"WHAT'S REALLY GOING ON?" A STUDY OF LAWYER AND SCIENTIST INTER-DISCIPLINARY DISCOURSE

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I. THE TWO CULTURES OF LAW AND SCIENCE

In the modern world the influence of science is everywhere. "At every turn," as a leading commentator puts it, "we encounter new material indicators of [science's] progress: air bags and antilock brakes, electronic mail, fax machines and bank cards, heart transplants and laser surgery, genetic screening, \textit{in vitro} fertilization, and a burgeoning pharmacopoeia for treating mental and physical illness."\(^1\) "In just one generation the space program has expanded the physical frontiers of human experience, while discoveries in the biological sciences have revolutionized our ability to manipulate the basic processes of life so as to fight infertility, aging, hunger and disease."\(^2\) Moreover, individuals use the lessons of science, sometimes without realizing it, to make most of the ordinary and basic decisions of everyday life, such as where and in what to live, what to eat and wear, what household and consumer products to use, what medicines to take, what type of work to do, what recreational activities to engage in, and the like. Similarly, public agencies and large-scale private organizations use

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2. JASANOFF, \textit{supra} note 1, at 2.
science to make social policy decisions about the allocation of risk, the distribution of resources, and the structuring of economic incentives and constraints, to determine, among other things, how to create jobs, prevent environmental degradation, and provide cost-effective health care. Additionally, these agencies and organizations use science to decide what types of technological innovation and economic arrangements to encourage and nurture, and whom to blame and sometimes also hold liable for individual and collective harms. Science informs nearly all of the long and short term lifestyle and policy choices presented by the modern world. Our very understanding of that world is mediated through, if not shaped by, science. One can be ignorant of science, but one cannot escape it.

Science intersects with law at almost every turn as well. As Peter Schuck explains:

[T]he law of intellectual property, especially patents, takes its very content from science, both pure and applied. The law of evidence often demands scientific support for the use of certain techniques, such as DNA typing and epidemiology, and for the drawing of particular inferences from testimony, such as causation. Medical malpractice cases usually involve (indeed, they may require) testimony by scientific experts. Antitrust litigation relies heavily upon technical economic analysis of product markets. Employment discrimination often looks to statistical analyses of labor markets for evidence of bias [and so on, and so forth].3

In some ways, common law courts have always dealt with disruptions arising from scientific and technological advances of one kind or another.4 However, science and law have become

3. Peter H. Schuck, Multi-Culturalism Redux: Science, Law and Politics, 11 Yale L. & Policy Rev. 1, 3-4 n.9 (1993); see also LEON R. KASS, TOWARD A MORE NATURAL SCIENCE 2 (1985) (listing legal questions raised by new biomedical technologies, including "the legality and morality of abortion, the definition of clinical death, the legitimacy of research on fetuses, the morality of 'test-tube babies' and surrogate motherhood, the propriety of sperm banks, the right to refuse treatment, the rationale for psychosurgery, justice in the distribution of medical resources, the dangers and benefits of gene splicing, and the use and abuse of psychoactive drugs"); Jonas Robitscher, Can Professionals Talk With Each Other?, 64 J. Med. Ass'n Ga. 302, 304 (1975) (describing other issues in which law and science interrelate).

4. See JASANOFF, supra note 1, at 24 (describing how early in the
pervasively intertwined with the advent of the industrial and electronic revolutions in the nineteenth and twentieth centuries. It is now routine for social change, particularly the kind calling for legal regulation, to be augmented, if not brought about, by scientific and technological change. Science based choices and problems pervade the most difficult issues of social regulation facing legislatures and courts. Consequently, law and science are inevitable bedfellows, whether they want to be or not. Not working effectively with one another is no longer an option for either discipline, if it ever was.

Notwithstanding the need to coordinate their efforts, however, law and science, or more accurately, the practitioners of law and science, do not get along all that well with one another, and perhaps never really have. Part of the difficulty is explained by the fact that law and lawyers, mostly in the form of personal injury lawyers and products liability and malpractice litigation, threaten the economic livelihood of scientists (including doctors), as well as restrict their ability to control the terms and conditions of their work.

seventeenth century common law judges were already grappling with cases that pitted homeowners interests against those of new commercial activities, giving as examples a “hog-sty,” the odor from which “corrupted” the surrounding air, or a “dye-house,” the waste from which polluted a fish pond).

5. This is particularly true for the last thirty years, roughly the period in this country in which there has been a widespread perception of a medical malpractice crisis. This crisis (or perception) has been responsible for much of the overheated and virulent rhetoric on the incompatibility of law and science. See JASANOFF, supra note 1, at 31-36 (describing the medical malpractice crisis); Kenneth S. Abraham, Medical Malpractice Reform: A Preliminary Analysis, 36 MD. L. REV. 489, 490-92 (1977); Joan M. Gibson and Robert L. Schwartz, Physicians and Lawyers: Science, Art. and Conflict, 6 AM. J. L. & MED. 173, 173-74 (1980) (stating that “relations between physicians and lawyers... deteriorated into mutual hostility... [w]ith the advent in the early 1970s of what was widely perceived to be a medical malpractice insurance crisis”); Benjamin Naitone, Medicolegal Education and the Crisis in Interprofessional Relations, 8 AM. J. L. & MED. 293, 294-98 (1982) (describing “the crisis in interprofessional relations”).

6. See Erle E. Peacock, Doctors v. Lawyers, 55 NO. CAR. MED. J. 41, 42 (1994) (describing the “most obvious” reason for the “universal condemnation” of lawyers by physicians as “that lawyers hold a power over physicians that is not reciprocated”); Gibson and Schwartz, supra note 5, at 174-75 (describing defensiveness between doctors and lawyers as “reinforced by potentially competing economic interests of the two professions”); Daniel M. Fox, Why
Money and power are the oldest, simplest, and most common sources of friction between social groups, so no doubt part of the law-science conflict is explained in this way. But money and power, by themselves, seem inadequate to account for the level of antipathy expressed by scientists for lawyers and vice versa. Law’s threat to the income of scientists, while real, is not likely to move scientists as a group from one economic class to another, and the explanation based on a lack of control over work suffers from some of the same difficulties. No professional group has unilateral control over its work, nor could it hope to in a political and economic system as multi-faceted, interdependent, and complex as our own, even if law was not in the picture. It is not the lack of control per se that is troubling to scientists, so much as it is the ceding of control to those who are thought not to know what they are doing. The foundation of scientists’ dislike for lawyers starts with an objection to the way in which control is exercised and to the people exercising it, rather than an objection to the loss of control in and of itself. To understand this conflict fully, then, one must look deeper into the professional cultures of the two disciplines, to see what it is about each that sets the other off.

