PH.D., J.D., FEDERAL JUDICIAL CENTER, WASHINGTON, DC**

I am delighted to have the opportunity to respond to Professor Gottesman's provocative paper.

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5 Nelson v. American Sterilizer Co., 453 Mich. 943, 554 N.W.2d 898 (1996).

6 Nelson v. American Sterilizer Co., 223 Mich. App. 485; 566 N.W.2d 671, 672 (1997).

I am here to urge you to consider adopting a reliability threshold in the state courts as a means of assessing scientific evidence—as a matter of fact, as a means of assessing all kinds of expert testimony.

The experience in the federal courts is instructive in this way: Many people look at the *Daubert* decision as a remarkable turnaround from what was going on in the federal courts up until that point. But, in fact, prior to the *Daubert* opinion there was growing uneasiness in the federal courts with expert testimony. In 1991, two years before *Daubert* was decided, the Federal Judicial Center did a survey of federal district court judges, asking, “What kinds of problems have you encountered with expert testimony?” We had a list of 12 problems, and we asked judges to indicate the frequency of the problems.<sup>7</sup>

The judges indicated that the most frequent problem by far was “experts abandoning objectivity and becoming advocates for the side that hired them.” To me, such a posture by an expert seems fundamentally inconsistent with the role of an expert testifying in court.

Other problems that judges indicated were common were “conflict among experts that defies a reasoned assessment,” and experts offering opinions that are “of questionable validity or reliability.” Those were three of the four most common problems that judges indicated. (The fourth, by the way, was the expense of expert testimony—a topic for another discussion.) When you try to understand the reaction of the federal courts to the *Daubert* opinion, keep in mind the problems judges told us about, because these problems provide the context for much of the interpretation that followed.

To me, the logic of the *Daubert* opinion, at least up to a point, is quite compelling. The logic of the opinion is this: If a scientist wishes to offer expert testimony relating to scientific knowledge, the scientist should have to demonstrate that the

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<sup>7</sup> Surveyed judges were asked to indicate the frequency with which they had observed 12 specific problems in civil cases on their own dockets involving expert testimony (not limited to “difficult or complex scientific or technical evidence”). The judges were asked to record their responses on a scale of 1 to 5, which referred to a range from “very infrequent” (rarely or never observed) to “very frequent” (observed in almost every civil case involving expert testimony). The problems cited in the survey were as follows:

1. Delays in trial schedule caused by unavailability of expert(s).
2. Indigent party unable to retain expert to testify.
3. Failure of party(ies) to provide discoverable information concerning retained experts.
4. Excessive expense of party-hired experts.
5. Attorney(s) unable adequately to cross-examine expert(s).
6. Experts abandon objectivity and become advocates for the side that hired them.
7. Conflict among experts that defies reasoned assessment.
8. Expert testimony appears to be of questionable validity or reliability.
9. Expert testimony not comprehensible to the trier of fact.
10. Expert testimony comprehensible but does not assist the trier of fact.
11. Expert(s) poorly prepared to testify.
12. Disparity in level of competence of opposing experts.

testimony is in fact derived from scientific knowledge, and that it has some basis in the methodologies, practices, and standards that scientists commonly subscribe to.

The problem with the *Daubert* opinion (and the problem that has vexed everyone in this room) is that the Supreme Court then was not able to provide an effective and clear indication of the extent to which this formulation preserves a role for right to trial by jury. Remember that Justice Blackmun said that there is this distinction between methodology and conclusions: that judges should look at methodology and not conclusions.<sup>8</sup> That distinction has no meaning for scientists. It is a distinction that lawyers and judges argue about, but scientists do not conceive of their work in that way. To scientists, the methodology anticipates a conclusion. The design of the research, expressed in terms of the methodology, limits the kind of conclusions the scientist can reach. I invite you to discuss this issue with scientists, and I think you will find them quite puzzled that the law is making fundamental distinctions about individual rights based on what scientists view as a fairly arbitrary distinction between methodology and conclusions.

That being said, please understand that there are a lot of problems in terms of the way the federal courts have interpreted this case. There is a great deal in Professor Gottesman's paper that is quite compelling. There are differences across circuits in the extent to which courts are willing to inquire into the underlying basis of scientific testimony. There is no question that some courts have looked to a uniform conclusion. In my opinion, those courts have gone too far in examining the conclusions that scientists are presenting, and have effectively deprived the parties of a right to trial by jury to deal with disputed scientific facts. I think the federal courts are divided on this issue. Part of the great shakeout over the next 10 years will be some kind of resolution of what it means to preserve this right to trial by jury.

Professor Gottesman mentioned the four factors.<sup>9</sup> One of the most encouraging things about the federal courts' conceptualization of *Daubert* is that the four factors have been elaborated to include factors that are appropriate for specific areas of expert testimony. So, looking beyond scientific testimony, we see, as Professor Berger mentioned this morning, courts developing specific ways of assessing forensic science and engineering science. Courts will continue to develop more refined standards for specific areas of expert testimony to assess whether the person appearing in court, purporting to offer expert testimony, is in fact offering testimony that is grounded in the methodology of that area of work.

So, as you consider the kinds of responses that each state should make to the problems of expert testimony, keep in mind the problems that the federal courts faced. I think you, too, will find that some form of screening for reliability is in fact appropriate.

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8 *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579, 595 (1993) ("The focus, of course, must be solely on principles and methodology, not on the conclusions they generate.").

9 Gottesman paper, text accompanying n.22.

**HONORABLE VICTORIA LEDERBERG, JUSTICE, RHODE ISLAND  
SUPREME COURT**

The answer to Professor Gottesman’s question, “Should state courts impose reliability thresholds on the admissibility of expert scientific testimony respecting causation in tort cases?” should be, “Yes, but. . . .” The court of appeals for the District of Columbia in *Frye* determined that in order to be admissible, scientific testimony must be based on methodology that has gained general acceptance in the relevant scientific community.

The Supreme Court, ruling in *Daubert* that the *Frye* test had been superseded by the federal rules, found that, indeed, the *Frye* test was a very rigid test, that the general acceptance test was fundamentally at odds with the more liberal thrust of the federal rules. The court then attempted to offer guidance on when scientific testimony should be admitted, and described the role of the trial judge as ensuring that any and all testimony of evidence that is admitted is not only relevant but reliable. The court in *Daubert* then identified two criteria for the admission of scientific evidence: relevance and reliability. The relevance factor is certainly familiar to judges and applies to all proffered testimony and evidence. The reliability inquiry, however, is unique to scientific testimony.

I concur with Tony Roisman’s conclusion this morning that scientific and legal processes share a rational cognitive thought process. But science and law do not always use the same language, even though they are using the same words. For example, scientists use the word “reliability” to refer to the consistency of results obtained by scientific methodology. The term “validity” describes whether the methodology in fact measured what it purports to measure. In *Daubert*, however, the court stated that it was using the word “reliability” in the sense of “evidentiary reliability”—that is, trustworthiness, which the court defined as “scientific validity.”<sup>10</sup> That is the same sense in which, I believe, Professor Gottesman is using the word “reliability.”

Thus, the reliability inquiry that is required by *Daubert* is in fact an inquiry into the validity of the method underpinning proposed scientific testimony. As the court phrased it, “the trial judge must determine as a threshold to admissibility whether the proposed testimony in fact constitutes scientific knowledge, as opposed to, for example, ‘junk science.’”

Professor Gottesman argues that state courts should not follow the lead of *Daubert* in interpreting their own codes of evidence, because of three fatal flaws: First, screening by judges usurps the role of the jury; second, a reliability threshold may in fact raise the burden of proof beyond a preponderance of the evidence in tort cases; and third, *Daubert*’s four factors are unworkable.

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<sup>10</sup> *Daubert*, 509 U.S. at 590–91 n.9 (“In a case involving scientific evidence, *evidentiary reliability* will be based upon *scientific validity*.”). (Emphases in original.)

Let me briefly address these concerns. Professor Gottesman makes a strong case that the jury, not the judge, is the proper authority for determining the reliability—that is, the validity—of expert scientific testimony. In favor of this view, he observes that judges are in no better position than juries to assess the reliability of an expert’s methodology. It is true that judges may be ill-equipped to evaluate scientific evidence, but I respectfully disagree with the professor’s conclusion that judges should refrain from exercising a gatekeeping role on the reliability of scientific evidence. Judges, after all, ordinarily play a gatekeeping role in determining the admissibility of evidence, whether it be in medical malpractice cases, engineering, DNA evidence, etc. They rule on the qualification of experts, or whether proffered evidence is misleading or unfairly prejudicial.

The reality of modern litigation often requires judges to immerse themselves in scientific aspects of the case. With regard to the exclusion of scientific evidence on grounds of unreliability—that is, trustworthiness—the strongest argument for such exclusion comes from a consideration of the types of evidence that could be admitted in the absence of a reliability threshold. Few judges would be comfortable in admitting, as scientific, testimony based on astrology or dowsing or mind reading, even though some people may regard such activities as reliable. Yet the exclusion of such evidence, whether accomplished under the guise of unreliability, irrelevance, or prejudice, unavoidably requires an evaluation of the validity of the underlying science. *Daubert* directs that the judge’s role should be to determine how and where to set that threshold, not to avoid it altogether.

We can easily envision a continuum wherein the various burdens lie. Will we ultimately assign numbers along the continuum to represent the burdens of proof? Are we going to “digitize” our burdens of proof, so that when we talk about “a preponderance of the evidence” versus “beyond a reasonable doubt” versus “clear and substantial evidence,” we are in fact assigning a numerical value to the quantity and/or quality of proof? In my opinion, *Daubert* does not necessarily place that burden on the parties offering scientific testimony. I believe that Professor Gottesman raises a very important point with this issue of the burden of proof.

In conclusion, the *Frye* test, with its monolithic emphasis on general acceptance, became too rigid a test because it automatically excluded novel but sound scientific

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*Few judges would be comfortable in admitting, as scientific, testimony based on astrology or dowsing or mind reading, even though some people may regard such activities as reliable. Yet the exclusion of such evidence, whether accomplished under the guise of unreliability, irrelevance, or prejudice, unavoidably requires an evaluation of the validity of the underlying science.*

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techniques that did not gain widespread approval. But the abolition of any gatekeeping role on the part of judges, I believe, would be an invitation to all kinds of questionable science. *Daubert* represents an effort to find an intermediate position. Although its factors may not apply in some cases, and may not be sufficient in others, it represents a useful point of departure for resolving those disputes that concern the validity of science in testimony given by experts. I believe the question of whether to admit specific scientific evidence will continue to be resolved as it has been historically, one case at a time, with a careful emphasis on the facts of the particular dispute and the principles and methods of the challenged science.

#### RESPONSE BY PROFESSOR GOTTESMAN

The comments set up very well a discussion of the three ways in which federal courts have taken *Daubert* and used it in a way that raises the burden of proof. Linda Atkinson described two of those ways, and Joe Cecil and Justice Lederberg focused on the third, which I think ultimately may be the most dangerous, because it is the hardest to cure.

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*The single most common error that judges make is misunderstanding what it means to say, "There is not proof, from the epidemiological studies at the 95 percent statistical significance level, that A causes B."*

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Linda described the Michigan Court of Appeals effectively saying "The plaintiffs are not allowed to introduce testimony because your animal data is inconclusive, that is, animal data alone are not sufficient to show causation. Your thesis is inconsistent with the epidemiological evidence, that is, such evidence as we have about what the impact of the substance is on humans." I respectfully submit there are two errors built into what that court did.

1. In saying the animal data are inconclusive, the court is demanding scientific certainty. It is true, we can never say conclusively that because a substance is doing harm to animals, we know that it will do harm to humans. But experts can say, depending on what the nature of the harm is and what the chemical analysis is, what the likelihood is that these results will be translatable to humans. In some cases it is very high. In some cases the likelihood is much smaller. In some cases it is more likely than not, and in others it is not. But when the court says, "You can't

prove a case with animal data alone," it is using as its test of admissibility scientific closure. The standard for *scientific closure*, of course, is *certainty*.

Now, that is a legal determination. On review it should be treated as a question of law. Is that what *Daubert* means? If not, the court was mistaken about *Daubert*. Is that what a state court's rule is going to mean? If so, the courts will have raised the standard of admissibility, and thus the burden of proof.

2. The statement that the evidence was inconsistent with the epidemiological data is a mistake about science. Now the court is not talking about closure. Instead, it is not understanding the science, which is a very serious problem,

obviously, when judges, who are not trained as scientists, begin to look at these issues. The single most common error that judges make is misunderstanding what it means to say, "There is not proof, from the epidemiological studies at the 95 percent statistical significance level, that A causes B."

Judges tend to draw two mistaken assumptions from that statement:

The first is that the epidemiology is exculpating the substance. Well, it isn't doing that at all. If you reverse the hypothesis and ask, "Do these studies prove that the thing is not causing it?" you would have an overwhelmingly low number. The fact is, the epidemiological evidence can be very informative, even though it doesn't rise to a 95 percent statistical significance level, which leads to the next.

The second mistaken assumption judges draw relates to the proper level of statistical significance in causation. Do you know what it means to get 90 percent statistical significance? It means that the people who were exposed are becoming sick in overwhelmingly higher numbers than the people who were not, even if we aren't at 95 percent statistical significance. All science is saying is, "We see the phenomenon, we see that there is a very high correlation. But we are not yet prepared to stop doing research because we want to see it at a 95 percent level before we are prepared to do that."

3. The third thing that is happening, that Joe Cecil and Justice Lederberg pointed out—and the one I suggest is the most dangerous—is that the distinction between methodology and conclusion that the Supreme Court drew so carefully in *Daubert* is being eroded by the lower courts. The lower courts are saying the following: "It is true that the expert consulted all the right data. But that is not enough. We want to know whether the expert used the scientific method in getting from that data to the conclusion the expert reached. So we are going to apply the *Daubert* factors to that step in the analysis, the step that led to the conclusions as well. Are the conclusions testable, what is the error rate of the conclusions, etc.?"

The Supreme Court, I suggest, very deliberately made the distinction it did. Joe Cecil is right: It is not the distinction that a laboratory scientist would make, which is, "I am trying to find out what the ultimate answer is. Until I get closure, I am not satisfied." But what the Supreme Court was trying to say was a legal point, not a scientific point: "We want to be confident that scientists who get on the witness stand are using the scientific method. We want them to use a method as to which science has arrived at closure or something close to it. We want to have a lot of confidence in the method. But we are not insisting on closure about the conclusions, because if we did, we would truly be saying that a plaintiff can't make a case unless they can prove to scientific certainty that the substance is causing it." The lower courts are ignoring this distinction, and that is what is raising the threshold of proof so high.

The last point I want to make relates to the role that the *appellate courts* are going to play in this. *Joiner* is now before the Supreme Court. Its big question is, What is the standard of review when a district court excludes testimony and, on the basis of that, throws the whole case out—in other words, when it says, “Plaintiff, you can’t have a trial. Your experts didn’t meet the standard.” The 11th Circuit, on the facts of that case, reversed the trial judge for errors that are not being challenged in the Supreme Court. But at the beginning of its decision, where it set out the standard of review, it said there are two separate standards of review that we apply in this kind of situation:<sup>11</sup>

Number one, to the extent that the court says, “I don’t think this factor is relevant, or I think this factor is relevant, etc.,” that is a question about the meaning of *Daubert* and ultimately about the meaning of Section 702 of the Federal Rules of Evidence. That is a question of law. We review questions of law as questions of law. Once we get past those two things, we get to . . .

Number two, which is the notion that the evidentiary rulings are ultimately discretionary rulings that are reviewed for abuse of discretion. What the 11th Circuit’s decision in *Joiner* said was, “When we see a judge making a dispositive evidentiary ruling, an evidentiary ruling that means that the case is not going to go to a jury that otherwise would, or it could be conversely a case will go to a jury that otherwise should not have” (it seems to me that this runs in both directions), “those are really important rulings. While abuse of discretion is the standard, we are going to review them with special care. We are going to give them a hard look to make sure that a determination that is so dramatic (namely, that the whole case is taken from the jury or given to the jury that would otherwise come out the other way) has been made on the basis of a careful assessment of the correct legal standard and the correct facts.”

That latter part of the decision has been challenged. The Supreme Court has granted *cert.*, and presumably is going to issue a decision that talks about the standard of review.

I’ll close by mentioning that, although that latter point was the only question presented in *Joiner*, the defendant General Electric and the dozen or more *amici* who have filed briefs in the case in support of General Electric are trying to smuggle a second question in, which is whether the court will abandon the distinction between methodology and conclusion, which is ultimately what the 11th Circuit relied on. It said, “Judge, you didn’t dispute the methodology, you were only disputing the conclusions of the witnesses.” So that legal issue may or may not also be addressed by the Supreme Court in *Joiner*.

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11 *Joiner*, 78 F.3d 524, 529 (11th Cir. 1996).

