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Essay

Legend vs. Pragmatism

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1. *Introduction*

In my third year of high school, Mr. Devlin taught us Cicero. We parsed and painfully translated Cicero's prosecution of Lucius Catilina for treason.[1] Cicero employed every rhetorical device: hyperbole, ad hominem attack, analogy, metaphor, appeal to reason and to the divine. No appeal had greater effect than the appeal to authority. [1]

In the legal debate over the use of scientific evidence we have seen again that nothing appeals like the appeal to authority. There is no more confident authority today than that of the scientific establishment, which has vanquished both gods and popes and claimed for itself the mantles of neutrality and infallibility.[2] [2]

The defense prefers the articulation of Justice Harry Blackmun in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*,[3] which draws upon the mystique of scientific certainty and objectivity:

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1 MARCUS TULLIUS CICERO, *Catilinum I-IV*, *in* CICERO (C. MacDonald trans., 1977).

2 In the condemnation of Galileo Galilei we saw the first great fissure in the alliance between faith and reason. *See generally* PIETRO REDONDI, GALILEO HERETIC (Raymond Rosenthal trans., 1987).

3 508 U.S. 579, 113 S. Ct. 2786 (1993).

> The scientific project is advanced by broad and wide-ranging consideration [sic] 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 . . . .[4] [3]

The conservative position espoused by defense theorists repeats the mantra that only ideas that constitute scientifically valid knowledge should be admitted into evidence. They have sought to downplay that knowledge as opinion, since the word opinion implies subjectivity.[5] They have encouraged an olympian view of science that Philip Kitcher, a philosopher of science and mathematics, named "Legend."[6] [4]

According to Legend, science proceeds by universally accepted canons of evaluation, which, when used to test novel or controversial ideas, bring a relentless advance toward the complete true story of the observable part of the world, with but an occasional mistake to their credit.[7] [5]

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4 *Id*. at 2798 (emphasis added).

5 "The give-and-take process of testing, review, criticism, and, ultimately, acceptance, is the scientific method, and it is how scientists distinguish unsupported propositions from valid scientific theories." Clifton T. Hutchinson & Danny S. Ashby, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: Redefining the Bases for Admissibility of Expert Scientific Testimony*, 15 CARDOZO L. REV. 1875, 1886-87 (1994) (footnotes omitted).

> Scientists evaluate their ideas against criteria that involve testability, *objectivity, impartiality*, and a belief in a deep and obvious connection between evidence and reason. . . .
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> . . . .
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> The key to rationality and consistency lies in the *process* by which science maintains its commitment to *objectivity*. . . . *Only after a theory has survived a period of . . . testing, review and refinement can it be used without significant questions*, and even then, it remains open to renewed doubt. . . .
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> . . . .
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> *The question of scientific validity, therefore, hinges on acceptance*, and the answer depends on the purpose for which a theory is advanced and on the extent to which it has been tested.

Bert Black, *A Unified Theory of Scientific Evidence*, 56 FORDHAM L. REV. 595, 622-23 (1988)(first, third, and fourth emphases added)(footnotes omitted).

6 PHILIP KITCHER, THE ADVANCEMENT OF SCIENCE: SCIENCE WITHOUT LEGEND, OBJECTIVITY WITHOUT ILLUSIONS 3 (1993); *see also* THOMAS S. KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (2d ed. 1970). For the most provocative critique of Legend see PAUL K. FEYERABEND, THREE DIALOGUES ON KNOWLEDGE (1991).

7 Sir Karl R. Popper wrote one of the more sublimely confident depictions of the advancement of scientific knowledge:

Legend is first cousin to the *Frye* "general acceptance" test.[8] Most scientists, especially the laboratory scientists in white coats whom most people picture when they think of science, operate on a day-to-day basis within a specialized professional consensus. They agree on protocols and procedures, and train the generation below them in those precepts. So if the views of a scientist do not fall within the professional consensus, they are not 'valid,' and therefore should not be admissible. This is the position that the American Association for the Advancement of Science and the National Academy of Sciences urged upon the *Daubert* court.[9] [6]

> Objective knowledge; for example, scientific knowledge . . . consists of conjectural theories, open problems, problem situations and arguments. All work in science is directed towards the growth of objective knowledge. We are workers who are adding to the growth of objective knowledge as masons work on a cathedral. Our work is fallible, like all human work. We constantly make mistakes, and there are objective standards of which we may fall short--standards of truth, of content, of validity, and other standards.