It is commonly understood that law and science are different enterprises, organized around different central values, animated by different incentives and constraints, constituted by different analytical and investigative methods and techniques, and driven by different biases and orientations. Perhaps because they are so

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*Doctors and Lawyers Don’t Get Along*, LEGAL TIMES, Aug. 30, 1993, at 25 (describing doctors’ objections to lawyers as based on “rising malpractice insurance premiums,” and the “mass of regulations that burden and . . . often demean” doctors and “interfere[] with [their] medical autonomy”).

7. Peter Schuck has described the differences between the two disciplines, including the extent to which such assertions of difference must be qualified, organizing his discussion around these themes. See Schuck, supra note 3, at 14-29; see also Markey, supra note 1, at 14 (describing different approaches taken by law and science towards the same factors). Leon Kass argues the stronger point that “[s]cience . . . is neither in spirit nor in manner friendly to the concerns of governance or the moral and civic education of human beings and citizens.” KASS, supra note 3, at 4. He continues:

Science fosters and encourages novelty; political society, governed by the rule of law, cannot do without stability. Science rejects all authority save the truth, and prefers skepticism to trust and submission when truth is unavailing; the political community requires trust in, submission to,
different, lawyers and scientists complain about one another in much the same way, with the arguments of each placing a heavy emphasis on the themes of vulgarity and obtuseness. Scientists bemoan law's lack of understanding of science, its inability to separate good science from bad, its demand that scientific findings be expressed in the language of certainty rather than probability, and the limited opportunity provided by legal procedures for keeping decisions provisional or contingent, subject to further testing and reconsideration in light of new information or understanding. They also object to the adversarial method of inquiry used by lawyers, with its perceived (by scientists) preference for "relative" rather than "absolute" truth, and for "strong[]" (i.e., rhetorically persuasive) rather than "right" answers. Additionally, scientists believe that legal concepts like "guilt, fault, intention, responsibility, liability, obligation, [and] duty," cannot be verified by appeals to scientific law. Karl Menninger expressed these objections in the language of an earlier

and even reverence for its ruling beliefs and practices. Science is universal and cosmopolitan; the political community is always particular and exclusive, resting on a distinction between who is in and who is out. The love of truth and the love of one's own are not always reconcilable.

Id. But see note 26 infra, for an argument that the differences between law and science can be greatly exaggerated.

8. See Schuck, supra note 3, at 25 n.78 (quoting from a scientist's letter to the author, "the notion of refusing to reconsider a determination that one knows is wrong would drive scientists even wilder than trotting out the concept of justice."); see also David Hyman, How Law Killed Ethics, 34 Pers. in BIOLOGY & MED. 134 passim (1990), for a clearly stated and vividly illustrated taxonomy of "scientific" objections to legal discourse (i.e., that it is too uncertain, equivocal and hedged, that it is too intrusive and hegemonic, that it is too ad hoc, emotional and particularistic, and that it is too adversarial and judgmental); Fox, supra note 6, at 25-26 (describing similar five-point taxonomy).

9. Gibson and Schwartz, supra note 5, at 176-77, 179.

10. Id.; see also Kass, supra note 3, at 4 (stating that "It is one thing to hold on trust as true that one should honor one's father and mother or that all men are created equal and endowed by their Creator with certain unalienable rights; it is another thing to have to prove it"). The best discussion of the relationship between the legal adversarial and scientific methods is David Luban, The Adversary System Excuse, in THE GOOD LAWYER: LAWYERS' ROLES AND LAWYERS' ETHICS 83, 94-96 (David Luban ed., 1983); see also Sidney J. Golman, Medicine and the Law, 12 PARAPLEGIA 237, 237-39 (1975) (describing the power of the adversary advocacy to manipulate emotion, bias, and prejudice to achieve lawyer goals).
generation of scientists when he wrote, "[t]he very word of justice irritates scientists. No surgeon expects to be asked if an operation for cancer is just or not. No doctor will be reproached on the grounds that the dose of penicillin he has prescribed is less or more than justice would stipulate." 11

11. KARL MENNINGER, THE CRIME OF PUNISHMENT 17 (1968); see also TIMOTHY FERRIS, THE WHOLE SHEBANG: A STATE-OF-THE-UNIVERSE(S) REPORT 300 (1997) ("philosophizing is about as popular among working scientists as is bird-watching among professional golfers"). Daniel Fox also has several choice lines for describing scientists' (mostly doctors') objections to law. See Fox, supra note 6, at 26. It is particularly ironic that law should be criticized at this time in its history for not understanding science, or for not building bridges between legal and scientific systems of thought. In the last two decades, interdisciplinary study and scholarship in American legal education have grown exponentially in importance. See, e.g., Symposium: Writing Across the Margins, 53 WASH. & L. REV. 943-1037 (1996) (discussing the question of "what can go wrong?" when legal scholars look at legal subjects from an inter-disciplinary perspective). Law schools have developed in ways that move beyond the view of law as an autonomous discipline, and scientists now routinely have appointments on law faculties. See Phillip Areeda, Always A Borrower: Law and Other Disciplines, DUKE L. J. 1029, 1038-39 (1988). Many law professors have dual degrees in law and science, science materials are routinely included in law school course books and studied in law school courses (e.g., courses in intellectual property, environmental law, health law, criminal law, and the like), and there are even courses and texts designed to teach lawyers how to use science in law. See, e.g., JOHN MONAHAN & LAURENS WALKER, SOCIAL SCIENCE IN LAW: CASES AND MATERIALS (2d ed. 1990). Mainstream legal journals routinely publish articles based on joint legal and scientific research and analysis, and some journals are devoted exclusively to the publication of interdisciplinary law and science scholarship. See David O. Brink, Book Review: Reviewing Theory and Practice (Ian Shapiro and Judith Wagner DeCew eds., 1995), 47 J. L. EDUC. 270, 272 (1997). In this, law is participating in an academy wide phenomenon. As Richard Posner notes:

It is no longer easy to distinguish a sociologist from an anthropologist, or a classicist from either, or a literary theorist from a philosopher, or even an economist from an evolutionary biologist. This fraying of lines is as advanced in the academic study of law as anywhere, with the result that the very distinctions among the different fields of interdisciplinary legal study, such as law and economics and law and literature, are blurred ....