### III. Luncheon Remarks by Grant Woods, Attorney General of Arizona

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*While expressing his confidence that our civil justice system works better than it is often given credit for, Attorney General Woods nevertheless stressed its fragility and its amenability to being weakened by special-interest politics with the goal of limiting recourse to the courts by ordinary citizens.*

*General Woods identified “phony conservatives” as the principal opponents of the justice system. These foes of the civil justice system are “phony,” he argued, because their real interest lies not in advancing traditional conservative goals of personal freedom, individual responsibility, limited government intrusion into private life, and local control of local affairs, but rather in the “bottom line” of special-interest groups with which they are allied or whom they serve in an attorney-client relationship.*

*As examples of this “phony” conservatism, General Woods cited contradictions between claimed devotion to local control coupled with distrust of the jury system; between insistence on individual responsibility for crime coupled with a permissive attitude toward corporate misbehavior; and between a devotion to free markets in contrast to vocal opposition to the contingency fee contracts negotiated between numerous state governments and private law firms representing them in litigation against the tobacco industry.*

*Bringing his point to the subject of the Forum, General Woods cited several examples of misuse or abuse of scientific evidence from the nationwide tobacco litigation, most notably the efforts of “Big Tobacco” to suppress the growing body of scientific evidence on the health risks of tobacco use and even the “malicious creation and perpetuation of junk science” to the contrary.*

*Although expressing firm support for the venerable attorney-client privilege, General Woods urged the judges attending the Forum to refuse to allow lawyers and their clients to use the privilege to conceal vital information, and also to resist efforts to enlist them in denying jury scrutiny of proffered expert testimony. In so doing, he argued, judges are fighting for the different “bottom line” they have sworn to support: the cause of justice.*

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We have a very fragile system of justice in this country. I think it works better than the average person is willing to give it credit for. Indeed, it works well in most situations, criminal and civil.

The one thing that I have learned in my years of practice, however, is that it doesn't necessarily have to be the way it is forever. I think our system of justice can be changed to the point where it won't work anymore. I am concerned that what will probably change is the part that means the most to me: that the doors can be shut to average people.

The role of “the average person” in the judicial process is very precarious, and there are very few people out there who take the time and spend the money and make the effort to fight for the average person’s ability to find some sort of justice and some sort of recourse in the courts across the country.

On the other side, there is an army of lobbyists and politicians and lawyers out there who are doing just the opposite, who spend their time trying to close the door, in effect, to the average person. It seems to me that the fight among those who are participants in the system—whether judges or advocates—has to be to make sure that this system of justice in America is always open to the least of us, to the poorest of us, as well as to the most powerful and most wealthy.

Most of the criticism of the current judicial system comes from the political right. It wasn’t mentioned that I am a Republican. I used to say I was proud to be a Republican; now I will say I’m a Republican, and I still am. I wasn’t conscripted into this party. But what disturbs me the most is hearing criticism after criticism of the judicial system from so-called conservatives.

It is basically the phony conservatives that I want to focus on first, before we get to another problem facing all of us—that is, the problem of junk science.

The phony conservatives out there claim that they care a lot about this country and they care a lot about the judicial system. But I think, basically, they are phonies. I don’t think they really care that much about the judicial system. I think they care more about their own bottom line, their own special interests. Whoever’s ox is being gored seems to be what generally drives the equation, much more than truly acting upon principle.

You can identify the phony conservatives in many areas, not just in the judicial area. They are the people out there who claim to be very pro-education, but are very anti-teacher. They are the people who value the idea that government should not be involved in the individual lives of people, or at least it should be involved as minimally as possible, except in the areas that are most personal, and especially when we are talking about the doctor’s office or the bedroom. We are talking about people who supposedly pledge allegiance to the flag and the United States Constitution every day, but don’t understand that the basic precept of civil rights in the United States is not some radical notion, but is based solely, totally on the United States Constitution. But if you believe in the U.S. Constitution, you should believe and fight for civil rights for all Americans.

When phony conservatives talk about the judicial system, there are a few other hypocrisies there. They claim to support individual responsibility. We hear that all the time, in every political campaign. Every crime known to man is now federalized. That is because politicians have to be tough on crime, and if you are a Congressman or you are running for Congress, you’ve got to federalize it—to the point of absurdity now.

But you see, they really are totally for responsibility. They are tired of excuses and therefore preach responsibility. If the person is poor, if the person is of color, if the

person is your common street criminal, we are tired of their excuses and we want to have them pay the price. But if we are talking about special interests, if we are talking about the people who feed their campaign coffers, if we talk about corporate Americans who do the wrong thing, the irresponsible thing, then we have a few problems with making special interests—the so-called white collar criminals—step up to the table of responsibility.

There is a reason why in this country you see jails filled with people of color disproportionate to the number of people of color who are walking the streets of America. It is a problem that so-called conservatives should be concerned about—things like inadequately funded education and disproportionate opportunity—and yet they rarely talk about it.

The so-called conservatives out there also claim to be for local control. They want every decision to be made at the most local level possible. Federalism they believe in, supposedly. But they don't really believe in federalism. They are the first to pass down

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*The so-called conservatives out there also claim to be for local control. They want every decision to be made at the most local level possible, but they don't seem to have much respect for the area of local control that is most local, and that is juries. What is more local decisionmaking than taking people basically at random out of a hat and asking them to make key decisions on the lives of the citizenry of your state?*

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these mandates when they have the opportunity, but in particular, they don't seem to have much respect for the area of local control that is most local, and that is juries.

What is more local decisionmaking than taking people basically at random out of a hat and asking them to make key decisions on the lives of the citizenry of your state? Yet, the so-called conservatives have problems with juries. Actually, they have problems with juries on the civil side, not on the criminal side. You see, on the criminal side, they believe that juries are fine. Criminal cases involve less important issues, like life or liberty, not important matters such as how much money one must pay or should receive. Jurors are smart enough, they are intelligent enough, they are experienced enough to make decisions on life and death issues in criminal cases, even though the science involved—particularly, lately, DNA science—is at least as complicated as any issue involved in a civil case.

On the criminal side, that's fine, they say, because on the criminal side we are not dealing with things that important; we're just talking about peoples' freedom. We are just talking about whether they are going to be incarcerated for the rest of their life. We are just talking about whether they are going to live or die.

That's not like on the civil side, they say, because on the civil side we're talking about money. Yet, we very rarely see the phony conservatives having an outcry for jury reform on the criminal side. It is always on the civil side. That should tell you

something about how much they really care about local control, about respecting the individual rights of jurors to be able to do their work, and to be able to do it responsibly and intelligently, which they can do.

That is another very interesting thing. The phony conservatives are all for the marketplace, for everybody except trial lawyers in the United States. But God forbid the trial lawyers should make any money here—even though, for example, in the tobacco case the tobacco companies have spent \$600 million a year over the course of this litigation, and our trial lawyers have been paid not one penny over the last three or four years. Now Congress and critics are trying to restrict the recovery for the plaintiffs' trial lawyers.

So now it is time, when we have realized some success, for plaintiffs' lawyers to be paid and people like Speaker Newt Gingrich say, "We have to look at those trial lawyers." Why don't they look at the tobacco lawyers and those people who have abused the system over and over and over, running up the bills, churning their accounts? The tobacco companies have an endless supply of money to fight these cases. Yet these people would deny or restrict recovery to the plaintiffs' lawyers.

So now we come to those of us who are lawyers, judges, and officers of the court. I don't know how much of what I have said you agree with or disagree with, but if you agree with a portion of it, then we have to look at ourselves and the topic at hand. We are told that judges are supposed to step into this problem of too many experts, too many unqualified "experts," too many so-called scientists making things up or coming up with things that don't really fit the so-called expertise requirement that you would have them meet.

Where does most of the criticism come from? It comes at the expense of plaintiffs' lawyers, and all the supposedly greedy plaintiffs who are coming up with bogus scientists, bogus experts, to further their claims, so that they can enrich not just themselves, but those greedy trial lawyers first and foremost.

I think we can look here at the role of judges, and try to facilitate justice, and make sure that we don't have junk science. I think there are a few things we ought to look at. Let's use the tobacco companies as an example, since we have been litigating against them for some time.

I brought a whole raft of examples with me. I'm not going to try to use them all, but I'll use a couple of them. What I want you to understand is that the problem is widespread. Yes, there are plaintiffs' lawyers out there who come up with experts who will say just about anything. But there are also defense lawyers who do the same thing. Neither one is good. All these issues require some sort of resolution. I'll give you my ideas later on what that resolution should be by the courts across the country.

First, however, I would like to be able to persuade you once and for all that it is not just the plaintiffs' lawyers who sometimes come up with bogus experts.

Let's use the biggest, most powerful companies in the United States—the tobacco companies, who have hired over the last 40 or 50 years the biggest and most powerful law firms in the United States to defend them. We have alleged and we will prove if we get the chance that not just the scientists were bogus, but the lawyers themselves were the ones responsible, first, second, third—all down the line—for the proliferation of junk science to mislead the American public, and for your purposes, to mislead courts across the United States—not once or twice, but thousands of times over the last seven decades.

In 1995, the *Journal of the American Medical Association* devoted most of an entire issue to an inside look at the tobacco industry's tactics, based on an analysis of internal documents provided by an anonymous source to the Institute for Health Policy Studies of the University of California at San Francisco.<sup>1</sup>

Here is what public health researchers and attorneys who examined the documents had to say in one article:

The documents demonstrate that the tobacco industry in general, and [tobacco manufacturer] Brown and Williamson in particular, were very concerned about the threat of products liability lawsuits, and they illustrate some of the steps taken by lawyers at one company to avoid the discovery of documents that might be useful to a plaintiff in such a lawsuit. These steps included efforts to control the language of scientific discourse on issues related to smoking and health, to bring all potentially damaging internal scientific documents under attorney work product and attorney-client privilege to avoid discovery, to remove “deadwood” documents, and to insulate [Brown and Williamson] from knowledge of potentially damaging scientific information from other [related] companies.<sup>2</sup>

In another article, they made the following observation:

The involvement of tobacco industry lawyers in the selection of scientific projects to be funded is in sharp contrast to the public statements made by the industry. The documents show that scientific merit played little role in the selection of special projects or consultancies. Instead, tobacco industry lawyers played an important role in selecting grantees on the basis of their potential legal or political usefulness to the tobacco industry. Projects or investigators that had the potential to produce data unfavorable to the industry were unlikely to be funded. This pattern of behavior should be considered by federal, state and local decision makers when weighing claims by the tobacco industry and its consultants made as part of legislative or administrative proceedings.<sup>3</sup>

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1 The articles in the special issue may be viewed in their entirety on the JAMA World Wide Web site at <[http://www.ama-assn.org/sci-pubs/journals/archive/jama/vol\\_274/no\\_3/toc.htm](http://www.ama-assn.org/sci-pubs/journals/archive/jama/vol_274/no_3/toc.htm)>.

2 Peter Hanauer et al., *Lawyer Control of Internal Scientific Research to Protect Against Products Liability Lawsuits: The Brown and Williamson Documents*, 274 JAMA 234 (July 19, 1995) (discussing efforts of lawyers to conceal information regarding health effects of smoking).

3 Lisa Bero et al., *Lawyer Control of the Tobacco Industry's External Research Program: The Brown and Williamson Documents*, 274 JAMA 241, 247 (July 19, 1995).

And in an editorial signed by every one of the AMA's officers and trustees, they concluded that

the evidence is unequivocal—the U.S. public has been duped by the tobacco industry. No right-thinking individual can ignore the evidence. We should all be outraged . . . .<sup>4</sup>

It started out this way. In 1953, the tobacco companies and their lawyers saw that they had a problem. They all got together at the Plaza Hotel on Central Park. The meeting was held in private. It was held in secret, and it was covered up for years.

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*Juries generally do the right thing and for the right reasons. They can understand complicated evidence, be it DNA or probably working through this complex tobacco lawsuit.*

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They decided they would come up with a statement that they would give the American public. The statement said something like this: "We believe the products we make are not injurious to health. We also believe that we can cooperate, and we will, with those whose task it is to safeguard the public health. Basically, we pledge to do anything and everything necessary to engage in a research effort that would be responsible for the American public, so that everything about our products will be known."

Then they did just the opposite. They put out that statement, and then the lawyers basically controlled the show from that time forward. They did it so that they could control the scientists, who would then produce favorable information and reports. They did it so they could cloud the public debate. They did it so they could claim attorney-client privilege, so the public health agencies and the plaintiffs could never get at those documents and discover the truth.

If you don't believe me, let's look at some of their internal memoranda that have now surfaced. A memorandum from Brown and Williamson's president, dated October 3, 1967, says "Doubt is our product, since it is the best means of competing with the 'body of fact' that exists in the mind of the general public. It is also the means of establishing a controversy."<sup>5</sup>

In 1972 they formed the Tobacco Institute, a phony scientific organization, that they funded, so that any time a legitimate health person would come up with a study that could be used in the courtroom, they would have a "scientific" study to counter it.

In 1982, Dr. Frank Colby from Reynolds said, "We can maintain controversy every darn day of the week." The tobacco industry lawyers, at the direction of their Committee of Counsel, were the generals, and they had the ability then to control, to suppress, to manufacture whatever data was out there.

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4 James S. Todd, MD, et al., *The Brown and Williamson Documents: Where Do We Go from Here?* 274 JAMA 256, 258 (July 19, 1995).

5 *Smoking and Health Proposal*, Brown and Williamson Tobacco Co., p. 4, ¶2 (Oct. 3, 1967).

It went further than that. When we look at the lawyers' involvement in and control over what would be researched, it was total. So you have the scientists being funded by the tobacco companies. The lawyers who worked for the companies decided what would be researched and what wouldn't be researched. Then when we came to the point in time where research that wasn't favorable to the industry might come up despite their best planning, those people were deep-sixed immediately, and the studies buried, under the guise that they were shielded as attorney work product or by the attorney-client privilege. We believe the evidence is irrefutable on that front.

The point for all of you is that, whether it is from the plaintiffs' side or the defense side, there are egregious violations that can happen. What do we do about it? It has been suggested very strongly in the federal rules and in recent decisions that it is for judges to decide. It is for judges now to step in and decide the value of scientific evidence and to be more of a gatekeeper than they have been before.

What I would suggest to you is this: We are the people who have to avoid the temptation to let the system erode so that it no longer resembles what made it great in the beginning and what keeps it great today.

I think the judges play a role here. Truth is what makes these documents relevant. And "sunshine laws" are what brought many of them to our attention. The judges across the United States have to make sure that no lawyer hides behind the attorney-client or work product privilege. We see Congressmen throwing up their hands and saying, "The tobacco companies should have to turn over all two, three, four million documents that they have in their possession, that they are claiming privilege on."

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*When someone gives you the chance to overstep what has traditionally been your role, don't take that opportunity. Respect the judicial system we have. Respect the jury system that we have.*

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We're not in favor of abolishing the privilege. Let's just use the privilege for what it is supposed to be, not for lawyers to cover up damaging information that helps continue the dissemination of a product that ultimately kills hundreds of thousands of people every year in this country. Judges have to step forward and make sure the privilege isn't misused. Judges have to step forward and make sure that lawyers aren't able to control science to such a point that it is no longer science, which is what happened here, with the biggest and best law firms for the biggest and most powerful companies in the United States.

Anyone who thinks these practices are limited to tobacco companies is naive. The bad news is, these are some of the most important companies and the biggest and best law firms. Judges need to stop it.

But what you don't need to do, I would say, is what you are being asked to do. That is, ultimately do what the phony conservatives want to do across the line. That is, when someone gives you the chance to overstep what has traditionally been your

role, don't take that opportunity. Respect the judicial system we have. Respect the jury system that we have. Yes, you're the gatekeeper, and there has to be a minimal standard. Maybe the minimal standard we have seen with this sort of subterfuge has to be raised, discussed, and set by judges. But ultimately, the decision is the jury's. As bad as the tobacco evidence problem is, we as plaintiffs in the tobacco cases would not want judges to weigh the science. The judges should set and enforce the standards; the jury should weigh the admissible evidence. That is the way the system is supposed to work.

I guess that is my point. I wanted to tell you how bad we think this is. That is why we sued the tobacco companies. That is why 40 States in the Union have sued them, for this sort of behavior. But as bad as it is, we still don't want judges to step in and make those factual decisions. We need judges, as much as they can, every chance they can, to stand up for the jury system in this country, because it still works.

Juries generally do the right thing and for the right reasons. They can understand complicated evidence, be it DNA or probably working through this complex tobacco lawsuit. As long as you as a judge have cracked the whip, have not allowed lawyers—no matter how powerful or how influential in your state—to get by with this sort of behavior, as long as everything is seeing the light of day that is supposed to see the light of day, then these lawyers can make their case and they can persuade that jury, if the jury should be persuaded. The other side can do the same thing.

Going back to the premise, I think it is up to us to maintain this fragile system of justice that we have. I had the occasion to go to a little trial in Los Angeles. Actually, it was arraignments that were going on. The judge called the calendar—he walked in, getting ready to call the calendar. I stood up. I was the only one in the whole place who stood up. It was jammed with people. They just kept talking, chewing gum, reading the paper. Nobody did anything to acknowledge that the judge had entered the courtroom.