SIR KARL R. POPPER, OBJECTIVE KNOWLEDGE: AN EVOLUTIONARY APPROACH 121 (rev. ed. 1981).

8 Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923). The *Frye* court concluded that the admission of scientific evidence rests with a determination of the "general acceptance" of the technique within the relevant scientific community. *Id*.

9 In an amicus brief filed in support of Respondent, Bert Black argued that

> [a] new theory or explanation must generally survive a period of testing, review and refinement before achieving scientific acceptance. This process does not merely reflect the scientific method; it is the scientific method. . . . Courts should admit scientific evidence only if it reasonably conforms to scientific standards and is derived from methods that are generally accepted [by the scientific community] as valid and reliable.

Brief for the American Association for the Advancement of Science and the National Academy of Sciences as Amici Curiae in Support of Respondent at 13, 19, *Daubert*, 113 S. Ct. 2786 (No. 92-102). Black argues in his influential article on scientific evidence that "[w]idespread consensus and acceptance, therefore, is the central test that scientists use to decide the validity of theories and reasoning in any given context, which is a test that the law can adopt and use successfully." Black, *supra* note 5, at 601. Justice Blackmun cited to Black's argument and confirmed the need for judicial verification of scientific opinion evidence, comparing it to the "common law insistence upon the most reliable sources of information." *Daubert*, 113 S. Ct. at 2795 n.9 (citing FED. R. EVID. 602 advisory committee's note). Justice Blackmun, however, did not recommend any particular approach.

> A number of authorities have presented variations on the reliability approach, each with its own slightly different set of factors. See, *e.g.*, [United States v. Downing], 753 F.2d [1224,] 1238-39 [(1953)] (on which our discussion draws in part); 3 [Jack] Weinstein & [Margaret] Berger, [Weinstein's Evidence] ¶702[03], pp. 702-41 to 702-42 [(1995)] (on which the *Downing* court in turn partially relied); [Mark] McCormick, Scientific Evidence: Defining a New Approach to Admissibility, 67 Iowa L. Rev. 879, 911-912 (1982); and Symposium on Science and the Rules of Evidence, 99 F.R.D. 187, 231 (1983) (statements by Margaret Berger). To the extent that they focus on the reliability of evidence as ensured by the scientific validity of its underlying principles, all these versions may well have merit, although we express no opinion regarding any of their particular details.

Id. at 2797 n.12.

Of course other passages in Justice Blackmun's opinion emphasize flexibility and affirm the similarity of law and science in their devotion to free debate. Such affirmation is forthcoming so long as the conclusion is reached by some scientific method,[10] and it shall be admissible even though it is not a generally accepted hypothesis.[11] [7]

At issue, therefore, is the attitude that judges will take in practice toward scientific evidence, and here *Daubert* gives no unambiguous guidance. The actual practice of judges will reflect the ongoing debates in science and in public policy. There are conflicting views of science *and* of law, and they divide along familiar fault lines. [8]

2. *Contending Views of Science*

The first view of science, the pragmatic view, emphasizes science's flexibility, diversity, uncertainty and practicality.[12] Pragmatists recognize the need to make decisions based on the best available evidence and acknowledge the role of motive, of metaphor,[13] of tradition and of culture in the development of scientific theory.

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10 Justice Blackmun made the elusive distinction between methods and conclusions, valid techniques and novel explanations, accepted practice and junk science, and made common knowledge and expert opinion a permanent feature of the rhetorical landscape. Much ink has been wasted in this endeavor. The Newtonian proposition $f = ma$ (force equals mass times acceleration) could be described equally well as a theory, a conclusion, a technique, or a method of analysis of physical phenomena; or simply a common-knowledge conclusion based upon observation of the difference between being struck by a baseball rather than a train; or a revolutionary advance in physics; or an outmoded expression now that we "know" that $e = mc^2$.

11 Justice Blackmun wrote that

> [t]he inquiry envisioned by Rule 702 is, we emphasize, a flexible one. Its overarching subject is the scientific validity--and thus the evidentiary relevance and reliability--of the principles that underlie a proposed submission. The focus, of course, must be solely on principles and methodology, not on the conclusions that they generate.

Daubert, 113 S. Ct. at 2797 (footnote omitted).

12 *See* Mervyn Susser, *What is A Cause and How Do We Know One? A Grammar for Pragmatic Epidemiology*, 133 AM. J. EPIDEMIOLOGY 635 (1991); *see also* Mervyn Susser, *The Logic of Sir Karl Popper and the Practice of Epidemiology*, 124 AM. J. EPIDEMIOLOGY 711 (1986).