RICHARD POSNER, OVERCOMING LAW 472 (1995). Whatever the effects of these changes in the academic legal world, however, and it is safe to say that they have been considerable, the movement for increased interdisciplinary understanding and collaboration between law and science has not yet had an optimal effect on the day to day behavior of legal practitioners and judges. See id. at 98-99 (noting and bemoaning this fact in responding to Judge Harry Edwards' criticism of legal scholarship for being impractical).
Lawyers complain just as assiduously about scientists’ failure to understand the fundamental purposes and methods of law.\textsuperscript{12} The familiar disjunction, that law pursues justice and science truth,\textsuperscript{13} while accurate to a degree, conceals a more complex reality which is easily misunderstood from each direction. As the sociology of science has taught, “scientific facts are not immanent [sic] in an objective reality waiting to be discovered by any scientists who look in the right place. Instead, they are constructed and validated... through a... process... [that is] shaped by scientific paradigms,... [and these paradigms, in turn] hold sway for reasons that may have less to do with their intrinsic merit than with their support of existing social structures, including the scientific establishment.”\textsuperscript{14} Moreover, the search for scientific truth “is not guided solely by [scientists’] autonomous, spontaneous curiosity. [It] is [also] constrained and channeled by resources available for research, which in turn reflect the priorities of politicians, corporations, foundations, and other sources of funding for science.”\textsuperscript{15} Much of the time, therefore, in its data, methods,
findings, and even in the extent of its authoritativeness, science is as contextual, contingent, and controversial as law.

By the same token, lawyers claim, justice is not purely subjective. While it is true that notions of justice vary with time and place, the concept also contains an objective component that remains constant over the same categories. This is because “[l]aw’s legitimacy, at least in the long run, rests in part on its ability to generate outcomes that . . . comport [more or less,] with the common morality and common sense of the lay community” being regulated. However, this common morality and common sense also demand, among other things, that law not deviate unacceptably from scientific truth, at the peril of being thought illegitimate if it does. While it may be difficult to determine what degree of deviation a society will accept as legitimate at any given moment, this consistency with common sense requirement, hard-wired into the concept of justice, causes scientific and legal truth to merge in a complicated, but real, way. Legal truth is not identical to scientific truth, but it also is not wholly at odds with it. Moreover, legal truth cannot ignore or fly in the face of scientific truth without losing its claim to legitimacy. Lawyers argue that scientists do not understand the nature or extent of this interrelationship, when they criticize law for its failure to take scientific truth seriously.

Regardless of the fact that justice and truth, in some ways fundamentally different ends, law and science also differ principally in the time frames in which they work, and the language each uses to express its results. Law is usually in more of a hurry to decide than science. Judges must resolve cases that come before them on the basis of the best evidence available at the time, even if that evidence is not wholly adequate by scientific standards. They do not have the option of waiting until a professional scientific

driven” as “charmingly naive.” R.C. Lewontin, Survival of the Nicest, N.Y. Rev. of Books, Oct. 22, 1998, at 59. Using examples from evolutionary biology, he explains, “there is no simple and direct ‘truth’ about how to understand the history of life on earth. [While] the descriptive facts of evolution are not at issue, . . . when we move from evolution as narrative to evolution as universal history the predispositions of ideology come to dominate science.” Id.

consensus emerges. In making these decisions, judges frequently will be forced to choose among competing scientific theories even when they are not competent (in scientific terms) to do so, and when scientists are not ready to do so for them.\textsuperscript{17} If this was not enough, judges also must justify their decisions in non-technical and relatively simple language, so that they may be understood and applied by people generally, acting without professional help and with limited resources. This necessarily causes law to draw lines "and create[] categories that force many legal decisions into a binary mode," where the difference between being in and outside of the category means everything.\textsuperscript{18}

"[T]his tendency toward categorical reductionism," as Professor Schuck explains, also "reflects [law's] commitment to the jury as a

\textsuperscript{17}Sometimes, in Edward Lazarus's felicitous phrase, courts must "build[d] the right house on the wrong land and put it up too quickly." \textit{Edward Lazarus, Closed Chambers} 371 (1998)(explaining how "locating the right to abortion in the due process clause's protection of privacy . . . skewed the terms of the debate away from the strongest (and textually supported) pro-choice legal arguments as well as from what was really at stake socially and morally for women." This could not be helped, Lazarus continues, because "[t]he argument from equality was not yet developed" at the time \textit{Roe v. Wade} had to be decided.); \textit{see also} Ruth B. Ginsburg, \textit{Some Thought on Autonomy and Equality in Relation to Roe v. Wade}, 63 No. CAR. L. REV. 376 (1985)(articulating an equality based rationale for \textit{Roe v. Wade}). The sixteen year litigation against Dow Corning Corporation over the safety of silicone breast implants, which may have come to an end with the Company's agreement to pay the plaintiffs $3.2 billion in settlement of their claims, is a good example of the different timetables within which law and science work, often at cross purposes. \textit{See} Gina Kolata, \textit{Deal Without Resolution}, N.Y. TIMES, July 11, 1998, at A1; \textit{see also} MARCIA ANGELI, SCIENCE ON TRIAL: THE CLASH OF MEDICAL EVIDENCE AND THE LAW IN THE BREAST IMPLANT CASE (1997).