Now, how did that happen? Did that just happen one day? All of a sudden, everybody decided, we're just going to have chaos here, we're not going to have the respect and decorum we're supposed to have in the courtroom? No, it happened the first time one observer didn't stand up, and nobody did anything about it. The day that another guy decided to read the paper, and the judge didn't do anything about it. The day they had private conversations going on in the back, and neither the judge nor anybody else did anything about it. Slowly but surely, over time, we got to the situation today where nobody stands up, and the judge is nothing in the courtroom but one of the participants.

Now I think we have a better sense of justice in Arizona, and hopefully in your states, when you as judges and we as lawyers respect the justice system. I don't tell lawyer jokes. They're not funny, by the way. They are so tired, it is unbelievable. But you can take that with a grain of salt, I guess, coming from us. But I don't think it is for me as

Attorney General to demean my profession, because I'm proud of my profession. I'm proud of the great things that our profession is able to do. And it is certainly not my job to demean the judicial system or the judges that participate in it.

After I was elected Attorney General I called the Chief Justice of the Arizona Supreme Court. He was the first person I called, because my predecessor Attorney General regularly demeaned the court any time, in my view, he disagreed with a decision that came down. The Chief Justice suggested that we get together and talk about it. I said, "No, we don't need to get together. Those days are over. That will never happen. It will never happen. No matter what decisions you make, what you say, how many times the Attorney General's office loses in the next eight years, it isn't going to happen, not once." And it hasn't happened, because it's not right.

If as a judge you can't respect the system, you shouldn't be a judge. If as a trial lawyer you don't respect the system, then you ought to get out and do something else. How do we expect the people walking around on the street to respect the system if we don't?

It is one thing just to talk positively about this profession and about this judicial system, but it is quite another for judges to restrain themselves in action, when they have the chance to overstep their bounds. In my view, when you restrain yourself when you have an opportunity to do more than you really should do, that is true respect for the system. It is true respect for what conservatives should be standing up for: that is, a judicial system that does work, a jury system that can work if we allow it to work.

Yes, I am embarrassed by some of the things that have gone on recently in our system of justice. But every profession has people that do the wrong thing, and events that don't turn out as they should. It depends on how we respond. Ultimately, justice is being done in this country. Ultimately, justice is done, I believe, because of this great system that we have.

So I would tell you that I think as judges, you have an important role to play. You need to not be naive, and understand the depth that some people will sink to for the bottom line of their client or their law firm. But having understood that, you need to ultimately understand that it is up to those 12 people to make the decision, because they can do the right thing, and they generally will do the right thing if we give them the chance.

Lastly, I would just like to leave you with this: Very, very few people in the world today have the chance to do what we do. Very few people in the world still have the chance at this stage in their lives to change the world a little bit to make it better for somebody else. In the cynicism that pervades America, I think too often we lose sight about how great it is that here we are. We are still in the game, be you in your 30s or 40s or 50s or 60s or 70s or 80s. You're still in the game, and you still have an opportunity here that most people don't.

How many people that you grew up with, if you can think about your grade school class, how many of those people in their lives today have the ability to change things for the better for other people? I would say, if you're like me, probably none of the people that I grew up with in first, second, third grade. Why not? Because life goes other directions. Some of them aren't around anymore. Some of them made poor choices. The majority of them have their hands full just living this life, just getting through the day, keeping a job, working hard, trying to do a little better than the generation before them, trying to do things right for their kids. They don't have the luxury that we do of being able to be involved in a profession that can truly make a difference in people's lives. We do, so we have to keep that opportunity alive. We have to fight for the things that got us involved in the law in the first place.

The decision we all made to go into the legal profession was an unusual decision. Most of us were in our early 20s when we made it. Now here we are, all this time later. With billable hours and pressures here and pressures there and all the things that we have seen, it is so easy to lose sight of the basics.

But my message to you is this: If you lose sight of it, if you don't stay idealistic, who will? There are powerful forces out there in this country, trying to tear down this judicial system. They may say derogatory or self-serving things, but ultimately, it is not for a noble cause. It is for somebody's bottom line. Our bottom line, be we justices and judges or attorneys general, our bottom line is justice. That is a noble cause. People have fought for it, people have died for it, and in our lives, we have to make sure that we continue to live for it.

## IV. The Judges' Responses

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Participants in six discussion groups were invited to consider nine standardized questions (A-I below) related to the papers and oral remarks. Their discussions led the judges to consideration of two additional matters, which appear at the end of this section as topics J and K.

Responses by judges to the questions are excerpted below, arranged according to topic, edited for clarity, and summarized in the *italicized* sections. Asterisks divide comments of different participants. Paragraphing within comments and footnote content have been provided by the Forum Reporter.

The excerpts are individual remarks, not statements of consensus. No attempt has been made to replicate precisely the proportion of participants holding particular points of view, but all of the viewpoints expressed in the discussion groups are represented in the following discussion excerpts.

[Discussion of the first two questions tended to overlap. Responses to both questions are set out below in this section.]

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**A. DO YOU OFTEN HAVE OCCASION TO RULE ON THE ADMISSIBILITY OF SCIENTIFIC EVIDENCE?**

**B. DOES YOUR STATE HAVE RULES OF EVIDENCE BASED ON THE FEDERAL RULES OF EVIDENCE? HAS YOUR STATE HAD OCCASION TO FOLLOW OR REJECT DAUBERT?**

*The judges stated that they are seeing challenges to the admissibility of scientific evidence more frequently, and in a variety of cases. Some reported that their state supreme courts had elected to follow Daubert; others continued to follow Frye; some had adopted neither rule, and some had not yet reached the question.*

### **DISCUSSION EXCERPTS**

Yes, we do have this question of scientific evidence come up. It is coming before our court more and more often, and not always in these toxic tort cases but in garden-variety cases. It is just getting applied everywhere. It has sort of gone rampant, and it is a very distressing turn of events in our state.



I think the question is coming up at different levels. I think appellate courts and the supreme courts are getting this more and more often because there seems to be a

tremendous misunderstanding about what *Daubert* did. Did it liberalize expert testimony, which is what most people felt, or did it, as some people are suggesting, restrict expert testimony by making the judge a gatekeeper and giving a great deal of latitude? It is coming up in a lot of different issues.



I do not see it presented very well at the appellate level, but at the trial court level I hear it all the time.



As a trial judge, I have seen it in every case involving DNA testimony.<sup>1</sup> Then we got into this psychiatric hearing, the child abuse accommodation syndrome.<sup>2</sup> It came up in that, too.



In our western state the question has come up only once. We decided the case without deciding this issue because it was not necessary. We found that nobody on the court really had much of an appetite to adopt the *Daubert* rule. So I suppose you could say that our standard is still that, if the evidence is not “superstition, religion, or magic,” it will probably be admitted.



This is kind of an interesting experiment in “reverse federalism,” it seems, because typically we think of states adopting unique rules or trying things out and then, if it worked, they spread it around the country and then maybe find it or adopt it on a nationwide basis. Here, the United States Supreme Court has jumped in and adopted a national rule for the federal courts, and the states are all sort of sitting around and watching.



We have not adopted the *Daubert* rule as such. We have shown a nodding acquaintance with it in a couple of our opinions, but we have adhered to some of our earlier opinions. I suspect that in the near term we will continue to do so.



In our western state we have done a lot of cases on DNA. We basically stuck with *Frye*, I think in part because we did not understand the difference between that and *Daubert*. In the DNA cases, they no longer have to conduct a *Frye* hearing at the trial court level because we have ruled that that type of evidence is scientific and acceptable. We have done a great number of cases on DNA. I recently attended a conference in New England on the Human Genome Project, and if you think that DNA as it presently stands is a problem for the court, wait until you get to that stuff!

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1 See Background section, n.27.

2 See Background section, n.24.



We have adopted neither the *Frye* test nor the *Daubert*. You might say we ignored *Daubert*. We thought we would like to see more development of what *Daubert* means.



In our western state, we have decided that we are not the appropriate people to decide on the reliability of methodology. So, right now, we are still a *Frye* state. I can see a drift in the DNA cases, and we have been doing some garden-variety polygraph cases, too, in which you can see concerns like, “Who are we to decide, will we be interjecting ourselves somehow and becoming an advocate for the methodology we, the judges, have chosen?” We are using some of the *Daubert* factors, but still in the context of what is general acceptance, how do you evaluate peer review, how do you evaluate this, that, and the other? So we are sort of a messy *Frye* state—a fuzzy *Frye* state.



We have not yet been asked to choose between *Daubert* and *Frye*. I have a feeling that if I were asked tomorrow whether I would choose between *Daubert* and *Frye*, my answer might be that I did not think I had to.



In our western state we have accepted *Daubert*. We have accepted the Federal Rules of Evidence for many years. After *Daubert*, we went ahead and adopted it, but always in a criminal case—we have not had a civil case yet. That is less dangerous than in a civil case, and I think, all in all, that probably less evidence gets in under *Daubert*.



I do not think you can say from this vantage point whether *Daubert* is going to be more restrictive or less restrictive in any context, criminal or civil. It may end up that really there is not much difference between what gets in under *Daubert* and *Frye* over a long period of time.



Our southern state applies *Frye*, mainly because the test seems to be simpler. We also are a little bit concerned about the ability of the trial judge to become a scientist. However, in the criminal context we have a number of situations in which the appellate courts have either found that *Frye* does not apply or have otherwise allowed cutting-edge scientific evidence—mainly in the domestic violence area, relating to the battered woman syndrome.<sup>3</sup> So I do not believe that *Frye* stops novel and new evidence from coming in, but I do believe that it is a more reliable test.

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<sup>3</sup> See Background section, n.23.



Our southern state goes by *Daubert*, but I think there needs to be a merging between *Frye* and *Daubert*. In criminal cases and paternity cases, acceptance of DNA evidence is almost automatic. I think *Daubert* would allow us to go off on a tangent if we are not careful; with a real good lawyer and a good expert, we might accept something as being scientific without input from the community as a whole, and I think we might make a mistake—with things like the domestic violence syndrome and child sex abuse syndrome. For some of those things I think it is a little bit too soon to say this is science and to be put into the position of saying yes, we accept it, or we do not accept it.



Our southern state does not use *Daubert*. As appellate judges, I think we have to be extremely careful that we do not telegraph something to trial judges that would intrude into the proceedings that the plaintiff cannot develop his or her theory in the case. I still trust juries, and for that reason I view *Daubert* a little bit suspiciously.



In our midwestern state we have adopted the *Daubert* standard. Our judges are basically looking at the witness's methodology. If the methodology is sound, the evidence will be admitted, and the jury is going to sort it out.



Our southern state has two conflicting decisions holding (1) that the *Daubert* analysis applies to all expert testimony and (2) that it applies only to scientific experts.



Our midwestern state has dealt with these questions as they relate to child abuse syndromes and that type of thing. Basically we are still using *Frye*. You are dealing with science that may or may not be novel. We have dealt with DNA. We have also had some recent cases on a test that is used for drunk driving—horizontal gaze nystagmus<sup>4</sup>—and we let that evidence in.



Our southern state does not follow either *Daubert* or *Frye* per se. We still believe that admissibility is a question for the judge, while reliability is for the jury.



In our state, we have admitted evidence of battered woman syndrome—and all of the syndromes—without ever adopting *Daubert*, and giving lip service to *Frye*.



I suspect that you're going to find, as *Daubert* continues over the years, that it is going to be the trend. You probably find state court judges interpreting *Daubert* more flexibly than

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4 See Background section, n.26.

federal court judges, because federal court judges are used to having a lot more gatekeeping power. I suspect that *Daubert* in the state courts may be much less troublesome for both sides than in the federal courts.



A lot of jurisdictions that have not accepted the Federal Rules of Evidence have accepted *Daubert* and are applying it as if it were a matter of common law.



In all fairness to the United States Supreme Court, I do not think, by virtue of their experience and training, that they understood what would happen. That is just because, unlike in most state courts, sitting on the United States Supreme Court does not necessarily mean you have ever tried a case with a jury before.

**C. IN YOUR COURTROOM WORK, DO YOU TAKE INTO CONSIDERATION THE DIFFERENCE BETWEEN THE STANDARD OF PROOF THAT SATISFIES SCIENCE AND THE LEGAL STANDARD OF PROOF?**

*Several judges acknowledged the difference between the two standards, and one mentioned a court that had adopted a “95 percent certainty” standard for questions of causation. One questioned the relevance of any testimony that did not point to a “more probable than not” conclusion, while another voiced concern that the decision on standard of proof could amount to a determination of substantive law.*

**DISCUSSION EXCERPTS**

One of the things that I think is valuable about conferences like this is that they prompt judges to think about what they have been doing and perhaps to reconsider some of their mistakes. We see judges expecting experts to testify about causation—whether or not A “causes” B. The scientist does not care about that at all. The scientist rests. He is perfectly happy once you reach a conclusion that there is a statistically significant association. He is happy with that. But lawyers are deeply frustrated with that. The two disciplines are talking past each other. It is very important, I think, for us to come away from this conference realizing that, as judges, we have to think differently than we have in the past about what we are doing when we talk with a scientist about an animal study, about benzene or a pesticide or an air pollutant. That scientist is going to be coming at this radically differently than trying to prove or disprove, in a courtroom setting, whether A “causes” B.



I had the experience recently of trying a major negligence action. My experience in the case confirms my confidence in our legal system to test the reliability of scientific evidence. We had a scientific question that was the subject of about four days of expert testimony. It became apparent that the biases that existed in the

evidence, the scientific evidence, were flushed out by the defense in the case as well as by the plaintiffs on cross-examination. My thoughts are that the questions surrounding what might be scientific mainstream are well addressed and thoroughly explored through the judicial system's process of testing evidence.

The standard that should act as a threshold is not as important, at least in my estimation, as the skills that litigators bring to the process in testing for the truth.



I do not think there is anything new here. In our western state, in civil matters, with respect to medical testimony, the opinion must be expressed to a reasonable medical probability. It is *probability*, as opposed to *certainty*. That means “more likely than not.” There may be an education problem as between attorneys and physicians (who, in my experience, incidentally, are terrible scientists), so that physicians can understand what reasonable medical probability means as a legal term of art. If a physician were to try to say, “I am unwilling to lower the burden that I place on myself that way to answer

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*For every Copernicus, there is the phrenology advocate and the alchemist. The truth may be relative, but there are certain things that we can definitely exclude as not being true.*

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your question because I am not going to express an opinion unless I can express it to a 95 percent certainty,” the court would be ordering the physician to answer the question or at least indicate whether he or she could answer the question on a “more likely than not” basis.



For every Copernicus, there is the phrenology<sup>5</sup> advocate and the alchemist.<sup>6</sup> The truth may be relative, but there are certain things that we can definitely exclude as not being true, more likely than not.



There's an important distinction between the reality of the practice of medicine and the way medical opinions are treated in the courtroom. It seems to me that if it is good enough for the clinic, it ought to be good enough for the courtroom—at least good enough for admissibility. It may not be good enough to win, but it ought to be at least admissible.



It seems ridiculous to adopt a standard where you have to have 95 percent certainty before you are going to have an expert testify. That is ridiculous. It is impossible in many of the fields of science. It may be impossible to get even 50 percent.

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5 See Background section, n.5.

6 See Background section, n.6.



When a product is alleged to have caused a problem, I have yet to see a pharmaceutical company that has not been able to find at least one expert who says that it did not cause the problem. There is always going to be scientific controversy on those kinds of questions.



If, by a preponderance of the evidence, the doctor did not commit malpractice, then by the same standard, a preponderance of the evidence should allow an opinion to be admissible on a *Daubert/Frye* analysis.



If an expert can't say something is more probable than not, it is not relevant, is it? It doesn't help the jury if the expert can't say it is more probable than not. I wouldn't let it in. It's not relevant.



I think we really are getting into substantive law, and in a way deciding that maybe juries are better able to determine that we are going to hold the company responsible if, for example, the scientific evidence is that one person in 100,000 may have had an adverse reaction to a drug. That, to me, is the hardest part. What, as a matter of public policy, do we want as tort law? If it is only in this one case that there is a good chance that this material caused an injury, is that enough to hold the defendant responsible?

That really gets into substantive law more than into reliability questions. Will it happen again? I have always given the example of aspirin, which is used everywhere, but there are people who have ill effects from taking aspirin. Do you hold the company responsible for that small group that have very bad side effects?



I think it's the ramifications of *Daubert* that are energizing this activity, creating a life of its own. Because of the potential application all around the country, someone is being very careful about how the evidence will be developed and from whom.

You know, when you think about the search for truth, would we let Copernicus testify in a case knowing what we know in the present day? The truth is relative. What may be true in this case may turn out in 20 or 30 years not to be true, but as a judge you have to do what you have to do now, because you have to decide the case. You cannot say "Let's wait."