13 Gerald Holton has developed the concept of "themata" as a way to escape the problem of "the philosophy that views science as a suprahistorical and culturally transcendent method of investigation." GERALD HOLTON, THE SCIENTIFIC IMAGINATION: CASE STUDIES ix (1978). Holton argues that

> a basic feature of the work of many seminal scientists is their acceptance of only a small number of themata and that their debates frequently involve antithetical dyads or

Pragmatists do not fear imperfection and do not shrink from making judgments along the way.[14] [9]

The second view, Legend, expresses an overly objective and rigid view of science that leaves little room for interpretation and is impatient with imperfection. By an improperly placed emphasis on deductive reasoning, the disparagement of inference and the over-reliance on a mathematical model, Legend sets standards of proof that really cannot be met.[15] For Legend's advocates, science equals certain knowledge and uncertainty is only a sign of "bad" science. Legend's often paralytic skepticism prevents the reaching of reasonable practical judgments.[16] Applied to law, Legend leads to a rigid view of proofs and of evidentiary standards that hinders, rather than enhances, the quality of justice. [10]

> triplets of themata--for example, atomicity / continuum, simplicity / complexity, analysis / synthesis, constancy / evolution / catastrophic change. Such posits help to explain the formation of traditions or schools, and the course of controversies.

Id.

14 Stephen Jay Gould, the paleontologist and popular writer on science and evolution, remarks: "One cannot hope to do anything significant or original in science unless one accepts the inevitability of substantial error along the way." STEPHEN JAY GOULD, WONDERFUL LIFE: THE BURGESS SHALE AND THE NATURE OF HISTORY 196 (1989). The discovery that Cambrian life forms were far more disparate anatomically than those we know showed that "[l]ife is a copiously branching bush, continually pruned by the grim reaper of extinction, not a ladder of predictable progress." Id. at 35. Gould, speaking of the route to this shift in views, writes:

> The pathway had not been smooth and direct, clearly marked by the weight of evidence and logic of argument. Intellectual transformations never proceed so simply. The flow of interpretation had meandered and backtracked, mired itself for a time in a variety of abandoned hypotheses . . . but finally moved on to explosive disparity.

Id. at 172-73.

15 The most influential conservative philosopher of science was Sir Karl R. Popper. He saw science as essentially deductive in nature and held that theories are essentially unverifiable, although they may be disproved or "falsified" by experiment. Popper stated that Einstein possessed

> an attitude utterly different from the dogmatic attitude of Marx, Freud, Adler . . . . It was utterly different from the dogmatic attitude which constantly claimed to find `verifications' for its favourite theories. Thus, I arrived, by the end of 1919, at the conclusion that the scientific attitude was the critical attitude, which did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it.

SIR KARL R. POPPER, UNENDED QUEST: AN INTELLECTUAL AUTOBIOGRAPHY 38 (1974).

16 Mervyn Susser pointed out that "undue skepticism can be as dangerous to scientific progress as credulity. Statisticians and epidemiologists are properly professional skeptics. But we must be aware of the trained incapacity to believe in positive results. As the White Queen implied to Alice, one may have to practice believing." Mervyn Susser, *Judgment and Causal Inference: Criteria in Epidemiologic Studies*, *in* EPIDEMIOLOGY, HEALTH, & SOCIETY, SELECTED PAPERS 69, 75 (1987).

We see the effort to advance these rigid, impractical approaches in writings such as those of Peter Huber and his collaborator, David E. Bernstein.[17] They take multi-factorial, flexible approaches to proof, like that of the late British bio-statistician Sir Austin Bradford Hill, and turn them into ladders and checklists. [11]

The real battle, therefore, is waged over the question: What is science? To use science well, it must be demystified. Scientists reach their conclusions through the reasoned interpretation of conflicting and often uncertain data, not by the requirement of burdensome forms of proof, nor by the application of mechanical formulae, nor by *sine qua non* tests of sufficiency. [12]

In his autobiographical obituary, Albert Einstein attributed to Ernst Mach much of his early abandonment of Newtonian mechanics as the "secured basis of physics," saying:

> I see Mach's greatness in his incorruptible skepticism and independence; in my younger years, however, Mach's epistemiological position also influenced me very greatly, a position which today appears to me to be essentially untenable. For he did not place in the *correct light the essentially constructive and speculative nature of thought and more especially of scientific thought;* in consequence of which he condemned theory on precisely those points where its constructive-speculative character unconcealably comes to light . . . .[18] [13]