\textsuperscript{18}Schuck, \textit{supra} note 3, at 28. Professor Jasanoff explains further why "'[t]here is no way for the law to access a domain of facts untouched by values or social interests." \textit{Jasanoff, supra} note 1, at 207. As she explains, "'[C]ourts . . . conduct the bulk of their scientific inquiries 'at the frontiers of scientific knowledge,' where claims are uncertain, contested, and fluid, rather than against a backdrop of largely settled 'mainstream' knowledge. Instructing courts to take their cues from . . . 'good' scientific methodology . . . as opposed to 'bad' scientific practices . . . can therefore be deeply misleading. Other forms of guidance are needed, more realistically attuned to the indeterminacy of scientific knowledge in the contexts of litigation." \textit{Id.} at 210; \textit{Areeda, supra} note 11, at 1036-37 (stating that "few of the theoretical and empirical inquiries deemed interesting by other disciplines are directly relevant to the law. One has to tease the relevant concepts and information out of them in order to answer the question posed by the law").
lay decision-maker." Based on populist premises, the legal culture in this country "enshrines the cognitive and behavioral standards of ordinary people . . . rather than expert standards, as the test of legally approved conduct." The jury affirms these populist values by "serving as the voice of common morality, a bulwark of common liberty, and a decentralized organ of popular government . . . reflect[ing] the legal culture's singular compromise between professional autonomy . . . and popular sovereignty . . . ." Legal opinions are never likely to be as nuanced as the realities they seek to shape then, given the need to decide cases quickly, at the time they are presented for decision, whether the science is in place to support such decisions or not, and the need to justify such decisions in non-technical terms. Scientists' criticism of law fails to take this simple reality into account and strike lawyers as uninformed, and thus, irrelevant.

It should be clear that the issues on which law and science divide are not easy ones to resolve. They embody timeless and in some ways intractable problems, and make up what has come to be known as the "two cultures" debate, perhaps the most well known intellectual fracas in western thought. To understand this debate

20. Id.
21. Id.
23. In contemporary parlance, the debate between the humanities and science is alternately referred to as "the culture wars" or "science wars," depending upon whether one is a scientist or a humanist. Culture wars seems right, since the fight is over the content of the culture. Not everyone agrees that there is a basis for such a war. For example, Professor Jasenoff argues against what she takes to be the overly simple characterization of law and science as fundamentally different enterprises, contending instead, that "law and science are in fact mutually constitutive, . . . jointly produc[ing] our social and scientific knowledge . . . ." JASANOFF, supra note 1, at 8. Moreover, she argues, "[a]s formal systems of inquiry, [they] have several important features in common." Id. For example: Each tradition claims an authoritative capacity to sift evidence and derive rational and persuasive conclusions from it. The reliability of observers (or witnesses) and the credibility of their observations are of critical concern to both legal and scientific decisionmaking. Unlike organized religion, neither science nor law owes allegiance to a single dogmatic authority. In both fields, rules governing the assessment of facts occasionally undergo massive shifts—in science through the work
we need to go back a little in time. At the beginning of the
seventeenth century, “the age of Harvey, Boyle, Newton, and a host
of other thinkers who profoundly influenced the development of
modern science,” science rejected the idea of authority based on the
wisdom of the “Ancients.” Making a semi-clean break with the
analytical method grounded in deductive logic and derived from
Aristotle, science (in the person of propagandists such as Francis
Bacon) insisted instead on an inductive approach to knowledge,
based on the patient accumulation of verifiable facts. Such

of paradigm-transforming pioneers and in law (ordinarily but not
always) through the actions of legislatures. [She might have added
common law courts.] Normal progress within each discipline occurs
through a decentralized, silent revolution brought about by individuals
making decisions at the frontiers of established doctrine in accordance
with their personal understandings of the existing tradition.

Id. at 8-9. Peter Schuck accepts a limited version of this argument, finding
commonalities between the cultures of law and science, and the differences
between the two to be not as pronounced as the differences between science and
politics. See Schuck, supra note 3, at 4, 14-29; Richard M. Cooper, Science and
(analogizing cross-examination to peer review); Gibson and Schwartz, supra note
5, at 180 (describing how “the physician, like the lawyer, is involved
in prescriptive and value-laden acts of decision-making” in deciding what to do for
particular patients).

24. CULTURES IN CONFLICT: PERSPECTIVES ON THE SNOW-LEAVIS
CONTROVERSY 56 (David K. Cornelius & Edwin St. Vincent eds., 1964); see also
JACOB BRONOWSKI, THE COMMON SENSE OF SCIENCE 15-20 (1978) (describing the
founding of the British Royal Society of Science in the 1660s, and the scientific
revolution of the seventeenth century).

25. See BRONOWSKI, supra note 24, at 26-40 (describing how the 17th century
reformers, particularly Newton, combined ancient and modern technique more
than they replaced the former with the latter, and how that is the distinctive beauty
of scientific advancement).

26. See CULTURES IN CONFLICT, supra note 24, at 56; KASS, supra note 3, at 2
(assuming that “Francis Bacon and Rene Descartes” are the “great founders” of
“modern science”). Stephen Jay Gould puts the matter nicely when he describes
science as “the art of the empirically soluble,” and a good scientist as “a person
with the horse sense to discern the largest answerable question—and to shun
useless issues that sound bigger,” in his comment on Edwards v. Aguillard, 482
U.S. 578 (1987), the Louisiana “creation science” case, pointing out in the process
how Justice Scalia does not understand evolutionary biology. Stephen Jay Gould,
Justice Scalia’s Misunderstanding, 5 CONST. COMMENTARY 1, 4, 6-7 (1988); see
also FERRIS, supra note 11 at 205 (stating that “Unlike philosophy, physics seldom
proceeds by hunting down sweeping answers to big questions. Most often it
involves poking around at specific issues which seem to promise some intellectual
“empirical” study was superior to “literary” (or “philosophic”) alternatives, Bacon and others claimed. So that, when the investigation of factual matters is involved, scientific methods should be used instead of these alternatives. With the issue so joined, science found, and finds, it difficult to mesh its way of thinking with a broad range of other systems of disciplinary (including most famously, but not limited to, religious) thought.

or practical reward . . . .”). Ferris’s description of science and scientific method would be hard to improve on for clarity and eloquence:

Science is not a static body of dogma, to stray from which is to risk having one’s epaulets stripped off in a ceremony of banishment from the scientific community. It is a self-correcting system of inquiry, in which errors—of which there are, of course, plenty—are sooner or later detected by experiment or by more careful analysis. Science is also a “bottom-up” system, in which grand pronouncements are arrived at not in an overarching, sui generis fashion but by building up inferences from many small cases. As a result, science, while it can be exasperatingly detailed, is also pliant. Scientific findings, even the most imposing ones, customarily stumble into the world fraught with blunders that have to be worked out before they really begin to fly. They lack the satisfying, thunderclap certitude of religious and pseudoscientific dicta that admit to no error. But they are alive, and the withering of one branch of a theory does not necessarily mean that the theory as a whole is doomed.