A scientist testifies, at least in our midwestern state, within a reasonable degree of scientific certainty. Therefore, the scientific discipline must determine what is a reasonable degree of scientific certainty, not something the courts conjure up—unless,

of course, we want to change the standard and change it to a reasonable degree of scientific probability, which may not be what they use within the discipline.

In our state, it is reasonable scientific certainty, medical certainty, engineering certainty, whatever it is. The discipline determines what that certainty is. Why is it suggested that that changes the plaintiff's burden of proof? That is what is necessary for him to prove his case by a preponderance.



I come from a state where a court has said that the trial court should not let evidence in unless it is 95 percent certain, and that is very disturbing to me. That does change the burden of proof.



The burden of proof and admissibility should be two different things. It is either admissible under our rules or it is not admissible. I never understood the distinction between something being admissible in a civil case and what would be admissible in a criminal case.



Doesn't it boil down to the quintessential definition of burden of proof? Due process demands that judges consider the risk of error from the evidence that is coming in. Is the risk of error acceptable under the circumstances presented in a particular case? That is the preponderance of evidence test. Preponderance says 51 percent is probably an acceptable risk if it is a monetary issue. If you are talking about reasonable doubt, it is not.



My dad, when he was taking criminology courses in college, used to take courses in phrenology—how the number of bumps on your head would tell you whether you were supposed to be a criminal or not. So, if there is some scientific validity to phrenology, should we let the testimony of a phrenologist come in in the course of a criminal prosecution, because obviously this person is predisposed to be a criminal by the number of bumps on his or her head? There has to be some screening mechanism, so what is the test?



I tend to agree with the thesis, and I think our court's case law reflects this, that basically scientific thought process and the legal thought process are pretty much the same. It is rational thinking.

**D. HAVE YOU EVER USED A COURT-APPOINTED EXPERT? UNDER WHAT CIRCUMSTANCES?**

*Few judges had had experience with court-appointed experts<sup>7</sup>, and several worried that a court's appointment of an expert would confer an unfair imprimatur of credibility on the witness's testimony in the minds of jurors.*

**DISCUSSION EXCERPTS**

Our state has a rule that allows us to appoint our own experts, but it is not within our tradition, and it is not done. I think our level of discomfort here is because we have taken an oath to defend the jury system and to do all these things that are within our tradition. If we were in Germany or France or other civil law countries, where they have an inquisitor system, that is a whole different story. Because of our common law tradition, I don't think we are there. We rely on the jury system.



I think if you do have a system of court-appointed experts, you have a tendency to usurp the jury's function because you have placed an imprimatur on the court-appointed experts' opinions. If you do that, why have experts from the plaintiffs or the defense?



I am on our state rules committee. We recently considered a proposed rule that would allow the trial judge to employ his own expert to assist him in the gatekeeping function. One of the main problems the rules committee saw with that is that an expert picked by the trial judge could turn out to be just another expert witness, rather than someone evaluating the opinions of the other experts. An expert like that would just come in with an opinion, and the judge would accept it, or the jury would accept it, *because the expert was selected by the judge*. Effectively, you would cut out the parties' experts.



Is there an inherent risk when the judge hires an expert who is then going to look at the evidence offered and say, "This guy's opinion is not good because the methodology is bad"? Is that likely to happen when judges hire scientific experts who have their own opinion, as opposed to saying, "The methodology is acceptable, and I am used to seeing experts who will testify differently on the same underlying methodology"?

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<sup>7</sup> See Background section, n.12.



Our western state does not allow court-appointed experts. In fact, we have legislative history in our evidence code that says, in effect, “We have not adopted Federal Rule 706 because we do not want court-appointed experts in our state.”



We allow court-appointed experts in our northern state, but I have never seen a case in which one was used.



You see court-appointed experts in family court. The judge will appoint a social worker, psychologist, psychiatrist, whatever, who will come in and give an independent opinion about the qualifications of the parent.



Some states have specific statutes on this practice. We don’t. It is something that has grown, but it is still quite confined. (We also have one trial judge in our department who is his own expert on any subject!)



It bothers me that a trial judge will be appointing his experts, and to say that he is not going to listen to the parties’ experts. You should be able to see the handwriting on the wall. You know that they are the court’s experts. In our southern state, I do not want my trial judges being placed in that situation. I prefer that the jury solve that problem. I believe in them, and Lord knows how they operate. After 20 or 30 years of it, I never did understand it, but I appreciate that they somehow or another do the job.

**E. OF THE FIVE MODELS OF JUDGING DESCRIBED BY PROFESSOR JASANOFF (I.E., “THE INQUISITOR,” “THE GATEKEEPER,” “THE REFEREE,” “THE MEDIATOR,” AND “THE JUDGE”), WHICH DO YOU FAVOR? WHAT STRENGTHS AND WEAKNESSES DO YOU SEE IN EACH OF THEM?**

*Nearly all of the discussion centered on the “gatekeeper” and “judge” models. Some judges felt that “gatekeeping” inheres in the practice of judging but that the way the gatekeeping function would be carried out remained unresolved. A few were concerned about the typical judge’s lack of scientific background to enter into the domain of science, while some simply advocated reliance on the jury.*

**DISCUSSION EXCERPTS**

I think the gatekeeper language really poses a problem because it implies to trial judges that their function is *to limit* the evidence that goes to the jury.



You cannot exclude the judge function. A judge to some extent is a gatekeeper. To me, being a gatekeeper is part of being a judge and always has been. It is just a question of how tightly you are going to close the gate.



I do not think there is so much of a problem between what was *Frye* and what is *Daubert*, but I think *Daubert* has brought a name to a monster that has really, as someone suggested this morning, changed the balance of what does and does not go to the jury. That is really troubling to me, to have this gatekeeper be able to say, “The jury is not even going to get to hear this.” It seems to me to really fundamentally change our whole court system.



“Gatekeeper” is getting to be such a loaded word, maybe we ought to come up with something else. But it seems perfectly appropriate that a trial court’s role is to say, “If you are going to roll out one of these ‘experts,’ we need to make sure that they are an expert.” Once they get over that hurdle, then it is “Katy, bar the door!” Whatever they have to say is fine, and it is subject to cross-examination.



I do not think that the gatekeeper role is inconsistent with the judge role. I think judges traditionally, in admitting evidence, have acted as gatekeepers. We have relevant evidence that we keep out because we say it is too prejudicial or more prejudicial than probative. We have evidence that is relevant only marginally, and so we leave it out because it is going to do some other things. We have lots of gatekeeping roles, and I do not think that it is inconsistent to defend that aspect of *Daubert* and say that a judge does have a little bit of a gatekeeping role in scientific evidence.



Well, I do not think it is terribly burdensome to say that, if you want the jury to believe that this is science, the judge has some gatekeeping role on determining whether it is scientific. I think it is a little naive to say, “We are just going to leave it up to the jury.” I think that there is some good argument to be made that, at the margins (and I think it really just operates at the margins), judges are better able to make those kinds of decisions than juries about whether this has scientifically valid underpinnings or whether it is junk science. If it is junk science, and it is just not good enough, I think the judge can keep it out and should.

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*I think the gatekeeper language really poses a problem because it implies to trial judges that their function is to limit the evidence that goes to the jury.*

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I have found that when I resolve the *Daubert* issues, usually let evidence in, that is when it is going to settle. You have to resolve these through a motion *in limine*, ahead of time, and then it is still going to settle, but you have to do that. It lets the defense know they are going to get to a jury.




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*A trial judge is a gatekeeper.*

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A trial judge *is* a gatekeeper. I think all of those other categories are subsumed under those two characterizations.



I think that in order to get to the question of reliability and balancing of the prejudice versus the probative value in criminal cases, that is definitely the gatekeeper's role.



Yes, there are some people who will only testify for plaintiffs. There are some experts who only testify for defendants. That all comes out on cross-examination—how much they are getting paid, etc., etc.—but I do not think looking into that is an initial gatekeeper function. Is what the expert is going to say relevant to that trial or not? I do not agree with the “gatekeeper” model. I like the “judge” model. I do not think the judge should be the gatekeeper.



I do not think judges should be gatekeepers even though the Supreme Court says it, but the definition of “gatekeeper” is where we get hung up.



If you call yourself a gatekeeper, meaning that you are going to *close* the door, then that is erroneous. In most cases, that is an erroneous interpretation of gatekeeping. If your interpretation of the role of gatekeeper is to keep out voodoo science, that is fine, providing you have sufficient background and training to determine what is good. With all due respect, judges are not equipped to do that.



When I think about these models I remember the old saying, “Judge, if you are going to try my case, please do not lose it for me.”



We don't have that much specialized knowledge among judges. If there were valid studies with valid methodologies on both sides, at that point it would seem that under *Daubert* the judge could reach a decision excluding it unless he is going to the opinion rather than methodology.



It seems to me that, in criminal cases, the trial judge has a greater responsibility, particularly when it is the prosecution that is trying to use some novel “scientific” theory to support a connection. It seems to me, in those kinds of cases, you have Constitutional implications in addition to the question of whether the trial judge is assuming too much of a fact-finding role.

I wonder whether it is not legitimate to say that there in fact are and should be two different thresholds, one for civil and one for criminal.



What we are really debating is the hearsay rule. We screen evidence that comes before a jury, and the question is how much screening. This is analogous, I think, to ruling on hearsay. How much hearsay are we going to let in? A lot of people advocate giving all hearsay to the jury and letting them determine what the truth is. If you get a jury trial, they ought to hear everything rather than have anything screened.

But we do not trust them too much, so we do a certain amount of screening. That is what we are doing here because experts have very special status. We label them as experts, and the question is how much screening we should do for the jury.



My experience with juries is, if I have let something in, they think it is deserving of serious consideration. They do look to the judge. They hear you calling balls and strikes, and if you are allowing something in, they are going to say, “There has got to be a reason.” So I worry, and I think there has got to be a threshold of some sort, at least for the jury, to give the process some credence.



The problem with our making these judgments, whether we want to admit it or not, is that we are making them on the basis of our own value systems. I think we all fight it, but I think that is what the end result is.



If you don’t like the witnesses, get yourself a decent lawyer and cross-examine them. Otherwise, what is the jury’s responsibility? If I’m going to decide that the evidence is credible, then I may as well just instruct the jury that the evidence is or is not credible. I’ll just decide that case. That is what it really amounts to. If you’ve got opposing opinions, then that would be a role for the jury. The question is, Is the person competent to testify, and is the information relevant? Causation is a continuum. It is never 100 percent black or 100 percent white. It is always a continuum, and people are going to testify as to their opinions, and the jury is going to listen and make their judgment.



Everybody knows that there are expert witnesses who are guns for hire, who are lying through their teeth about what they really believe, who are making it all up. You sit there and you say to yourself, “I know this person is not telling the truth, I know it. This is incredible testimony. I would like to take this away from the jury, because they might believe it.”

If we could come up with a filter that would get that expert out of our courtroom without, at the same time, leading to very valid cases getting kicked out of court, I would get on that bandwagon in a minute. Let’s always have good experts. The question is how to create a filter that gets the bad stuff out without sending the good stuff with it.



Maybe I am cynical, but I think that the Supreme Court probably knew when they came out with *Daubert I, II, III* and *IV*,<sup>8</sup> that the message they gave to trial court judges at the federal level (especially those who think in reality that they are too busy and those who imagine it) is that *Daubert* is a case management tool to be used before you get into a lot of cases, if you really want to be technical about this, because there are so many areas where there is no scientific means to come to these conclusions.

The thing that disturbs me the most is that I have seen some federal courts, at the trial level recently in my part of the country, who are applying this not only to scientific expert testimony but to expert testimony across the board. Now you are really going to get into a bramble bush. I will bet you that you see more and more of that unless something dramatic happens here. It is not simply a *Frye/Daubert* rule. It is more basic than that.



One thing I think we have learned from *Daubert* is that our function is *not to be understood*, our function is *to be impossible to misunderstand*.



I don’t have any more qualifications to make a scientific decision than the most illiterate juror in the state that I live in, but I am charged with some responsibility to follow some of these laws and to consider some policy issues, and, to some degree, I have some consistency.

Though I am uncomfortable in making these decisions from time to time, it is my job to do that. I can’t pass the buck on to somebody else. I am aware of a number of trial judges whose attitude is, “Let the appellate court decide.” That is particularly true where they are motivated by moving the docket, which is a curse. “Pass it on to the jury,” they say, or, “Let the appellate court decide.” They certainly have the right

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8 See Background section, n.11.

to do that. If that is the way they feel with their conscience, that is okay. But comfort is not my job. My job is to give it my best shot, right or wrong.

**F. HOW EFFECTIVE DO YOU FIND CROSS-EXAMINATION TO BE IN PROTECTING PARTIES' INTERESTS IN MATTERS OF SCIENTIFIC EVIDENCE?**

*The judges were generally supportive of cross-examination as an effective means of exposing weaknesses in testimony. There was general acknowledgment that advocacy skills played a great role in contests over expert testimony, as well as some concern that even well-founded testimony could be undermined by the cross-examiner. At least one judge warned that the judiciary must not substitute faith in cross-examination for the judge's responsibility to rule on evidence questions.*

**DISCUSSION EXCERPTS**

I do think that a good cross-examination, by a well-prepared lawyer, is probably adequate to dispel any misleading effect that would be created by "expert testimony" that is not worthy of the name. I think it is a good and effective stopgap against that kind of evidence.



It is hard to generalize about cross-examination, because it is generally as good or as bad as the cross-examiner is good or bad, and depending on how well prepared that individual is.



I would agree that cross-examination is probably effective in big cases, where each side has the ability to get prepared and to hire experts and to have good cross-examination. But I think there is a danger in criminal cases, where you frequently have a court-appointed attorney who is not provided with any funds for investigation and experts, and may not be a very good cross-examiner, and that person goes up against a couple of ace prosecution witnesses.



I think the effectiveness of cross-examination depends on what type of case we are talking about. The criminal cases really worry me, when the prosecution has access—almost unlimited access—to experts. At least in our state, under our system, the defendants, even in capital cases, do not have that kind of access.

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Cross-examination seems to take a very effective toll on particular biases or backgrounds. It is hard to cross-examine on the expert's opinion. You can only cross-examine on the expert's basis for the opinion.



I think cross-examination certainly is sufficient to be the truth filter and is the most appropriate way to perform that function. I would be inclined, if I were the trial judge, to let the jury make those decisions, if they are the fact-finder, and use cross-examination, or whatever other methods there would be, to test veracity rather than to have the judge make the decision.



In our southern state we considered *Daubert* in connection with a case where a major chemical company was the defendant. It just seemed inconceivable to me that that defendant would not be able to cross-examine the plaintiff's expert sufficiently—so much so that they needed this *Daubert* determination by the court. I still have not gotten over that. In so many of these cases in the scientific field, the defendant has far more resources than the judges, certainly. I find it surprising.



In reviewing records of trials, I have seen a couple of instances where cross-examination has been very effective, to the point where the trial judge then reconsidered allowing the testimony in, entertained a motion to strike the testimony, and then instructed the jury. But once the evidence has been allowed in, is cross-examination really an effective tool in terms of persuading a jury that the evidence is not reliable? In the abstract it is effective, and in practicality, it may not be.



Cross-examination can be misleading, too. An effective cross-examiner can sometimes use it to cause problems with evidence that probably is pretty good. We have all seen that happen. So it really depends on the practitioner and the experts. I don't think you can answer the question of the effectiveness of cross-examination in the abstract.



I wonder if we are not begging the question. If the real issue is what role the court plays in determining the reliability of scientific evidence as a prerequisite to admission, it begs the question to say that cross-examination is in fact a tool and we are going to rely on that alone.



Cross-examination may sway the jury because it goes to weight. Once you get to the point where the expert is a credible expert, and has the proper credentials, and has the proper experience, to say that just because 7 out of 10 scientists think the truth

is over here today, and not over there, is not reason to eliminate the expert. That is exactly where the decision should be: in the finder of fact. The way I see it is that *Daubert* right now is usurping the right of the jury to weigh the expert's credibility—not the reliability, because of the expert's conclusions.



There is no question, I think, that superior advocacy is going to win a great number of cases. As a preference I am a great believer in letting it go to the jury. But one of the concerns I think we face, especially in medical malpractice, and maybe products cases to a lesser degree, is the claims made by experts. Pick up any legal journal and you are going to have page after page of advertisements by experts, saying, essentially, "Call us, we will testify for you." If you call them and you send them a \$5,000 retainer, they are not going to say, "The doctor did the right thing," and they will find a reason.

There is some concern that the day of the professional expert is dawning. We are beginning to see an expert witness in virtually every case, where never before people would even dream of bringing in an expert. You'll get a case where there is an expert you really don't need. Somewhere, somebody is going to draw a line, and perhaps this is the beginning of that line.



I think, in the kinds of cases in which this issue arises, the skill level of the attorneys on both sides is usually so high that cross-examination may very well be a fair arbiter.