Einstein cited two principal tests for judging the validity of a physical theory and, in my view, they are equally applicable to other explanatory theories:

> The first point of view is concerned with the confirmation of the theoretical foundation by the available empirical facts. The second point of view is not concerned with the relation [of the theory] to the material of observation but with the premises of the theory itself, with what may briefly but vaguely be characterized as the "naturalness" or "logical simplicity" of the premises.[19] [14]

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17 *See* David E. Bernstein & Peter Huber, *Defense Perspective (on Daubert)*, 1 Shepard's Expert & Sci. Evid. Q. 59 (1993); *see also* David E. Bernstein, *The Admissibility of Scientific Evidence After Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 15 CARDOZO L. REV. 2139 (1994). *Cf.* George W. Conk, *Against the Odds: Proving Causation of Disease with Epidemiological Evidence*, 3 SHEPARD'S EXP. & SCI. EVID. Q. 103 (1995) (stating that scientific knowledge is culturally determined; multi-factorial causal models are best understood when set in a coherent narrative, *e.g.*, as 'The Story of Organism').

18 ALBERT EINSTEIN, PHILOSOPHER - SCIENTIST 21 (Paul A. Schilp ed., 3rd ed. 1970) (emphasis added).

19 *Id*. at 23.

The ready acknowledgment of the speculative, constructive nature of scientific knowledge, the soft characterization that "it must not contradict empirical facts,"[20] and the clarifying, simplifying nature of the successful explanation are values that are absent from Legend. They are also absent from the thinly concealed appeals to authority, rather than to reason, that characterize much of the current judicial urging on how to decide questions regarding the admissibility of scientific opinion evidence. [15]

The proponents of Legend in the courtroom use the uncertainty of scientific opinion as a billy club to exclude evidence. Proponents profess that we should wait for certainty; we should wait for the authorities to decide, and we should wait for the scientists to decide when the sufficiency of the evidence is enough. Legend's proponents forget that it is the job of the law to decide how much evidence is enough, and to decide the law's objectives. For example, in a product liability case, the defendant's objective is to avoid paying a monetary award. The plaintiff's objective is to receive compensation for the injury inflicted by another. The judicial system's job is not to seek neutrality, but to apply the law, and the law is that we seek to compensate those who have been injured as soon as we can reasonably identify the injury and the parties responsible for causing such injury. The question of how much scientific evidence is enough is determined by the time available to make a decision. As Sir Austin Bradford Hill stated,

> [A]ll scientific work is incomplete—whether it be observational or experimental. All scientific work is liable to be upset or modified by advancing knowledge. That does not confer upon us a freedom to ignore the knowledge that we already have, or to postpone the action that it appears to demand at a given time.
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> ....
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> ...What I do not believe . . . is that we can usefully lay down some hard-and-fast rules of evidence that *must* be obeyed before we accept cause and effect. None of my nine viewpoints can bring indisputable evidence for or against the cause-and-effect hypothesis and none can be required as a *sine qua non*. What they can do, with greater or less strength, is to help us to make up our minds on the fundamental question—*is there any other way of explaining the set of facts before us, or is there any other answer equally, or more, likely than cause and effect?*[21] [16]

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20 The "softness" is that Einstein did not insist on verification or failure to "falsify" the theory by empirical test, nor did he insist on support by empirical test. For Einstein it might have been enough that the hypothesis was not contradicted by material observation, especially if the theory had other merits, compared to the alternative hypotheses. *See* Holton, *supra* note 13, at 97-100.

21 Sir Austin Bradford Hill, *The Environment and Disease: Association or Causation?* 58 PROCEEDINGS OF THE ROYAL SOCIETY OF MEDICINE 295, 299-300 (1965) (discussing the bases upon

3. *Propositions in a Box*

Philip Kitcher calls the conception of scientific knowledge that he seeks to rebut the "propositions in a box" conception of the mind.[22] According to Legend, each mental statement or proposition is like a separate atom, of equal status, waiting to be weighed and measured by the scientist's logical mind. The informed scientist, via logic, peer review, experiment, and other devices tests his own hypotheses and those of others.[23] Because logic rules, because all propositions and all information are presumed to be available to, even known by, all other scientists in the field, the "good" eventually wins out over the "bad." The valid inevitably vanquishes the invalid. Why should not the law just be patient and wait for the scientists to announce that good has finally triumphed? [17]