Id. at 13-14. Leon Kass describes scientific method in this way:
The pursuit of knowledge in our time differs radically from the Socratic pursuit of wisdom. When we say ‘knowledge,’ we mean scientific knowledge. The paradigm of our knowing, aped by the other sciences, is mathematical physics, a science that took its beginnings in the seventeenth century, in explicit opposition to ordinary experience and to speculative philosophy. Most radically, it redefined what it means to know something, in terms of the standards of certainty and clarity possessed by symbolic mathematics and through the rigorous application of a universal method. Explicitly antiphilosophical in its spirit, it rejects as unworthy of its attention all questions that it cannot treat methodically and ‘objectively,’ and confines its attention to those problems that permit a scientific approach and solution . . . . [In science] [o]pinions about good and bad, justice and injustice, virtue and vice have no cognitive status and are not subject to rational inquiry . . . .

KASS, supra note 3; see also NEWTON, supra note 13, at 9-22, 119-23 (describing the scientific method). For a “rhetoric of science” perspective on Bacon’s new vision of science and the role of the Royal Society in propagating it, see ALAN G. GROSS, THE RHETORIC OF SCIENCE 164-72 (1996). For an argument that science does not involve being checked by the “unrelenting angel of reality,” and characterizing such a view as “charmingly naïve,” using evolutionary biology as an example, see Lewontin, supra note 15, at 59-63.

27. See, e.g., CULTURES IN CONFLICT, supra note 24, at 56.
There have been many noteworthy moments in this now three hundred-year-old debate. A famous example is the exchange between Matthew Arnold and Thomas Huxley at the end of the nineteenth century over the relative merits of science versus humanities instruction in the British Public School curriculum highlights this debate.\textsuperscript{28} And many celebrated thinkers and writers have taken part, from William Wordsworth, Thomas Love Peacock, John Ruskin, John Ciardi, Maxwell Anderson, Archibald MacLeish, George Orwell, Erich Fromm, E.M. Forster, and D.H. Lawrence, and others, on the side of literature or philosophy, to Stuart Chase, John Baker, Bertrand Russell, Aldous Huxley, H.G. Wells, and Jacob Bronowski, among others, on the side of science. But perhaps the most famous moment, and the one which gives the debate its name, is the “Two Cultures” exchange between C.P. Snow and F.R. Leavis at Cambridge University in the early 1960s.\textsuperscript{29} Snow led off with his 1959 Rede Lecture, “The Two Cultures and the Scientific Revolution,”\textsuperscript{30} and Leavis responded with his 1962 Richmond Lecture, “Two Cultures? The Significance of C.P. Snow.”\textsuperscript{31} The lectures provoked a set of spirited responses, mostly in literary journals and magazines, from partisans on each side, resulting in a sort of intellectual food fight.\textsuperscript{32} Today, the lectures and their responses read a little like quaint period pieces, peculiar in the extent to which they use adjectives rather than nouns and verbs to carry the brunt of their arguments; yet in the early 1960s they created a major stir in intellectual and academic circles on both sides of the Atlantic. They also illustrate, however, perhaps better than anything else, the difficulties involved in reconciling scientific and non-scientific ways of looking at the world.


\textsuperscript{29} See generally Snow, supra note 22; F.R. Leavis, Two Cultures? The Significance of C.P. Snow (1962).

\textsuperscript{30} See generally Snow, supra note 22.

\textsuperscript{31} See generally Leavis, supra note 29.

\textsuperscript{32} See, e.g., Martin Green, A Literary Defense of the “Two Cultures,” 24 Kenyon Rev. 731 (1962); Lionel Trilling, Science, Literature & Culture: A Comment on the Leavis-Snow Controversy, 33 Commentary 461 (1962).
In substance, if not in tone, much of what Snow and Leavis had to say, directly or by implication, about cold war politics, the relative merits of socialism versus democracy (or of living in Russia versus living in the United States), the place and prestige of science in British culture and British education, High Table conversation at Cambridge, the extent to which literary intellectuals are “natural luddites” in matters of science but not vice versa, the possibility of effectuating the Hegelian dream of reason taking over the management of the world, and the like, even though related to points in the present discussion, may safely be put to one side. So too with the lectures’ boisterous ad hominem style. Even by today’s standards, Snow and Leavis appeared to really go at one another, often looking a little like two pigs wrestling in the mud, with each getting dirty, but each also seeming to enjoy it (perhaps Leavis a little more than Snow). Anyone worried about the tone of contemporary academic discourse has only to read the “Two Cultures” debate to feel confident that development in the field of academic manners has been linear and progressive.