Regardless of what standard you employ, I think it is the responsibility of the judge to make the decision to admit something or not admit something. I can't use cross-examination as a panacea. If it is truly junk science, if it is truly voodoo, then I'm not comfortable saying it will be exposed through cross-examination. When you are making the decision, because of the vicissitudes of trials and life in general, you don't know what kind of cross-examination there is going to be. You may know about the quality of counsel on the opposing side, you may not. But irrespective of that, I think I have my job to do and they have their job.



Whenever we identify a kind of witness that we think doesn't meet our own personal smell test, we might ask ourselves, "Why do we feel that way?" What is it about that witness's claim that we think is an invalid conclusion? Then ask how that can be brought out in cross-examination. I ask myself what I would do with that witness, and how I would try to attack that and preserve my case and protect my client from that effect. Cross-examination seems to be the alternative to the filter.

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What is the difference between a judge qualifying somebody as an expert, allowing the evidence to go to the jury for whatever weight it is worth, subject to cross-examination, and what we are talking about here in terms of filtering scientific evidence? To me, it is the same thing. Good attorneys will cross-examine, they will in fact get their message across to the jury. The jury will weigh it, they will determine how much weight they want to give. I just don't see a difference. Cross-examination definitely works.

**G. HOW EFFECTIVE DO YOU FIND LAY FACT-FINDERS (BOTH JUDGES AND, COLLECTIVELY, JURIES) TO BE IN UNDERSTANDING AND MAKING DECISIONS BASED ON SCIENTIFIC TESTIMONY?**

*The judges were nearly universally supportive of the abilities of jurors, and there was some suspicion that the scientific evidence controversy was, in reality, an attack on trial by jury.*

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*Jurors aren't mushrooms: they shouldn't be kept in the dark and fed manure.*

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**DISCUSSION EXCERPTS**

Jurors aren't mushrooms: they shouldn't be kept in the dark and fed manure.



I have had a lot of complex cases in the years that I was a trial judge, and I have yet to see a jury get bamboozled by plaintiffs or defendants who come in with novel scientific theories. I, myself, have never had any trouble letting a jury decide that. In the vast majority of cases, I have thought that the juries' decisions were pretty much what I would have decided, after hearing exactly the same evidence.



I was a trial judge for 10 years. To take something away from the jury is patronizing. It is saying that these women and men who work for Popeye's or whatever cannot really analyze something that is so deep and that we have to sift through it first to say whether juries should hear it. In our jury charge, we always tell a jury that even with expert testimony, if you do not believe it, disregard it entirely. When they go back to deliberate, that is what they do.



I get the feeling that maybe the *Daubert* controversy is an attack on our way of trying jury trials and the jury system rather than a principle of evidentiary law or substantive law. If you do not have confidence in the system to begin with, you are going to be much more careful about what you let in and what you do not let in.



My reading of most of the state court opinions is that state judges still trust the jury system and the cross-examination process and the general adversarial process. If the science is really that bad, the jury is probably going to figure that out.



Isn't it possible for reasonable experts, duly qualified, to have reasonable disagreements? If so, isn't that by definition the answer to the jury question, to provide the fodder for it?



I think in the area that I serve in, in our southern state (up until the last two or three years, when there has been overwhelming publicity about "lawsuit abuse"), it seems that jurors were inclined to award money in situations—whether there was expert testimony or not—where there was legally held to be no liability. And from the political standpoint, judges (who in our state are elected in partisan elections) are inclined to follow whichever political party or whichever side of the docket is in control of the political process.



I think the underlying frustration we are feeling is that we have sat on benches and listened to juries get sold a bill of goods by snake oil salesmen, and we watch the outcome of trials that affect people's lives and pocketbooks based on the preparation or lack of preparation of the attorneys. We have experts who have gone back to school to study acting, and we have folks who are giving you a bill of goods who are scientists who sit in a laboratory and communicate either through the Internet or through papers that very few people other than their peers can understand. You get them in front of a jury, and these 12 persons good and true are sitting there totally missing it. The other side brings in somebody who is slick and knows how to do that, and you watch that witch's brew boil. Then you look at the result and you say, "This wasn't right."



*Daubert* has not simplified anything for federal judges. It has created a cottage industry of trials within trials. At least, from the feedback that I get from a lot of friends that are federal litigators, *Daubert* is being used in the federal system to keep evidence away from juries of matters the trial judges just don't perceive they should hear. The judges don't believe it. What makes a trial judge more qualified to determine what proper scientific methodology is than 12 jurors? We are all lay people in relation to the science. So I don't know what *Daubert* did to help anybody in that regard.



You either believe in this system and you think that it works, or you don't. *Daubert* is kind of an out for those of us who may not really believe in the system. I just can't believe that most juries, if they get that kind of evidence, subject to cross-examination,

are going to fall for that kind of stuff. "Do you understand, Doctor, that you are the only person in the world that has that opinion?" That stuff is murder.



What makes the court any wiser about the expert's conclusion than about the methodology, or vice versa? The court can look at literature, but the literature comes from human beings. It is not God-given. They simply write an article in which someone is expressing an opinion.



Sometimes I don't think we give juries enough credit. When they get behind those doors to deliberate, and they consider all this testimony, they use common sense and they come up with a verdict. I just don't think that we are giving enough credit to juries when we start trying to manipulate the evidence.



I tell jurors at the end of every trial that the reason they are here is because there are issues in the world that are too important to be left to judges to decide, and we need 12 people to decide them. In the end, judges don't have any more technical or scientific education than anybody else does, unless it is coincidental that we have gotten a master's or a doctorate in some hard science or social science. I don't have one, so I don't know any more than the next guy about this. If we are going to have a jury, let the jury do its job. I have heard judges say, "Jurors get confused when they hear this stuff. It is really confusing to jurors." Any time I have heard a judge, either a trial or appellate judge, say that jurors are confused by this, it is because the jurors have reached a conclusion different from that of the speaker who says that they are confused.



We have a charge in our state that I absolutely find despicable, but it certainly resolves all the questions about whether the jury thinks admissible evidence should be given some sort of credibility. It is a charge that basically says, "You can disregard all expert testimony." And juries frequently do.



Sometimes I am sitting here, listening to evidence, and I am saying to myself, "This jury is going to take 10 minutes to come back with X verdict." And they come back three days later, and I have obviously misread that jury. They have taken something seriously. I make it so clear to myself, and obviously it is not. Sometimes they come back with the same verdict I would reach, and it is just that they have taken longer to do it. The only thing I can think is that in that jury dynamic, in that room, they are giving things very serious consideration, *because I told them to*. This is what I have sent them back there to do.



One thing that concerns me as a judge is that sometimes you will almost feel justice can be purchased, because if you have a good enough lawyer and you have a good enough expert, you are going to win. I don't know what the answer to that is, but we keep making the litigation process more and more sophisticated, and in doing so, do we necessarily make it fairer? I'm not sure. Who has the greatest purchasing power? It depends. If the trial lawyer is suing the poor employer, it may be the plaintiff. In a lot of situations, it is going to be the defendant.



I have a lot more faith in juries than the critics. I think that 12 people with their experiences are a lot better than what I can bring to bear on the flaws in the evidence. So, if I think there is a reasonable argument, or that reasonable people would differ on the issue, even though I would not believe the evidence myself, I am going to let the jury take that role.



Everything we are talking about—the panel and we in here—what we are talking about is the risk that someone's testimony might be wrong about this issue or another in a tort case. That is where the jury comes in. The jury, at one time thought not to be the forum for expert testimony, turned out to be maybe the best place to evaluate expert testimony.

#### **H. WHAT CRITERIA HAVE YOU USED IN RULING ON ADMISSIBILITY OF SCIENTIFIC TESTIMONY?**

- 1. THE EXPERT'S CREDENTIALS?**
- 2. PEER REVIEW OF THE EXPERT'S CONCLUSIONS?**
- 3. PUBLICATION OF RESULTS IN PROFESSIONAL JOURNALS?**
- 4. EMPLOYMENT OF GENERALLY ACCEPTED SCIENTIFIC METHOD?**
- 5. REACHING "MAINSTREAM" CONCLUSIONS?**
- 6. TESTABILITY OF THE THEORY OR TECHNIQUE?**
- 7. KNOWN ERROR RATE?**
- 8. THE EXPERT'S FREQUENCY OF INVOLVEMENT IN PROVIDING TESTIMONY?**
- 9. TESTIMONY OUTSIDE OF THE EXPERT'S USUAL AREA OF EXPERTISE?**
- 10. THE FACT THAT THE EXPERT'S RESEARCH WAS UNDERTAKEN IN RESPONSE TO LITIGATION RATHER THAN PRIOR TO LITIGATION?**
- 11. OTHERS?**

[Criteria omitted below were not discussed to a significant extent.]

*The judges acknowledged that experts' credentials, by themselves, should not confer respectability; voiced mistrust of the reliability of peer review and publication as guarantors of credible scientific testimony on behalf of any party; questioned the practicality of separating scientific methodology from conclusions as they relate to the evaluation of*

*testimony; voiced concern over experts who testify repeatedly on one side of a controversy; and expressed concern over an overly mechanical, checklist-based analysis of proffered testimony. At least one judge feared for the public policy implications of delegating to lay jurors decisions with potentially far-reaching economic effects, while at least two others advocated admitting any testimony that would assist the trier of fact.*

## DISCUSSION EXCERPTS

### 1. THE EXPERT'S CREDENTIALS.

It seems that there is a consensus that if *Daubert* has done anything, it has given us a wake-up call that it would be appropriate to test the credentials of an expert witness before he or she is allowed to give his or her conclusions, whatever they are. I think everybody agrees that it is a legitimate part of that process to allow the adversaries to inquire into the methodology that these people use. In fact, if their methodology is total quackery, then we can expose them for what they are and say, "You're out."

Part of inquiring into that methodology requires us, using the term of a law teacher of mine recently, to be "knowledgeable consumers" of statistical and scientific information. So while we don't want to hold ourselves out as expert statisticians, we need to have at least what we call in our southern state some "walking around" knowledge of what we are talking about.

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*I'm not sure we should be gatekeepers on the basis of the products of "publish or perish," which is the source of a lot of the publications you're talking about. How do you get on the tenure track? Get something published.*

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### 3. PUBLICATION OF RESULTS IN PROFESSIONAL JOURNALS.

I'm not sure we should be gatekeepers on the basis of the products of "publish or perish," which is the source of a lot of the publications you're talking about. How do you get on the tenure track? Get something published.



I have a question about how peer review is really implemented. In reality, if you know the editor of a journal and you are submitting something, and you are going to get it published and he is going to send it to the peer reviewers who are supportive of your position, it is hard to say that it is entirely independent. Somebody who doesn't know anybody and just submits a paper cold gets a more critical peer review. It is like anything else—it is manipulated.

Another question is, Is something good science if it has been submitted to peer-reviewed journals and been rejected by seven of them? Is that a "peer-reviewed" article? Is that a subject for cross-examination or do we just say it's been peer reviewed?



I think, particularly under the *Frye* regime, the pivotal question is whether it is generally accepted in the relevant scientific community. If you are talking about an article that hasn't been peer reviewed, then I think that probably won't make it beyond the threshold and will never be subjected to cross-examination.



To the extent we are looking at questions about which there are significant and legitimate expert differences of opinion, anything that tests the legitimacy of that opinion is something that I, as a trier of fact, am interested in knowing more about if I am being asked to make a specific decision. So I think that is another factor that is legitimately considered: whether or not it was peer reviewed and the extent of the peer review that exists.



Is peer review a factor for admissibility or a factor for sufficiency for the jury? How much do we weigh it, and where do we draw our line, to say, "This is excluded because it is not peer reviewed"? These are all questions about where we put the line and how we make that evaluation.



Are there instances in which peer review would be the only factor on which I would base a decision on the question of admissibility? If it is just a question of the number of times an article has been published, for example, and therefore is subjected to peer review, I don't think that is a fair measure of whether or not something should be admissible.

Speaking of peer review, we always have to ask who funds the journal. That is always a primary consideration—which think tank controls the publication. I would think they are going to accept for publication those articles that they agree with, that line up with their philosophy, of course.



When we are dealing with people of quality, then the journals are actually competing to get certain authors in their journal. An immunology journal, for example, wants material from major immunologists. They are actually inviting them to submit their latest research, and their own qualifications lessen the scrutiny because the journal wants the article. They don't subject it to the same level of peer review. In some cases the peer reviewers could all

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have lambasted the article, but that doesn't mean that it won't get published. It only means that there are reviewers that have looked at it.



I have seen everything. There is a peer-reviewed article written about every single crazy thing you could ever think of, saying it is good or bad.



There are all kinds of areas that are considered fringe areas but that you might call the mainstream. If you go to court over one of those issues, you can get credible people giving credible testimony on each side of that issue. So it is a tough question, when you get right down to it. It is not an easy question from a gatekeeper's standpoint either. That is why, again, I come back to the bottom line: Let it in, let the jury sort it out.



Sometimes doctors see one patient, and they will send in a case report to a journal. Is that literature—one case report—so that they could have said to you, "There is literature"?



One of the problems with imposing a literature requirement is that there are a lot of reasons why there might not be literature. This gets back to *Daubert* and social sciences. For years, there was no study of rape because it was well known that 75 percent of women who were raped didn't report it. So what were you going to study? You couldn't conduct a valid test or a valid study or a valid experiment for that very reason—because your data wasn't going to be there, or wasn't going to be representative. Literature can be as phony or as doctored as anything else. Who is conducting it? Who is supporting it? Who is paying for it? Who has the greatest interest in creating that document?



Who do we rely upon for the assertion that the scientific community has accepted something? If it is the scientific literature, who writes the literature?

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*Literature can be manufactured. Either party can create the peer review. It is all part of the litigation process.*

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Our court decided a case that is particularly egregious, involving a drug company that produced a chemical allegedly reducing inflammation of the eye. It is inserted directly into the eyeball. It is very difficult to get it out once it gets in. The question in the case was whether the literature supported the proposition that the entire ophthalmological community knew about the danger of the drug. It came out that the drug company was sending free samples to these doctors for off-label use, along with money, along with the offer to utilize their research staff: "Go ahead and experiment. Tell us how it came out. We will write it up." They did, and it was written up. It became part of the literature.

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9 See Background section, n.21.

Back at the farm, at the factory where they make the stuff, the doctors in charge of the research said, "This is a lousy experiment. They did not do a blind test. They did not do this; they did not do that. Tell these people that they have to do better." Meanwhile, it has become part of the literature. Is this the search for truth?



Literature can be manufactured. Either party can create the peer review. It is all part of the litigation process.<sup>9</sup>



I think it is troubling that there are interest groups out there who are in effect creating science, on behalf of whatever interest they represent. That is totally foreign to peer review as we understand it.



#### 4. EMPLOYMENT OF GENERALLY ACCEPTED SCIENTIFIC METHOD.

I think the differentiation between method and conclusion that the court drew in *Daubert* is a valid one. It may not be valid from a scientific standpoint, but I think it is valid from a legal standpoint. I don't think it is unprecedented that the language we use in the law is a little different from the language we use in science. It has a different purpose. For example, in the psychiatric testimony, we talk about mental disease or defect, which is not the psychiatrist's language, and we talk about elements of responsibility, and the whole mental disease or defect defense doesn't track a psychiatric vocabulary. But we force the psychiatrists to come into court, to educate themselves, and to translate their psychiatric findings into legal terms. I think it is valid to do the same thing in scientific evidence, to draw a distinction between the method that is used and the conclusion that is reached. I think if the courts are faithful to that, then there will be fewer problems with *Daubert*. Maybe the problems are because the courts are collapsing method and conclusion and looking at the ultimate conclusion.

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*[Some court opinions]  
have now started to say  
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So judges are not just  
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but with whether they  
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ultimate opinion in the case.*

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Why shouldn't the fact-finder have the methodology question as well as the ultimate expert opinion? Why shouldn't the fact finder be able to decide if the methodology is questionable? Do we think the questions are valid? Does it raise the question of reliability in our minds? Should we follow the conclusion based on this method that has been challenged, or should we not? Why does the judge have any particular expertise that transcends that of a fact-finder?



## 5. REACHING “MAINSTREAM” CONCLUSIONS.

One opinion I saw, and others as well, have now started to say you cannot really look at methodology without looking at the conclusion. So judges are not just dealing with methodology but with whether they agree with the result or the ultimate opinion in the case, which is problematic for obvious reasons.



Apparently, the way it is considered in at least one *Frye* state, it sounds to me like it has to be mainstream science—that is, a consensus of a scientific community. If you take that seriously, a lot of toxic tort litigation would never get through the door under *Frye*.



The same phenomenon we are talking about is in the article by Louis Frank<sup>10</sup> that we received in the Forum materials, where he published in 1986 his theory about “cosmic snowballs.” He had gathered some evidence, but this respected scientist is suddenly derided as a quack because he questions much of the mainstream science. They question his methodology, and now, many years later, everyone says, “I’m sorry, you were right.” He talks about how he was ostracized from the profession during that period.



Who was the astronomer who saw all of the canals on Mars? Then all of a sudden they found out this poor man had a disease of the eye. It ruined his reputation. He was looking at his own eye veins. Poor guy.