It is this conception, this psychology of knowledge as propositions in a box, that underlies all the calls for "rigorous validity testing," for increased use of Rule 706 "neutral" specialists, for reports and recommendations to the courts by the National Academy of Sciences or other panels of experts, and for applying in the courtroom the rules that supposedly govern scientists in their daily practice.[24] This conception of how the mind works is completely at odds with what our everyday experience tells us about how people actually function. It is also sharply at odds with how the common law and the Federal Rules of Evidence teach us to look at human behavior and opinion formation.[25] [18]

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which science moves from observed associations to a conclusion of causation) (third emphasis added). The nine viewpoints under which a scientific observation should be viewed include: strength, consistency, specificity, temporality, biological gradient, plausibility, experiment, coherence, and analogy. *Id.* at 295-99.

22 Kitcher, *supra* note 6, at 61-65.

23 *Id.* at 219-21.

24 An excellent critique of the dangers of "neutral" experts appears in Ellen Relkin, *Some Implications of Daubert and Its Potential for Misuse: Misapplication to Environmental Tort Cases and Abuse of Rule 706(a) Court-Appointed Experts*, 15 CARDOZO L. REV. 2255 (1994).

25 As Holmes stated,

> [t]he life of the law has not been logic: it has been experience. The felt necessities of the time, the prevalent moral and political theories, intuitions of public policy, avowed or unconscious, even the prejudices which judges share with their fellow-men, have had a good deal more to do than the syllogism in determining the rules by which men should be governed.

Oliver Wendell Holmes, Jr., THE COMMON LAW 1 (1881).

One problem with Legend is that *interest* nowhere appears in its psychology of knowledge and opinion formation. Scientists, like other people, are attached to their ideas, to their opinions, to their beliefs, and consider them to be knowledge and truth because of habit, affection, inertia, material self-interest, and national, ethnic, institutional, generational or personal pride. In contrast, the common law's great strength lies in its *dependence upon interested advocacy* to discover the truth. That is why so-called neutral experts have been so little used, and why there is no funding for judicial inquisition. The whole fabric of the common law and our common experience as attorneys tells us that individual biases, preferences and affections determine what people believe and what they claim to know. It also tells us that possessing an interest in something does not invalidate the opinions developed. [19]

Scientists have personal goals that motivate them in their work. They have aspirations, and they seek approbation, revenge, advancement and security.[26] They have political views that motivate them, such as the defeat of Nazism in the case of the Manhattan Project scientists, and the defense of Germany from the advancing Allies in the case of Werner Heisenberg.[27] Legend speaks of such goals only as biases, as hindrances to the scientist's ability to determine the truth, but they can equally be viewed as a motivation to find the truth. Once we accept motivation, accept goals, accept the theory-ladenness of perception, we abandon the Legend of god's-eye neutrality and of the propositions-in-a-box view of knowledge. [20]

Harvard epidemiologist Richard Monson, without being pejorative, has called his field "political arithmetic,"[28] and states:

> The interpretation of the meaning of data must take into account all these considerations of data source, means of collection, and methods of analysis. This is largely a scientific process—one in which the judgment of one's [scientific] peers is most important. However, decisions as to the utility of the data . . . must be based on an

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26 *See* Kitcher, *supra* note 6, at 73.

27 Conversely, some have argued that Heisenberg's mistrusT OF NAZISM LED HIM, CONSCIOUSLY OR UNCONSCIOUSLY, TO SLOW THE PACE OF GERMAN NUCLEAR DEVELOPMENT. DAVID C. CASSIDY, UNCERTAINTY: THE LIFE AND SCIENCE OF WERNER HEISENBERG 424-25 (1992).

28 Sir William Petty, a contemporary of John Graunt, an English epidemiologist working in London, England during the 1500s, called Graunt's science "political arithmetic." Monson writes that "[t]his term is a good description of today's epidemiology. Much of the data manipulation that epidemiologists do requires a fourth grade education in arithmetic. However, the wisdom as to the validity of the data and the conservatism of interpretation requires persons with a keen political sense." RICHARD R. MONSON, OCCUPATIONAL EPIDEMIOLOGY 2 (2d ed. 1990).