Snow argued that the thinking world was divided into two camps, the scientific and the literary (or what today might be called philosophical), each of which was grounded in a different set of values and beliefs, organized knowledge in a different way, and proceeded by means of different methods of investigation and analysis. He claimed that the inhabitants of these two camps lived in what amounted to different “cultures,” and that between the two cultures there existed “a gulf of mutual incomprehension—sometimes... hostility and dislike, but most of all a lack of understanding.” Culture, for Snow, described a set of “common attitudes, common standards and patterns of behavior, [and]

33. See generally SNOW, supra note 22; LEAVIS, supra note 29.
34. The popular view is that Leavis was the nasty one, jealous of Snow and his social success in the Oxbridge environment, particularly in relation to Leavis’ comparative lack of such success, see IAN MACKILLOP, F.R. LEAVIS: A LIFE IN CRITICISM (1997), but Snow takes a lot of gratuitous cheap shots at literary types in the Rede lecture, before Leavis responds, or even decides that he is going to respond.
35. See generally SNOW, supra note 22; LEAVIS, supra note 29.
36. See SNOW, supra note 22, at 4.
37. Id. at 4.
common approaches and assumptions," shared by members of a social group, which cut "surprisingly wide and deep," across "other mental patterns, such as those of religion or politics or class." 38 As he states in a frequently repeated phrase, "[w]ithout thinking about it, [members of a culture] respond alike. That is what a culture means." 39 One could quibble with this definition; for example, Matthew Arnold thought that culture "was the best that has been said and thought in the world," 40 and Martin Green claimed that the term culture had "fifty meanings." 41 But Snow's definition seems to have identified something true and timeless in the concept.

Scientific culture, for Snow and kindred spirits, 42 was marked mostly by an optimism about the possibility of improving the human social condition. 43 The middle decades of the twentieth century, when Snow wrote, were a particularly productive period for science, so much so that it seemed to many at the time that there were no limits on what science could be made to do. Such a view

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38. Id. at 10. He thought, for example, that Russian and American scientists could join together to prevent their respective countries from taking the cold war to a destructive conclusion. See SNOW, supra note 22, at 39-42. He believed that scientists would feel more allegiance to the culture of science than to their respective nation states. See id.

39. Id. at 11.

40. ARNOLD, supra note 28, at 322.

41. See Green, supra note 32, at 735.

42. See Bertrand Russell, Science as an Element in Culture — II, 1 NEW STATESMAN 234, 234 (1913).

43. See SNOW, supra note 22, at 6-8. This is not to say that Snow thought that all scientists were optimistic. Some, Malthus for example, would be difficult to describe in upbeat terms; however just that he thought science as a discipline was that way, and with good reason. Science had made numerous and obvious improvements in the human condition, from eliminating disease to relieving many of the most onerous burdens of work, and while these improvements often brought their own new sets of difficulties with them, Snow chose to focus on the harms that had been eliminated rather than the harms that had been introduced.

"Overly optimistic" was Snow's way of describing traditional intellectuals' criticism of the scientific culture, but this may itself be an optimistic way of paraphrasing the criticism. See id. at 6-8. Traditionalists probably would have used arrogance rather than optimism to characterize their objections to scientists in many instances. Snow's paraphrase has a self-serving quality about it that indicates he did not understand fully what his critics were saying, or the intensity of their feelings. This is a telling objection for Snow, since he claimed to be part of both the scientific and literary cultures.
was unduly upbeat as subsequent events have shown, but at the
time it was a widely shared view. While acknowledging that the
individual human condition was lonely and tragic ("you live 60 to
70 years, and then you die alone"), Snow felt that there was a
"moral trap" in this insight, and that scientists as a group were less
likely than non-scientists to fall into the trap. In Snow’s words:

[Scientists] see no reason why, just because the individual
condition is tragic, so must the social condition be. Each of us
is solitary: each of us dies alone: all right, that’s a fate against
which we can’t struggle—but there is plenty in our condition
which is not fate, and against which we are less than human
unless we do struggle.

Rather than sit back, complacently resigned to humanity’s tragic
condition, “scientists . . . are inclined to be impatient to see if
something can be done: and inclined to think that it can be done,
until it’s proved otherwise.”

Snow acknowledged that this optimism sometimes led scientists
to “regard the other culture’s social attitudes as [too quiescent, and
thus morally] contemptible,” but he criticized this view as “too
facile.” The fact that he reported the view at all, however,
subjecting it to only a mild rebuke, suggests, as Lionel Trilling has
noted, that he thought “[the view was] essentially . . . right.”

Snow admitted that in individual cases, literary, or what he also
sometimes called “traditional,” intellectuals could be as progressive
and optimistic as anyone, but scientists as a group were this way by
inclination and training. They had, as he put it, “the future in
their bones, . . . [whereas] the traditional culture responds by
wishing that the future did not exist.”

Here too, Snow may have been overly influenced by contingent external events. Many
literary intellectuals, to his mind, had been unduly sympathetic to,
or at least insufficiently critical of, the rise of fascism and national

44. Snow, supra note 22, at 7.
45. See id.
46. Id.
47. Id.
48. Id. at 8.
49. Trilling, supra note 32, at 465.
50. See Snow, supra note 22, at 5-6.
51. Id. at 11-12.
socialism in the first half of the twentieth century. As he put it, "nine out of ten of those who have dominated literary sensibility in our time [were] not only politically silly, but politically wicked. . . . [T]he influence of all they represent brought Auschwitz that much nearer."^53

The other distinctive characteristic of the scientific culture, identified by Snow, but described with more eloquence by Bertrand Russell earlier in the century, has to do with the special properties of the "scientific habit of mind."^54 "The kernel of the scientific outlook," as Russell described it, is a "refusal to regard [one's] own desires, tastes, and interests as affording a key to the understanding of the world."^55 In Russell's words:

The scientific attitude of mind involves a sweeping away of all other desires in the interest of the desire to know—it involves suppression of hopes and fears, loves and hates, and the whole subjective emotional life, until [one] becomes subdued to the material, able to see it frankly, without preconceptions, without bias, without any wish except to see it as it is, and without any belief that what it is must be determined by some relation, positive or negative to what [one] should like it to be, or what [one] can easily imagine it to be.^56

Lest one think Russell decidedly unmodern, or a little thickheaded about the role of the observer in constructing the content of scientific observation, it should be added that he also wrote:

Human beings cannot . . . wholly transcend human nature; something subjective, if only the interest that determines the direction of our attention, must remain in all our thought. But science comes nearer to objectivity than any other human pursuit, and gives us, therefore, the closest contact and the most intimate relation with the outer world that it is possible to

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52. See id. at 8.
53. Id.
55. See Russell, Science as an Element in Culture— II, supra note 42, at 235.
56. Id.
As a consequence, "science . . . represents, though as yet only in a nascent condition, a higher stage of evolution than any pre-scientific thought or imagination, and, like every approach to self-transcendence, it brings with it a rich reward in increase of scope and breadth and comprehension." 58