If there are valid studies with valid methodologies on both sides, it would seem that, under *Daubert*, the judge cannot reach a decision excluding it unless it is based on the opinion rather than the underlying methodology.

## 8. THE EXPERT’S FREQUENCY OF INVOLVEMENT IN PROVIDING TESTIMONY.

In capital cases, where you are looking for defense experts who will take on a major crime lab, the attorneys general have kept paper on every expert who has ever testified against the lab. You are going to have only one or two experts left in the country who will testify against the lab. That is all they do. They don’t perform independent DNA testing; they just testify.



If we interpret the *Daubert* criteria to be more relaxed criteria, or criteria that vest more discretion in the trial judge to determine what is or is not valid science, some

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10 See Background section, n.20.

trial judges will begin to use the *Daubert* standard to exclude witnesses that they know have been prone on many occasions to get up and say whatever they have been paid to say. That was much more difficult under *Frye*. All a witness had to do was articulate a generally acceptable methodology, and draw a conclusion. Now we'll be talking about how many times the witness has testified for plaintiffs or defendants, and so on. Trial judges are armed with a lot more under *Daubert*. The judge can use those criteria because they are not quite as fixed as the *Frye* rule was.

## 11. OTHERS.

I think a great deal is going to depend on who the lawyer is, how the lawyer goes about the assignment, whether the witness has been prepared to be able to testify to the *Daubert* propositions or the *Frye* propositions. I think, as in every other case that we see, justice may or may not be done depending on what comes out and what does not come out. I think probably we are going to be leaning toward the little checklist of *Daubert* because it is the easiest thing to do. At the trial, we do not have to reinvent this. Here is the first issue, the second, the third, the fourth, and the fifth. You will just go down the line; you do not have to think too much about it—just go down the line and see what you come up with.



I think that, in *Daubert*, the Supreme Court really meant to liberalize the *Frye* rule, and that the four items in the *Daubert* checklist<sup>11</sup> are just meant to be benchmarks. But I think, trial judges being what they are, if they have a checklist they are going to go down that checklist. They may be afraid that if you do not meet that checklist, they may be subject to reversal.



I don't think the Supreme Court ever intended that the methodology be tested by each of the four examples, or that any of the four examples be used to exclude evidence. But, unfortunately, that has all been lost in the translation by some of the trial courts and some of the intermediate appellate courts, and that is what is giving rise to the problem now. But I don't think that is what the Supreme Court intended. I see that happen in our decisions all the time.



When information is peculiar to an industry and you don't have access to it, that does bother me. That is one-sided. If the research has been funded by the industry or the universities and so forth, and it is peculiar to them, then what difference is it going to make whether you have one standard or the other?

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11 See Background section, n.15.



In the trial arena, the attorneys arguing *Daubert* and *Frye* to the trial judges are saying, “Everything that this expert is saying is bull. It is just not supported by the scientific community with any science brought to bear—this expert is just guessing. Even though they have a PhD behind their name or an MD, they are just guessing, and it is just bull.” They are asking the trial judges to say, “Yes, that is bull, so I am going to keep it out.” Somehow, that is not filtering up yet to the appellate level very well.



I still think the trial judge is testing credibility.



A lot of judges forget the fact that we have not repealed Rule 702. Any jurisdiction that says that it applies to all types of testimony, they just literally have not thoroughly read *Daubert*. Rule 702 says scientific, technical, or specialized knowledge. You have three types of testimony, and most courts overlook the distinction. Footnote 8 of *Daubert* is very, very explicit. It says, “Rule 702 also applies to technical or other specialized knowledge. Our discussion is limited to the scientific context because that is the nature of the expertise offered here.”<sup>12</sup> So, if you apply this only to scientific evidence—I say you can go one step further: novel scientific evidence—you are going to come up with the right answer. But you are not going to get novel scientific evidence. In 18 years, I probably have had two cases involving novel scientific evidence: two toxic tort cases. I let the evidence in. I probably still would today.

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*When information is peculiar to an industry and you don't have access to it, that does bother me.... If the research has been funded by the industry or the universities and so forth, and it is peculiar to them, then what difference is it going to make whether you have one standard or the other?*

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In our western state, as long as the expert is qualified and the testimony is relevant and will assist the trier of fact, we have allowed scientific expert opinion.



I agree that *Daubert* could evolve into a monster. But it does not have to. As long as it is invoked for the purpose of setting out hopeful suggestions as to criteria that can be considered in making the threshold admissibility or qualification determination that a trial court has to make, that is fine. But it's another matter if we forget the original *Daubert* disclaimer<sup>13</sup> that the list of factors is not exclusive, that it is just suggestive, and that the weight to be accorded to each of the factors is not being quantified here, and treat the four inquiries as if they are cast in stone and applied across the board to any scientific question.

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12 See Background section, n.16.

13 See Background section, n.15.



Often, manufacturers of medical products don't reveal to other doctors the reports of side effects that they are receiving. They will not tell them, "Yes, there is excessive inflammation. Yes, we can see it is still not being absorbed." All of these reports are there, but they are within the heart and soul of the drug company that is now cheering the thing. So where is the scientific consensus that we are supposed to be looking for, and where does the trial judge find that consensus to allow someone to testify to this or not allow it? What do we do as reviewing court judges with respect to the evaluation?



I wonder if anyone has a notion whether or not we as judges have some responsibility to the larger community that is subsumed within the concept of justice, so that manufacturers of toxic substances (*legal* toxic substances) or pharmaceutical companies are subject to (or, more importantly, vulnerable to) a finding of liability on a "more likely than not" standard, where scientific truth is not yet known, nor can it be known for some time?

This would necessarily have—and probably has to some degree had—a dampening effect on that industry and on the willingness of those who are in that industry to conduct research and bring new products onto the market that over time would be shown to be helpful to the human condition. Is that part of our responsibility as judges, or are we just supposed to, in doing justice in individual cases, get "close enough for government work" and accept a finding of "more likely than not," knowing that this will have a deleterious effect upon an industry that is extremely important not only to the economy but to the health and well-being of our citizens and, more globally, to the leadership role of this country in the area of science?

To go just one step further, I spent a lot of years before juries, and I have a lot of confidence in juries, I really do. I think it is the best way to resolve essentially insoluble, intractable questions: finally get a jury decision, and that is the end of it. But even if juries were given the opportunity to factor that into their consideration in an individual case—and they are not—it would be improper, I think, for a court to instruct them upon that possible consequence of a decision for the plaintiff when 95 percent certainty was not in the picture. But even if they were so instructed, I don't think juries would be able to factor that into their decision when they are looking at a plaintiff who has been terribly ravaged by something—allegedly the use of a particular toxic substance or pharmaceutical. Maybe that is the cause and maybe it isn't the cause.

Maybe justice is served by saying, "Well, more likely than not it is, and hell, we're not scientists, someday science will figure it out to 95 percent of the probabilities. But that isn't our problem. We are here to do justice. There's a couple of million bucks." And a pharmaceutical industry goes down the tubes.



I am wondering what the empirical data is for the connection between jury verdicts and pharmaceutical companies going “down the tubes.”



It seems to me what you’re talking about is what was earlier mentioned as Justice Andrews’s reference to the practical politics of the law,<sup>14</sup> which I think in contemporary discussions has become referred to as the law and economics, which is something taught at Vanderbilt in particular.

But it also seems to me that those are decisions and relationships that are being more and more commonly addressed by legislatures as opposed to courts. What has amazed me is that legislators demand so much less of the industry, which claims economic hardship in terms of a causal relationship, than industry would demand of plaintiffs who want to offer scientific evidence. There seems to be a certain inconsistency there.



I spent 19 years on the trial bench, and I always operated under the rule that evidence is admissible unless there is some reason to exclude it. There has to be some reason why it should be excluded. I think that is important philosophically for the trial judge. The burden really should be on the other side.



I really think that ultimately we come down to the question of whether the evidence is going to assist the trier of fact, and all of these wonderful tests are just sort of thrown in.

**I. DO YOU BELIEVE THERE IS A SIGNIFICANT PROBLEM WITH UNRELIABLE SCIENTIFIC TESTIMONY, SUCH THAT MISCARRIAGES OF JUSTICE OCCUR IN MORE THAN A TINY MINORITY OF CASES?**

*The judges generally did not consider their past experiences with scientific evidence to have been overly challenging, and did not believe there is a major problem involving scientific evidence.*

**DISCUSSION EXCERPTS**

We’re talking numbers. What percentage of the cases that we see are toxic tort or medical malpractice or products liability? I am involved in a committee fighting about a rule change on medical malpractice. I checked the numbers and found that last year in our state we had some 36,000 tort cases filed, and 300 of them were medical malpractice. And how many of those went to trial? It is really not a huge problem in

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<sup>14</sup> See remarks of Linda Atkinson, Esquire, p. 59, n.2.

terms of numbers. This may be very significant for the individual case, and that's important, but we are not talking about large numbers of cases.



As a trial court judge here, my experience is that the problems surveyed by the Federal Judicial Center<sup>15</sup> are not, in the sphere of my experience, "problems." I wonder if the study included the question, "Are problems with expert witnesses one of your major concerns?"



We get all excited about this *Daubert* decision, and it just affects corporate America's pocketbook. Why are we not equally excited about the jailhouses being full of defendants convicted on eyewitness testimony, which, as we all know—and every study shows—is some of the most unreliable testimony in the world?



I suggest that the perception that there is a great, humongous problem out there is not real. The alternative is that we set up judges as super scientists. I suspect every trial judge has excluded junk stuff sometimes, long before *Daubert* ever came along. Everyone did that under the existing rules. I say, "If it ain't broke, don't fix it."



Sometimes you have an extremely credible hired gun who overwhelms the jury. You sit there and you think, "I can't believe they believe that," and yet they did. But that is part of the judicial process, part of the process of trying cases.



I think the concern here is that the balance has shifted in certain areas, and the toxic tort field is one. The defendants have done all the research, they have financed all the papers, and the plaintiffs can't find any experts. Is that what the trial lawyers are whining about?



I have been a lawyer for 35 years, and I have been a judge for 20 years. In all that time, only once have I seen somebody testify as an expert who was not qualified or didn't have the credentials to do that. And actually, that witness committed perjury. He lied with respect to his credentials. I might also say that I think I disagreed with a jury verdict only once.

What I am concerned about in *Daubert* is that it makes us pseudoscientific scientists. We are not that, and I

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<sup>15</sup> See oral remarks of Dr. Joe S. Cecil, p. 62 supra.

don't see where we are really any more qualified than a juror would be if you had a very intelligent jury—and most lawyers do pick intelligent juries for this type of litigation.

I can't see why this additional burden of proof is put on any attorney for either the plaintiff or the defendant without a necessity for it. Unless I have a witness who doesn't have the credentials or the expertise or the background, then I always follow the rule that I followed for 30 some years: If you are in doubt, let the evidence in.



Scientific evidence has been a problem in our southern state, and it is getting to be more of a problem. You've got witnesses all over the country who agree to testify to the conclusions that their employers want them to. So you end up with cases where you have competing experts whose opinions are diametrically opposed. The problem with that is that the jurors, because of the particular aura of respectability that attaches to anyone who is qualified as an expert, tend to believe that expert testimony is reliable. When they have competing experts, I believe the jurors go with the one they think is the more believable. They do not, in my opinion, test the underlying reliability of the opinions. They just pick the one that they think is the more believable. I believe it is probably true that jurors are adept at recognizing fakes, phonies. But I don't believe that jurors are very adept at recognizing genuine experts who use incompetent methodology. I think judges are better at recognizing bad methodology, because judges are supposed to be experts at *judging*. Jurors are not.



I recall Professor Jasanoff saying that “a major risk of the *Daubert* approach . . . is inequity, as litigants with similar complaints are subjected by gatekeepers to substantially different evidentiary standards and validation processes.”

To that I ask, what else is new? I do not think that is new. I maintain that in this country we have always had that. We have had criminal defendants who have been convicted in the Northwest or in the South who would never have been convicted on the same evidence in the East. In our southwestern state we have the same thing. In other words, judges have been making decisions in different parts of this country for years. So I say that this has been happening forever. We really do not have a system of equal justice in this country. We never have, and I do not think we ever will.



I am a trial-level judge and had the experience recently of trying a major negligence action. It is one of two decisions of its kind that have gone to a verdict. I guess my experience in the case was one that confirms my confidence in the system to test the reliability of scientific evidence. A question of causation was the subject of about four days of expert testimony. It became apparent that the biases that existed in the scientific evidence were flushed out by the defense in the case as well as by the plaintiffs on cross-examination.

My thoughts are that the questions surrounding what might be in the scientific mainstream are well addressed and thoroughly explored through the judicial system's process of testing evidence. The standard that should be the threshold is not as important, in my estimation—having sat through that trial—as the skills litigators bring to the process in testing for the truth.



I have never excluded an expert opinion at all. But if there is one that is so lacking in basis, I would have to keep it out.



I may have seen the problem of unreliable scientific evidence one time in 40 years, and in the same 40 years I have had many knotty problems that were rather severe. So I guess my bottom line is, I don't see it as a problem at all.

**J. WHAT IS THE APPROPRIATE STANDARD OF REVIEW ON APPEAL FROM A TRIAL COURT'S RULING ON SCIENTIFIC EVIDENCE?**

*The judges, and their jurisdictions, were divided on the question of appellate review, which was then pending before the United States Supreme Court.<sup>16</sup> Some considered the admissibility issue a question of fact and others viewed it as a question of law.*

**DISCUSSION EXCERPTS**

Why can't an appellate judge say, "You are wrong as a matter of law. This is not science. I do not care how many facts you find; it is not, because there is only one type of science. There is only one science"?



In our court, we give a considerable amount of discretion to the trial court where admission of evidence is concerned, but then frequently the question comes before us as to whether or not that testimony presents substantial evidence to support the verdict.



It seems to me that it is definitely a *de novo* review. It pretty much has to be. There is not one right answer.



In our northwestern state, our experience has been limited to the criminal cases involving DNA evidence, where the issue was whether or not there was a significant data base for the background of the testing for DNA. In a criminal case, the trial court makes a preliminary determination of the admissibility of the evidence. That is clearly a reviewable issue of law for the appellate courts—a question of law, not a question of fact.

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<sup>16</sup> See Background section, n.16.

In the civil cases, if it is a mixed question of law and fact, if the ultimate decision really is based on undisputed facts, it is a legal question that is reviewable as a question of law in the appellate courts.



All of the debate that is now in law journals is very new. As we build a culture of understanding in the state courts about how to approach this new topic, it is important to understand how much of it really is a matter of law that is fully reviewable at the appellate level.

There is very little in the methodology discussion and the debate about the lawfulness of permitting a certain expert to testify that is really fact-bound and therefore subject to deferential appellate review. It is almost all going to be done on briefing and exhibits offered in support of an expert's testimony—not really from the standpoint of somebody testifying in a factually conclusive way and then a finding being made factually. That is the whole difference. I think the appellate courts are going to find that more and more often they are going to have sweeping powers of review.



There are a couple of practical problems. One is *Frye* being looked at in terms of novel scientific evidence. *Daubert* is applying a general rule of evidence that covers a far broader area, and you are getting into stuff that we never, ever apply *Frye* to, like psychiatric testimony. That is not a *Frye* issue, but it may be a *Daubert* issue. So it is a broader scope because it involves a general rule of evidence.

The other practical problem is, How many courts have to reinvent the wheel? If you get the supreme court of State X that goes through an exhaustive analysis and comes out with a 97-page opinion saying it is good or it is not good, then, when we get the same case, do we have to go through the same stuff or can we say, "The Supreme Court of State X already decided this question"? You could have two trial courts in adjoining counties with different evidence, different experts. One says it is this; another one says it is that. If you use an abuse of discretion standard, you could have the supreme court affirming both.



What the Supreme Court is affirming is the trial court's prerogative to let the witness talk. The Supreme Court is not placing its imprimatur on the validity of what the expert says.



It is very difficult for me, as an appellate judge, to set aside evidence that has been admitted in the trial court.

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*The credibility issue is the quintessential jury decision. Why should the appellate court defer to a trial judge to make that determination when the judge has essentially blocked the jury from doing its job?*

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I think this is a relevancy issue, and the determination of whether evidence is relevant is a question of law. Whether it should be excluded under Rule 403 for probative value being outweighed by prejudice may involve the exercise of discretion. That initial admissibility question does not meet the threshold for relevance as a question of law.



I think I would agree with the DC court that said you go *de novo*, because the case is ultimately disposed of on review. In an appellate court, you would go *de novo*. It goes to the question of admissibility, which doesn't depend so much on credibility, which you normally defer to the fact-finder.



The credibility issue is the quintessential jury decision. Why should the appellate court defer to a trial judge to make that determination when the judge has essentially blocked the jury from doing its job?