> independent process that is largely political—one in which the data collector and indeed the scientific field must also be judged.[29]

Monson makes an essential point: the law governs science, not vice-versa. [21]

4. *Strength In Diversity*

The Legend of science requires uniformity of method of understanding because knowledge is presumed to be uniform, but why should uniformity occur in science when it does not occur in any other human sphere? People differ in how they form their scientific opinions, just as they differ in other endeavors. Some are slow to form an opinion and some are quick. Some quickly grasp the motivational factors in perception and opinion formation while others ignore them. Arithmetic is governed by politics, and people's politics inevitably vary. [22]

Should we not consider that in developing knowledge or opinion such diversity of style may enhance the search for truth, that the advancement of science is obstructed if we impose uniformity of method, that diversity in how people think, in how and what they call knowledge, is a strength, not a deviation? Once we accept these values, our attitude toward the question of how to gauge scientific evidence changes. We face squarely the questions of goals and objectives. We state clearly the law's objectives, for example, to compensate fully those who have been injured as soon as we can identify the injury and measure its extent. [23]

5. *Conservatives Shrunk the Debate*

Once we accept diversity in what constitutes knowledge as both a fact and a good, we will stop fretting about what is admissible, about what is junk, and about what is fraudulent. We will start asking: What is persuasive? What is rationally founded? What is simply personal preference, Legend or prejudice? We should ask, How much can we know and how soon can we know it? We can see now the damage done by the conservative domination of the debate that led to *Daubert*. The conservative triumph in the war of the slogan making it the “junk science” debate has shriveled the dialog. Conservatives have shrunk the question to How can we exclude bad science, the science that does not conform to Legend, non-consensus, non-uniform science? [24]

Only rarely acknowledged straightforwardly was the motivation driving the *Daubert* attack: the effort to protect the financial interests of the defendants in the pharmaceutical industry. The industry's opinion is reflected in this statement:

29 *Id.*

"Should the judicial system transfer property on a causal premise that is not supported by a majority of the scientific community? We believe that the answer should be no."[30] The fact that the pharmaceutical industry sought to protect its interests is not disqualifying. The method, however, by which it sought to advance its objectives should be assessed by whether it comports with the judicial system's objectives, that is, by whether it is good public policy. [25]

For such purposes then we can restate the above proposition: We believe that in order to encourage industry to invest in new products, injured persons should not be compensated until a majority of scientists interested in the question have reached agreement that the product does cause harm. We think that leaving uncompensated those whose cases are reached early is worth the price since it will avoid saddling manufacturers with high costs for harms that may later be determined not to have been caused by their product. Therefore we propose a rule of law that only when most scientists have agreed that a product causes harm can the opinion of a scientist to that effect be admitted into evidence and form the basis of a judgment of compensation. The preceding reformulation is a debatable public policy proposition, but it is an honest one, more honest than the debate that we experienced over the last few years about whether "junk science" was overrunning our courtrooms. [26]

We should be asking the following questions: How can we better inform juries of the questions before them? How can we better understand science so that we can present it better to the jury or judge? How do people learn? What makes a theory or conclusion persuasive? How do we explain to juries that debate among scientists is the norm, that disagreement is to be expected, not disparaged? Let us tell juries what "good" science looks like, how to choose among scientists whose opinions differ. We should not repeat the myths of Legend. We should emphasize the uncertainty and explanatory flexibility of real science, and we should clearly enunciate the law's objectives, molding the burden of proof explicitly to the public policies that we seek to advance. We must remember that law, not science, is the master, that science is a tool we use to advance the objectives of the law. [27]

6. *Conclusion*

In closing, the words of Mervyn Susser, a fine epidemiologist and writer on causation and the philosophy of science, are fitting:

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30 Paul S. Miller & Bert Rein, *The Unknowable Is, By Definition, Unprovable*, 133 N.J. L.J. 1309, 1326 (Apr. 5, 1993). Mr. Miller is Vice President and General Counsel of Pfizer, Inc., a pharmaceutical concern.

> Is there common ground between lawyers and [scientists] in the ways they reach conclusions about causes? One notable similarity is the dependence of both fields on subjective judgments.
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> . . . .
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> . . . In the end, a quality which lawyers should understand better than any—judiciousness—matters more than any.
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> Scientists use both deductive and inductive inference to sustain the momentum of a continuing process of research. The courts of law, and the courts of application, use inference to reach decisions about what action to take. Those decisions often cannot rest on certitudes, most especially when population risks are converted into individual risks. It is my firm belief, nonetheless, that practical decisions that draw sustenance from scientific inference will be better decisions than those that do not.[31] [28]

31 Mervyn Susser, *Rules of Inference in Epidemiology*, 6 REGULATORY TOXICOLOGY & PHARMACOLOGY 116, 127 (1986).