There is a good deal of self-serving, chest-thumping, and tendentiousness in this way of putting things, of course, and few would use Russell’s words verbatim today. Scientists understand the influence of context, circumstance, ideological commitment, self-interest, and the like (including pettiness, venality, jealousy, arrogance, spite), 59 in shaping the content of scientific discovery better now than they did at the time Russell wrote. This is due, in major part, to the work of historians, philosophers, sociologists, and rhetoricians of science. In a post-Kuhnian world we are all social constructivists to some extent. 60 Yet, Russell’s view is not

57. Id. at 236.

58. Id.; see also FERRIS, supra note 11, at 205 (describing how Herman Helmholtz’s prediction that humans would gain “intellectual mastery over nature” has “pretty much come true, and in a remarkably brief period of time. Physicists today can accurately predict the outcome of every fundamental (meaning simple) process in the known universe . . .”).

59. For discussions, see generally JAMES WATSON, THE DOUBLE HELIX (1968); ROBERT K. MERTON, THE SOCIOLOGY OF SCIENCE 286-324 (1973); DAVID HULL, SCIENCE AS A PROCESS: AN EVOLUTIONARY ACCOUNT OF THE SOCIAL AND CONCEPTUAL DEVELOPMENT OF SCIENCE (1988); GROSS, supra note 26, at 54-62; NEWTON, supra note 13, at 221.

60. See, e.g., PETER GALISON AND DAVID J. STUMP, eds., THE DISUNITY OF SCIENCE (1996); GROSS, supra note 26; JASANOFF, supra note 1; DAVID OLDROYD, THE ARCH OF KNOWLEDGE: AN INTRODUCTORY STUDY OF THE HISTORY OF THE PHILOSOPHY AND METHODOLOGY OF SCIENCE (1986); but see Steven Weinberg, The Revolution that Didn’t Happen, N.Y. REV. BOOKS, Oct. 8, 1998, at 48, 50 (criticizing Thomas Kuhn’s claim that paradigms governing successive periods of normal science are incommensurable, and arguing instead for a view of scientific discovery as cumulative, moving toward the uncovering of permanent truths “out there,” giving examples from the physical sciences); see also Letter from Richard C. Strohman, Emeritus Professor, University of California at Berkeley, to New York Review of Books (undated, responding to The Revolution that Didn’t Happen echoing Weinberg’s claim that a paradigm shift does not involve the replacement of an old paradigm with a new one, but that instead, “the normal outcome has been the coexistence of the old with the new,” giving examples from the biological and behavioral sciences) (on file with the author).

In the process of criticizing those who see science as merely invented or
all that different from Snow's, and many noteworthy scientists in the present day espouse a version of the same view. While contemporary beliefs may be more nuanced and variegated than Russell's, it is fair to say that many scientists believe that the intellectual disciplines are organized in a hierarchy, with science located at the top. They view scientific understanding and determined by ideological forces. Bernard Williams has described the social constructivist view as carefully as anyone. He states:

the [view] is ... that science is a complex social activity, and the fact that some branch of science at a given time settles on certain theories or models rather than others is not an outcome straightforwardly determined by perceptions of the world, but rather by scientists' habits and practices, including their ways of selecting and interpreting observations. This is not an abstract philosophical point; it is the conclusion of detailed historical studies.


61. "The scientific edifice of the physical world ... [is] in its intellectual depth, complexity, and articulation, the most beautiful and the most wonderful collective work of the mind of man." Snow, supra note 22, at 15.

62. See, e.g., Ferris, supra note 11, at 204 (stating that "physics ... stands as a living monument to what many esteem as the paramount intellectual achievement of the twentieth century"); Newton, supra note 13, at 222 (expressing the "hope that the present cynical and nihilistic state of our culture is a transitional one, and that in the future it will be replaced by a more positive outlook, with the values of science in a central position"); John P. Wiley, Jr., *Phenomena, Comments & Notes: Two Cultures — Never the Twain Shall Meet*, SMITHSONIAN MAG., Oct. 1997, at 22 (explaining that "the most original, most exciting work of the mind these days is being done by scientists, and if anyone should be included in the ranks of the intellectuals, they should"); Edward O. Wilson, *The Coherence of Knowledge*, HARV. MAG., July-Aug. 1998, at 58 (describing how the "borderland disciplines" of cognitive neuroscience, behavioral genetics, evolutionary biology, environmental sciences, and the like have begun to understand human nature objectively by "weaving a skein of cause-and-effect explanations from brain to mind to culture," and in the process "approaching the grail of scholarship, and fulfilling the dreams of the enlightenment").


64. Many also believe that there is a hierarchy within science. See, e.g., Ferris, supra note 11, at 204 (describing physics as having a pre-eminent role in science, and as the "quintessential science"); Newton, supra note 13, at 54 (stating that "physics is regarded as the most advanced of the sciences"). For
method as a purer and more advanced form of understanding and method than philosophically based equivalents, even if they also believe that one must be careful about where, and with whom, one expresses this view.\footnote{65} While ravaged somewhat by the passage of time, and the work of the meta-critics of science, the “science as first among equals” view is a major source of the tension between science and law in the present day.\footnote{66}

Scientists’ confidence in their intellectual primacy of place can be traced in principal part to the belief that they are the only academics who work in the pursuit of objective truth. They are philosophical realists, whether self-consciously so or not,\footnote{67} whose data consists of an external, physical world independent of human sense perception and language based description. This physical world has the singular advantage of staying put from interpretive moment to interpretive moment, and from interpretive era to interpretive era, so that all scientists in a given field, whenever and wherever they work, in a real but not literal sense, participate in the same intellectual investigation. In this view, for example, Einstein, Hubble, Newton, Kepler, Galileo, Copernicus, Ptolemy, and others all looked out onto the same universe(s) and worked on the same problems, improving upon one another’s work and causing some this is because the natural world is “seen as composed of objects belonging to one of a sequence of levels,” existing in a kind of “hierarchical ontology.” John Dupré, Metaphysical Disorder and Scientific Disunity, in THE DISUNITY OF SCIENCE, supra note 60, at 103; accord Newton, supra note 13, at 61-64 (stating that “the relation between general and local theories implies the existence of a certain hierarchical relationship between scientific disciplines”).