As an appellate judge, let's say I review a trial judge for abuse of discretion, on a question of qualification of a witness as an expert. We look at the methodology before the trial judge, and there is some evidence that the expert based the opinion on a flip of the cards—if seven comes up first there's causation. If that trial judge excludes that person as an expert witness, I can only affirm. I can't say there was an abuse of discretion. If a trial judge lets it in, though, I may reverse in that situation because it is clearly abuse. No reasonable person would have allowed that person to be qualified as an expert. What's wrong with that?



I think whether we should abandon *Frye* and adopt *Daubert* is a question of law. But I think that determining whether or not someone should be permitted to testify, and looking at his qualifications and relevance and all that, I think that is a question of fact. I don't think that is a question of law. So as a member of a court of review, why should we substitute our judgment on an issue of fact? I think it is a question of fact, not law.



In our western state, it is a question of law. The admissibility of scientific evidence is viewed as a question of law with complete *de novo* review. It is odd, but I know that in the federal system that is not true, which leads to a very different review process depending upon which side of the street and which courthouse you choose.

**K. WHAT ABOUT THE COST (TO LITIGANTS AND TO THE PUBLIC AS WELL) OF EXTENSIVE COURT PROCEEDINGS ON THE ADMISSIBILITY OF SCIENTIFIC EVIDENCE?**

*Several judges voiced concern over the cost of Daubert proceedings, both to litigants in terms of dollars and to the public through drain on the courts' limited resources of money and time.*

**DISCUSSION EXCERPTS**

I can see the philosophy of the plaintiffs' attorneys, I can identify with that. On the other hand, I can see from the judiciary standpoint that litigation is taking too long. At this time, a lawsuit takes 10 times as long as it should because of all of these experts and so forth. I think that they take enough of the court system. I can see why a judge would want to set you up for a summary judgment to dispose of that case and get it out of the court.



What does the scientific evidence controversy mean for the trial judge? Are the appellate judges going to drive their workloads up by saying you have to be very detailed in your findings and in your conclusions? That could take up a lot of judicial resources.



I think there is a lot of fear on our part that we are not the adjudicators of what is scientific knowledge, that we do not want to get into methodology. We do not want to be gatekeepers, we are not trained to be gatekeepers, and we literally cannot afford to be gatekeepers if we are going to have all of the *Daubert* hearings.



One thing that is important in criminal cases that isn't as important in toxic tort is the amount of resources a defendant has. The defendant can't go out and hire all sorts of great experts to point out that there is a problem with the psychological testimony, so we might have a different threshold in a criminal case too.



I do not know that the Supreme Court ever contemplated, in answering the question whether the *Frye* rule is a part of the Federal Rules of Evidence, that they would embolden some judges to set up this gigantic, months-long panoply. Who pays for all of these experts? Where do the lawyers come from on both sides?



I think the real basic question has to do with personal liberty. In the criminal context, you are concerned about the state being

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*I do not know that the Supreme Court ever contemplated, in answering the question whether the Frye rule is a part of the Federal Rules of Evidence, that they would embolden some judges to set up this gigantic, months-long panoply. Who pays for all of these experts? Where do the lawyers come from on both sides?*

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allowed to put on questionable evidence, and it can't be rebutted by an impecunious defense attorney. Should the state be allowed to come in with "junk science," or whatever you want to call it, when a person's life might be at stake?



State judges do not have the luxury of having week-long hearings with court-appointed experts, typically. What happens is, you are in day four of the trial and the plaintiff calls an expert, they take the stand, there is a motion to exclude, and you have to decide quickly. That is typically how it happens in state court.



I think there are some inherent problems with court-appointed experts. One concern is the cost.



When the court appoints its own expert, who pays the tab if there isn't a grant?



Another concern that I think is rather significant is *the cost to litigants*, and certainly in state court proceedings I think we are perhaps a little bit more sensitive to that than a federal judiciary, I think for obvious reasons.



## V. Points of Agreement and Closing Comments

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In the discussion groups, the moderators were asked to seek out consensus—where it could be achieved—on the issues raised by the standardized questions, and to characterize their groups' discussions in a few sentences, to be announced at the Closing Plenary Session. The questions, the moderators' informal summaries of their groups' discussions, and two closing comments follow, edited for clarity.

- A. DO YOU OFTEN HAVE OCCASION TO RULE ON THE ADMISSIBILITY OF SCIENTIFIC EVIDENCE?
- B. DOES YOUR STATE HAVE RULES OF EVIDENCE BASED ON THE FEDERAL RULES OF EVIDENCE? HAS IT HAD OCCASION TO FOLLOW OR REJECT DAUBERT?

In our group we learned that *Frye* is still the active case in many jurisdictions, as opposed to *Daubert*.

Most people thought that the *Daubert* rule was initially supposed to be liberalizing the admissibility of testimony, but that has not always been true.

Some states have adopted *Daubert* because they feel it is a better standard, and we heard that other states had not adopted *Daubert* because they felt it was too loose. Some states felt that *Daubert* allowed more evidence in, and some states felt *Daubert* prevented evidence from coming in. However, everyone stated that there was a need for simplicity to the extent possible.

Probably a third of the jurisdictions that were represented in our group have followed *Daubert*, a third are somewhere in between, and a third are still with *Frye*.

There was a feeling that *Daubert* had relaxed the admissibility standards regardless of what the federal courts have done with it—that that is what it should have done.

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*There was a feeling that Daubert had relaxed the admissibility standards regardless of what the federal courts have done with it—that that is what it should have done.*

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**C. IN YOUR COURTROOM WORK, DO YOU TAKE INTO CONSIDERATION THE DIFFERENCE BETWEEN THE STANDARD OF PROOF THAT SATISFIES SCIENCE AND THE LEGAL STANDARD OF PROOF?**

We generally agreed that if you have biological plausibility, and general causation is based on a 95 percent factor, then you have revolutionized tort law.

We did hear from one person who thought that if the witness is an expert and meets the preponderance test, which is more likely than not, then it is a question of reliability, and 95 percent proof is an inane standard of proof.

**D. HAVE YOU EVER USED A COURT-APPOINTED EXPERT? UNDER WHAT CIRCUMSTANCES?**

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*We generally agreed that if you have biological plausibility, and general causation is based on a 95 percent factor, then you have revolutionized tort law.*

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There was a fair consensus in our group that court-appointed experts were not the way to go.

We probably met the same results as the other groups, with one exception. We had a discussion about court-appointed experts in the context of family law, where you have experts testifying that a child has been abused based on behavior patterns, or that a woman has been raped based on behavior patterns, or that a parent is unfit because of sexual orientation, with epidemiological studies being offered.

**E. OF THE FIVE MODELS OF JUDGING DESCRIBED BY PROFESSOR JASANOFF (I.E., “THE INQUISITOR,” “THE GATEKEEPER,” “THE REFEREE,” “THE MEDIATOR,” AND “THE JUDGE”), WHICH DO YOU FAVOR? WHAT STRENGTHS AND WEAKNESSES DO YOU SEE IN EACH OF THEM?**

We heard repeatedly from judges that they look to the individual circumstances and the context in which the issues arise in judging what happened at the trial-court level.

We heard that some judges apparently consider epidemiology to be—or accept the fact that it is—a “soft” science versus some of the other approaches, like animal studies, and believe that judges are making determinations of admissibility based on how rigorous the particular science is.

**F. HOW EFFECTIVE DO YOU FIND CROSS-EXAMINATION TO BE IN PROTECTING PARTIES' INTERESTS IN MATTERS OF SCIENTIFIC EVIDENCE?**

There was pretty much the conclusion that cross-examination does work, although there was a lot of discussion as to what kind of protection the system needs.

This afternoon, we discussed what the best “truth filter” is, and our group did have a consensus that cross-examination was the best truth filter.

**G. HOW EFFECTIVE DO YOU FIND LAY FACT-FINDERS (BOTH JUDGES AND, COLLECTIVELY, JURIES) TO BE IN UNDERSTANDING AND MAKING DECISIONS BASED ON SCIENTIFIC TESTIMONY?**

Overall, we felt we should trust the jury. We don't give the jury enough credit. The judges should be more judge than gatekeeper and keep out the voodoo, but only if it is clearly voodoo.

There is some question of why federal courts are going so far in taking decisions away from the jury—that perhaps a better way is to use Rule 403, and if there are specific pieces of evidence that you don't think should come in, use Rule 403 and take them out, as has traditionally been done. But most of these decisions should go to the jury.

Although we drew different conclusions, our methodology was great. We didn't have consensus, except that the right to the jury trial should prevail.

If I could say anything by way of a summary, it was basically an abiding respect for the jury system—that, on the whole, juries are very good at evaluating the evidence.

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*Overall, we felt we should trust the jury. We don't give the jury enough credit. The judges should be more judge than gatekeeper and keep out the voodoo, but only if it is clearly voodoo.*

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**H. WHAT CRITERIA HAVE YOU USED IN RULING ON ADMISSIBILITY OF SCIENTIFIC TESTIMONY?**

THE EXPERT'S CREDENTIALS? PEER REVIEW OF THE EXPERT'S CONCLUSIONS? PUBLICATION OF RESULTS IN PROFESSIONAL JOURNALS? EMPLOYMENT OF GENERALLY ACCEPTED SCIENTIFIC METHOD? REACHING "MAINSTREAM" CONCLUSIONS? TESTABILITY OF THE THEORY OR TECHNIQUE? KNOWN ERROR RATE? THE EXPERT'S FREQUENCY OF INVOLVEMENT IN PROVIDING TESTIMONY? TESTIMONY OUTSIDE OF THE EXPERT'S USUAL AREA OF EXPERTISE? THE FACT THAT THE EXPERT'S RESEARCH WAS UNDERTAKEN IN RESPONSE TO LITIGATION RATHER THAN PRIOR TO LITIGATION? OTHERS?

We tried to discuss the issue of methodology and conclusion, and didn't really come up with an answer as to where the reliability issue comes into play on that.

There was a definite feeling on the part of some judges that the criteria set out in *Daubert* should be applied only to novel scientific evidence, relying in part on footnote eight of the opinion.<sup>1</sup>

There was a feeling that the *Daubert* factors perhaps should go to weight, not admissibility, and should not be used by a judge except in extreme cases.

There was an opinion expressed that the burden should be on the challenger of the evidence, not the proponent of the evidence, so that the burden should be on the challenger to show that there is a serious question about the reliability before any issue should be determined by the court.

We had virtually a consensus that the focus should be on methodology, certainly not conclusion.

**I. DO YOU BELIEVE THERE IS A SIGNIFICANT PROBLEM WITH UNRELIABLE SCIENTIFIC TESTIMONY, SUCH THAT MISCARRIAGES OF JUSTICE OCCUR IN MORE THAN A TINY MINORITY OF CASES?**

The only consensus we reached is that it isn't really a big problem in state courts, except in one Southern state, where it apparently has become a problem recently. That state has adopted *Daubert*.

We should remember the point that Professor Berger made this morning, that we are really dealing with different fields of law here. The rules probably should be different for different fields of law. And toxic torts are one thing, rare in state court, whereas family law problems are much more common and may require a different method for the court to use.

We had someone say that these issues have been handled for a long time, that "if it ain't broke, it shouldn't be fixed."

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<sup>1</sup> See Background section, n.16.

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Rick Weiss, *Nobel Prize Vindicates U.S. Scientist*, WASH. POST, Oct. 7, 1997, A1

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## WORLD WIDE WEB SITES

American Medical Association resolution on discipline of medico-legal experts who give false courtroom testimony:

<[www.ama-assn.org/meetings/public/int97/finalrpt/i97-refb.htm](http://www.ama-assn.org/meetings/public/int97/finalrpt/i97-refb.htm)>.

Analyses and discussion of Brown & Williamson Tobacco Corporation documents published in the *Journal of the American Medical Association* (JAMA):

<[http://www.ama-assn.org/scipubs/journals/archive/jama/vol\\_274/no\\_3/toc.htm](http://www.ama-assn.org/scipubs/journals/archive/jama/vol_274/no_3/toc.htm)>

The “cosmic snowball” phenomenon:

<<http://smallcomets.physics.uiowa.edu>>.

“Frequently Asked Questions in Mathematics”:

<<http://daisy.uwaterloo.ca/~alopez-o/math-faq/math-faq.html>>.

The Public Broadcasting Service’s program on Fermat’s Last Theorem:

<[www.pbs.org/wgbh/nova/proof](http://www.pbs.org/wgbh/nova/proof)>

Research on prions conducted by Dr. Stanley Prusiner:

<<http://keck.ucsf.edu/neuroscience/prusiner.htm>>.

## B. COURT RULES CITED IN FORUM PAPERS AND DISCUSSIONS

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### FEDERAL RULES OF EVIDENCE

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#### RULE 102. PURPOSE AND CONSTRUCTION

These rules shall be construed to secure fairness in administration, elimination of unjustifiable expense and delay, and promotion of growth and development of the law of evidence to the end that the truth may be ascertained and proceedings justly determined.

#### RULE 103. RULINGS ON EVIDENCE

(a) **Effect of erroneous ruling.** Error may not be predicated upon a ruling which admits or excludes evidence unless a substantial right of the party is affected, and

(1) Objection. In case the ruling is one admitting evidence, a timely objection or motion to strike appears of record, stating the specific ground of objection, if the specific ground was not apparent from the context; or

(2) Offer of proof. In case the ruling is one excluding evidence, the substance of the evidence was made known to the court by offer or was apparent from the context within which questions were asked.

(b) **Record of offer and ruling.** The court may add any other or further statement which shows the character of the evidence, the form in which it was offered, the objection made, and the ruling thereon. It may direct the making of an offer in question and answer form.

(c) **Hearing of jury.** In jury cases, proceedings shall be conducted, to the extent practicable, so as to prevent inadmissible evidence from being suggested to the jury by any means, such as making statements or offers of proof or asking questions in the hearing of the jury.

(d) **Plain error.** Nothing in this rule precludes taking notice of plain errors affecting substantial rights although they were not brought to the attention of the court.

#### RULE 104. PRELIMINARY QUESTIONS

(a) **Questions of admissibility generally.** Preliminary questions concerning the qualification of a person to be a witness, the existence of a privilege, or the admissibility of evidence shall be determined by the court, subject to the provisions of subdivision (b). In making its determination it is not bound by the rules of evidence except those with respect to privileges.

**(b) Relevancy conditioned on fact.** When the relevancy of evidence depends upon the fulfillment of a condition of fact, the court shall admit it upon, or subject to, the introduction of evidence sufficient to support a finding of the fulfillment of the condition.

**(c) Hearing of jury.** Hearings on the admissibility of confessions shall in all cases be conducted out of the hearing of the jury. Hearings on other preliminary matters shall be so conducted when the interests of justice require, or when an accused is a witness and so requests.

**(d) Testimony by accused.** The accused does not, by testifying upon a preliminary matter, become subject to cross-examination as to other issues in the case.

**(e) Weight and credibility.** This rule does not limit the right of a party to introduce before the jury evidence relevant to weight or credibility.

#### **RULE 201. JUDICIAL NOTICE OF ADJUDICATIVE FACTS**

**(a) Scope of rule.** This rule governs only judicial notice of adjudicative facts.

**(b) Kinds of facts.** A judicially noticed fact must be one not subject to reasonable dispute in that it is either (1) generally known within the territorial jurisdiction of the trial court or (2) capable of accurate and ready determination by resort to sources whose accuracy cannot reasonably be questioned.

**(c) When discretionary.** A court may take judicial notice, whether requested or not.

**(d) When mandatory.** A court shall take judicial notice if requested by a party and supplied with the necessary information.

**(e) Opportunity to be heard.** A party is entitled upon timely request to an opportunity to be heard as to the propriety of taking judicial notice and the tenor of the matter noticed. In the absence of prior notification, the request may be made after judicial notice has been taken.

**(f) Time of taking notice.** Judicial notice may be taken at any stage of the proceeding.

**(g) Instructing jury.** In a civil action or proceeding, the court shall instruct the jury to accept as conclusive any fact judicially noticed. In a criminal case, the court shall instruct the jury that it may, but is not required to, accept as conclusive any fact judicially noticed.

#### **RULE 403. EXCLUSION OF RELEVANT EVIDENCE ON GROUNDS OF PREJUDICE, CONFUSION, OR WASTE OF TIME**

Although relevant, evidence may be excluded if its probative value is substantially outweighed by the danger of unfair prejudice, confusion of the issues, or misleading the jury, or by considerations of undue delay, waste of time, or needless presentation of cumulative evidence.

#### **RULE 702. TESTIMONY BY EXPERTS**

If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise.

#### **RULE 703. BASES OF OPINION TESTIMONY BY EXPERTS**

The facts or data in the particular case upon which an expert bases an opinion or inference may be those perceived by or made known to the expert at or before the hearing. If of a type reasonably relied upon by experts in the particular field in forming opinions or inferences upon the subject, the facts or data need not be admissible in evidence.

#### **RULE 705. DISCLOSURE OF FACTS OR DATA UNDERLYING EXPERT OPINION**

The expert may testify in terms of opinion or inference and give reasons therefor without first testifying to the underlying facts or data, unless the court requires otherwise. The expert may in any event be required to disclose the underlying facts or data on cross-examination.

#### **RULE 706. COURT APPOINTED EXPERTS**

**(a) Appointment.** The court may on its own motion or on the motion of any party enter an order to show cause why expert witnesses should not be appointed, and may request the parties to submit nominations. The court may appoint any expert witnesses agreed upon by the parties, and may appoint expert witnesses of its own selection. An expert witness shall not be appointed by the court unless the witness consents to act. A witness so appointed shall be informed of the witness' duties by the court in writing, a copy of which shall be filed with the clerk, or at a conference in which the parties shall have opportunity to participate. A witness so appointed shall advise the parties of the witness' findings, if any; the witness' deposition may be taken by any party; and the witness may be called to testify by the court or any party. The witness shall be subject to cross-examination by each party, including a party calling the witness.

**(b) Compensation.** Expert witnesses so appointed are entitled to reasonable compensation in whatever sum the court may allow. The compensation thus fixed is payable from funds which may be provided by law in criminal cases and civil actions and proceedings involving just compensation under the fifth amendment. In other civil actions and proceedings the compensation shall be paid by the parties in such proportion and at such time as the court directs, and thereafter charged in like manner as other costs.

**(c) Disclosure of appointment.** In the exercise of its discretion, the court may authorize disclosure to the jury of the fact that the court appointed the expert witness.

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## C. PARTICIPANTS' BIOGRAPHIES

### PAPER WRITERS

Professor **Sheila Jasanoff** is founding chair of Cornell University's Department of Science and Technology Studies. She is the author of over 50 articles and book chapters and has written or edited several books, including *The Fifth Branch: Science Advisers as Policymakers* (Harvard 1990); *Science at the Bar: Law, Science and Technology in America* (Harvard 1995); *the Handbook of Science and Technology Studies* (ed. with Gerald Markle, James Petersen, and Trevor Pinch, Sage Publications, 1994); and *Learning From Disaster: Risk Management After Bhopal* (ed.; University of Pennsylvania 1994). (*The Fifth Branch* was cited by the U.S. Supreme Court in its *Daubert* decision.) Among other degrees, she holds a PhD in linguistics from Harvard University and a JD from Harvard Law School.

Professor **Michael Gottesman** teaches torts, labor law, and international law at the Georgetown University Law Center in Washington, DC. He has argued numerous cases before the United States Supreme Court, including *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, on behalf of the petitioners. He is a member of the Executive Board of the Lawyers Committee for Civil Rights Under Law and of its Amicus subcommittee.

### LUNCHEON SPEAKER

The Honorable **Grant Woods** has served as Attorney General of Arizona since 1990. He directs a staff of 850, including 300 attorneys, and personally represents the State in court hearings, trials, sentencings, and appellate arguments, including oral argument in the United States Supreme Court in *Lewis v. Casey*, 116 S.Ct. 2174 (1996), a case concerning inmate access to the court system. He was named "Attorney General of the Year" in 1995 by the National Association of Attorneys General. In addition to his official duties, he hosts a two-hour weekly radio program.

### PANELISTS

**Anthony Z. Roisman** is a full-time environmental and toxic tort lawyer. He lives and practices in Lyme, New Hampshire, teaches part-time at Dartmouth College, and is of counsel to a Washington, DC, law firm. He has served as a member of the National Research Council's Committee on Nuclear Safety Research and as chief, and then as special litigator, with the Hazardous Waste Section of the U.S. Department of Justice's Land and Natural Resources Division. He was the founding executive director of Trial Lawyers for Public Justice and currently serves as the Association of Trial Lawyers of America's designated adviser to a National Judicial College program on scientific evidence and as an adviser for The Roscoe Pound Foundation's Roscoe Hogan Environmental Law Essay Contest.

**Margaret A. Berger** is Professor of Law at Brooklyn Law School and the author of the Evidentiary Framework of the Federal Judicial Center's *Reference Manual on Scientific Evidence* (FJC 1994). She is also co-author (with U.S. District Judge Jack Weinstein) of *Weinstein on Evidence* and *Weinstein's Evidence Manual*. She serves as a consultant to the Carnegie Commission on Science, Technology and Government, on whose behalf she filed an amicus curiae brief in *Daubert* in support of neither party.

The Honorable **Mark I. Bernstein** is a judge of the Court of Common Pleas of Philadelphia County, Pennsylvania, and the author of *Expert Testimony in Pennsylvania*, 68 Temple L. Rev. 699 (1995). He has served as a member of a Court of Common Pleas reform committee appointed by the Pennsylvania Supreme Court and has been active in other innovative case management and alternative dispute resolution programs.

**Linda Miller Atkinson** is an experienced products liability and medical malpractice litigator in Detroit and Channing, Michigan. She is a prolific writer and lecturer on litigation practice subjects, including discovery, expert testimony, and pharmaceutical litigation. She is a fellow of The Roscoe Pound Foundation, was a founding member of the Women Trial Lawyers Caucus, and is a member of the board of governors of the Association of Trial Lawyers of America.

**Joe S. Cecil** holds both a JD and a PhD in psychology from Northwestern University. He is director of the Program on Scientific and Technical Evidence at the Federal Judicial Center in Washington, DC, and was principal editor of the Center's *Reference Manual on Scientific Evidence*. He has taught in both the law school and the graduate school at Northwestern University, and is the author of several articles on the use of court-appointed experts.

The Honorable **Victoria Lederberg** is a justice of the Rhode Island Supreme Court. In addition to her law degree, she holds an AM in biology and a PhD in psychology, both from Brown University. She served in both the Rhode Island House and Senate, and was appointed by President Jimmy Carter to serve as chair of the National Advisory Panel on Financing Elementary and Secondary Education in 1979–82. She has taught at the college level, and has served as a trustee of both Brown University and Roger Williams University.

#### **DISCUSSION GROUP MODERATORS**

**Leo V. Boyle** is completing his term as the Association of Trial Lawyers of America parliamentarian. He is a past president of the Boston and New England Bar Associations and a past member of the American Bar Association House of Delegates, a fellow of the American College of Trial Lawyers and of the American Bar Foundation, and a trustee and fellow of The Roscoe Pound Foundation.

**Kathryn Clarke** is an appellate lawyer and complex litigation consultant in Portland, Oregon. She specializes in medical negligence, products liability, punitive damages, and constitutional litigation and has briefed scientific evidence issues in both state and federal courts.

**Tom H. Davis**, of the firm of Slack & Davis, Austin, Texas, is an experienced litigator with an emphasis on aviation accident law. He is a fellow and honorary trustee of The Roscoe Pound Foundation. A past president of the Association of Trial Lawyers of America, Mr. Davis is also a member of the International Society of Barristers, the Lawyer-Pilots Bar Association, and the International Society of Air Safety Investigators.

**Linda Eyerman** practices law in Portland, Oregon, with a concentration in products liability, medical negligence, and employment discrimination. A former public defender, she is a frequent speaker at continuing legal education programs. She currently serves as treasurer of the Oregon Trial Lawyers Association and is a past chair of the Oregon State Bar's continuing legal education board.

**William A. Gaylord** practices in Portland, Oregon, specializing in major products liability and medical negligence litigation. Most recently he has been integrally involved in breast implant litigation. He is a member of Trial Lawyers for Public Justice and a past president of the Oregon Trial Lawyers Association.

**Mark S. Mandell** is completing his term as vice president of the Association of Trial Lawyers of America. He is a board-certified civil trial specialist and has served as president of both the Rhode Island Trial Lawyers Association and the Rhode Island Bar Association. He is a fellow and trustee of The Roscoe Pound Foundation.

**Barry J. Nace**, of the law firm of Paulson, Nace and Norwind in Washington, DC, practices predominantly in the areas of medical malpractice and products liability. In that capacity, he represented the plaintiffs in *Daubert v. Merrell Dow Pharmaceuticals, Inc.* He was also counsel for the plaintiffs in *Ambrosini v. Labarraque*, 966 F.2d 1464 (D.C. Cir. 1992), and in *Oxendine v. Merrell Dow Pharmaceuticals, Inc.*, 506 A.2d 1100 (D.C. App. 1986), 649 A.2d 825 (D.C. App. 1994). He has served as president of the Association of Trial Lawyers of America and of the District of Columbia Trial Lawyers Association, and is a fellow and honorary trustee of The Roscoe Pound Foundation.

**Gerson Smoger** practices law in Oakland, California, and Dallas, Texas, with a concentration in environmental and toxic tort cases. Before attending law school he received an interdisciplinary PhD from the University of Pennsylvania; he later worked at the United Nations Commission on Human Rights. He served as lead counsel in the Times Beach, Missouri, toxic pollution litigation, and represented a group of veterans' service organizations as *amici*, contesting the Agent Orange class action settlement before the U.S. Supreme Court in 1994. He is a member of the board of governors of the Association of Trial Lawyers of America, a director of Trial Lawyers for Public Justice, and vice-chair of the Toxic Torts and Environmental Law

Committee of the American Bar Association's Tort and Insurance Practice Section. He has lectured on litigation and environmental subjects throughout the United States (including at the National Judicial College in Reno, Nevada) and in Russia, Austria, and Vietnam. He is a fellow of The Roscoe Pound Foundation and chair of its Environmental Law Essay Contest.

**Gayle L. Troutwine** practices law in Portland, Oregon, specializing in products liability, toxic tort, and professional negligence litigation. She has served as a member of plaintiffs' steering committees for both L-tryptophan and breast implant litigation. She is a fellow and trustee of The Roscoe Pound Foundation and a current or past member of the boards of governors of the Association of Trial Lawyers of America, the Oregon Trial Lawyers Association, and the Western Trial Lawyers Association.

**Michael L. Williams** practices in Portland, Oregon, specializing in products liability, professional negligence, and toxic tort litigation. He has served as chair of the Oregon State Bar's Toxic Tort Section and of its procedure and practice committee. In addition to membership in the Association of Trial Lawyers of America and Trial Lawyers for Public Justice, he is a fellow of The Roscoe Pound Foundation.

#### **PLENARY SESSION AND LUNCHEON MODERATORS**

**Roxanne Barton Conlin**, a trial lawyer from Des Moines, Iowa, is completing her term as president of The Roscoe Pound Foundation. In addition to her leadership of Pound, she has served as president of the Association of Trial Lawyers of America, of the Civil Justice Foundation, and of the National Organization for Women's Legal Defense and Education Fund. She is a former U.S. Attorney for the State of Iowa and was the Democratic Party's 1982 candidate for state governor.

Program chair **Philip H. Corboy** practices law in Chicago. He has written and lectured prolifically on legal and trial practice topics, including evidence, damages, the jury system, products liability, medical negligence, and tort-related legislation. He has served as president of the Chicago Bar Association and of the Illinois Trial Lawyers Association, and as chair of the American Bar Association's Section of Litigation. He is also a director of the American Judicature Society, and a fellow of the American College of Trial Lawyers, of the International Academy of Trial Lawyers, and of the American Bar Foundation. He is chair of the ABA's Special Committee on Medical Professional Liability, and a past chair of its Litigation Section. He was a founding trustee and original finance committee chair of The Roscoe Pound Foundation. He has been active with the Law Science Academy, the National Safety Council, and the Cook County Task Force of Life Sustaining Treatment.

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## **D. JUDICIAL ATTENDEES**

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Honorable Ralph D. Cook, Justice, Supreme Court  
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## E. FORUM UNDERWRITERS

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# About the Roscoe Pound Foundation

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### **1997–1998**

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## **WHAT IS THE ROSCOE POUND FOUNDATION?**

The Roscoe Pound Foundation seeks to support and strengthen the U.S. civil justice system. It was established in 1956 to honor and build upon the work of Roscoe Pound, Dean of the Harvard Law School from 1916 to 1936. The Foundation sponsors programs, publications, awards, and grants that encourage open, ongoing discussion and debate among lawyers, academics, and others on issues affecting the civil justice system.

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## **WHAT PROGRAMS DOES THE FOUNDATION SUPPORT?**

**The *Civil Justice Digest*** is a quarterly publication that discusses news, research, and recent court decisions on the U.S. civil justice system. It is currently distributed to nearly 9,000 federal judges, state supreme court judges and intermediate appellate court judges, law libraries, law schools, law professors, attorneys, state and federal legislators, members of the media, and other interested groups.

**Pound Roundtables**—Private discussions among Fellows and other distinguished professionals bring a variety of views to bear on complex problems such as health care and the law and injury prevention in America.

**Forum for State Court Judges**—Judges from state Supreme Courts and Intermediate Appellate Courts come together with Pound Fellows to analyze issues affecting state courts.

**Pound Foundation Grants to Legal Scholars**—Grants for research on a variety of topics of concern to the trial bar are awarded by a jury of academics, jurists, and lawyers.

**Papers of The Roscoe Pound Foundation**—Reports of the Pound programs on health care and the law, injury prevention in the American workplace, the safety of the blood supply, and other topics are made available to jurists, academics, regulators, legislators, the media, and others.

**Trial Advocacy Training for Law Students**—In cooperation with ATLA's National College of Advocacy, this program provides free advocacy training.

**Richard S. Jacobson Award for Excellence in Teaching Trial Advocacy**—Each year an outstanding law professor receives this prestigious award.

**Pound Award for Excellence in Teaching Trial Advocacy as an Adjunct**—This award is made annually to an individual who balances a trial practice and teaching trial advocacy as an adjunct professor of law.

**Elaine Osborne Jacobson Scholarship for Women Working in Health Care Law**—This scholarship is given each year to a woman law student who is committed to a career in health care law.

**Roscoe Hogan Environmental Law Essay Contest**—The Pound Foundation administers this 26-year-old contest which annually honors a law student's writing ability in the area of environmental law.



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#### **1995 Preserving Access to Justice: The Effect on State Courts of the Proposed Long Range Plan for Federal Courts**

Report of the third Forum for State Court Judges. Discussions include the constitutionality of the federal courts' plan to shift caseloads to state courts without adequate funding support, as well as the impact on access to justice of the proposed plan. (01Q) \$35

#### **1993 Preserving the Independence of the Judiciary Report of second Forum for State Court Judges.**

Discussions include the impact on judicial independence of two contemporary issues: judicial selection processes and resources available to the judiciary. (09R) \$35

#### **1992 Protecting Individual Rights: The Role of State Constitutionalism**

Report of the first Forum for State Court Judges, in which more than 100 judges of the state supreme and intermediate appellate courts, lawyers, and academics discussed the renewal of state constitutionalism on the issues of privacy, search and seizure, and speech, among others. Also discussed was the role of the trial bar and academics in this renewal. (08R) \$35

#### **REPORTS OF ROUNDTABLES**

#### **Justice Denied: Underfunding of the Courts**

Report on the 1993 Roundtable, examining the issues surrounding the current funding crisis in American courts, including the role of the government and public perception of the justice system, and the effects of increased crime and drug reform efforts. Moderated by Chief Justice Rosemary Barkett of the Florida Supreme Court. (10R) \$20

#### **Safety of the Blood Supply**

Report on the Spring 1991 Roundtable, written by Robert E. Stein, a Washington, DC, attorney and an adjunct professor at Georgetown University Law Center. The report covers topics such as testing for the presence of HIV and litigation involving blood products and blood banks. (06R) \$20

#### **Injury Prevention in America**

Report on the 1990 Roundtables, written by Anne Grant, lawyer and former editor of *Everyday Law* and TRIAL magazines. Topics include: "Farm Safety in America," "Industrial Safety: Preventing Injuries in the Workplace," and "Industrial Diseases in America." (05R) \$20

**Health Care and the Law**

Report on the 1988 Roundtables, written by health policy specialist Michael E. Carbine. Topics include: "Hospitals and AIDS: The Legal Issues," "Medicine, Liability and the Law: Expanding the Dialogue," and "Developing Flexible Dispute Resolution Mechanisms for the Health Care Field." (37B) \$20

**Health Care and the Law II—Pound Fellows Forum**

Report on the 1988 Pound Fellows Forum, "Patients, Doctors, Lawyers and Juries," written by John Guinther, award-winning author of *The Jury in America*. The forum was held at the Association of Trial Lawyers Annual Convention in Kansas City and was moderated by Professor Arthur Miller of Harvard Law School. (35B) \$20

**Health Care and the Law III**

Report on the 1988–1989 Roundtables, written by health policy specialist Michael E. Carbine. Topics include: "Drugs, Medical Devices and Risk: Recommendations for an Ongoing Dialogue," "Health Care Providers and the New Questions of Life and Death," and "Medical Providers and the New Era of Assessment and Accountability." (02R) \$20

**Demystifying Punitive Damages in Products Liability Cases**

A Survey of a Quarter Century of Trial Verdicts. This landmark study, written by Professor Michael Rustad of Suffolk University Law School with a grant from The Pound Foundation, traces the pattern of punitive damage awards in U.S. products cases. It tracks all traceable punitive damage verdicts in products liability litigation for the past quarter century and provides empirical data on the relationship between amounts awarded and those actually received. (07R) \$22

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