\footnote{65. See Peter Galison, Introduction: The Context of Disunity, in THE DISUNITY OF SCIENCE, supra note 60, at 1.}

\footnote{66. Id.}

\footnote{67. See GROSS, supra note 26, for an explanation and critique of why scientists are metaphysical realists; see also Gibson and Schwartz, supra note 5, at 178-79 (arguing that “while physicians-as-scientists dwell largely in the realm of physics . . . lawyers inhabit the world of nomos”). Even those associated with the so-called “Strong Program” in the sociology of science, which stresses the importance of social factors in the formation and acceptance of scientific theories, reject metaphysical idealism and insist that a sociological account of science must presuppose the interactions of science with an independent physical reality. See, e.g., BARRY BARNES, DAVID BLOOR & JOHN HENRY, SCIENTIFIC KNOWLEDGE: A SOCIOLOGICAL ANALYSIS I, 32 (1996). As Thomas Nagel explains, all attempts to argue against a reality independent of us founder on the need to place ourselves in the world so ordered. THOMAS NAGEL, THE LAST WORD 13-35 (1998).}
scientific understanding to grow cumulatively in the process. Similarly, each generation of scientists in whatever field, standing on the shoulders of the generations before it, moves scientific understanding closer to some notion of objective truth, some final explanation of how the physical universe, or whatever is the subject of their study, works. Final understanding, or objective truth, is the holy grail of science.

In more philosophical modes of inquiry, however, including that used by law, truth is made more than discovered. Truth is temporary, contingent, provisional and partial. It is the truth of language and ideas, constructed in different ways by different groups at different times, and not found pre-existing in nature. There are many reasons for this, but ultimately they reduce to the fact that law operates in a social, not physical, world. Because "the [social] past cannot be recovered," as Richard Posner explains, "it [is] difficult to verify or falsify hypotheses about it.... [This means that] there are... a large number of legal cases in which the question of what happened is indeterminate, and must be resolved by a decision on who shall bear the burden of producing evidence or of persuading the trier of fact." This problem is exacerbated by the fact that law places a "heavy but largely unremarked reliance on

68. See Stephen Hawking, A Brief History of Time 1-13 (1988). This list might even include the "little old lady at the back of the room," who suggested to Russell that "it's turtles all the way down." Id. at 1. Using our understanding of the origins of the universe as an example, Steven Weinberg describes how the process of the cumulative development of understanding in science works. See Steven Weinberg, Before the Big Bang, N.Y. Rev. Of Books, June 12, 1997, at 16-20 (describing the cumulative development of "big-bang" cosmology); see also Newton, supra note 13, at 210 (stating that "[s]cience does make progress."). For a "rhetorical" perspective on the same story, see Gross, supra note 26, at 98-128. Because it is cumulative, and about data which does not move, scientific understanding is different from philosophical understanding. For example, Kant is not so much an improvement on Aristotle as an alternative to him, but Einstein is an improvement on Newton (at least for astronomers, and they are the only ones likely to need either view). See Hawking, supra, at 10 (arguing that "we still use Newton's theory for all practical purposes because the difference between its predictions and those of general relativity is very small in the situations we normally deal with").


70. Id.
being able to determine counterfactual... causes and consequences. This is a particularly difficult process when the question to be answered is why something was done rather than what was done, as is often the case in law. As if this was not enough, in the American legal system, truth is also in competition with many other goals, "such as economy, preserving certain confidences, fostering certain activities, [and] protecting constitutional norms."

As Judge Posner explains further:

... neither the fact that lawyers, like scientists, use induction, nor the intriguing suggestion that scientists, like lawyers, are judgmental rather than mechanical in their use of induction... makes [legal fact finding] scientific in an interesting sense. What is missing from law are penetrating and rigorous theories, counterintuitive hypotheses that are falsifiable but not falsified... precise instrumentation, an exact vocabulary, a clear separation of positive and normative inquiry, quantification of data, credible controlled experiments, rigorous statistical inference, useful technological by-products, dramatic interventions with measurable consequences, and above all and subsuming most of the previous points, objectively testable - and continually retested - hypotheses... [L]aw is closer to theology and to metaphysics than to science. Lawyers are not only quick but unashamed to make emphatic assertions on matters of fact... without attempting, desiring, or even being willing to subject those assertions to an empirical test.

Practices deeply woven into the fabric of law may place even the aspiration of systematic, disinterested factual inquiry beyond law's reach. In short, fact inquiry in law usually will be more context-specific than timeless, more discontinuous than cumulative. And scientists see the pursuit of objective truth as more developmentally

71. id.
73. POSNER, supra note 69, at 69-70.
74. See id. at 70.
advanced than the pursuit of relative truth, or partial truth, or truth good enough for the situation.

This belief in the existence of a kind of disciplinary hierarchy, with science at the top, makes it more difficult for scientists to accept legal rules and methods of proceeding when they clash with scientific alternatives. A less advanced system of inquiry should defer to a more advanced one on matters of common application, everything else equal, scientists believe, and this means that on matters having to do with the investigation of empirical fact, for example, law should defer to science. Scientists can understand that law would sometimes get factual truth wrong. Evidence can be confusing, correct interpretations can be elusive, and charlatans can be as rhetorically gifted as geniuses. However, for a system of regulation, with the power to deprive people of their liberty and property, to work with facts it knows to be false (or could know if it thought about it correctly), and investigative methods it knows to be imperfect (when better methods are available), is seen as indefensible, especially to persons trained to regard truth as the overriding end of work. Scientists simply cannot understand how it could ever be better, or even fairer, for a legal system to be unconcerned with, ignorant of, or oblivious to factual truth.

The cultural tension between law and science is also exacerbated by the self-contained and exclusivist nature of disciplinary thought. All intellectual disciplines analyze data from different vantage points, originate in different starting-point insights, use different analytical methods, work with different basic theory, concepts and vocabularies, and identify the nature of the problems to be solved in different ways. It is not that disciplinary thought, by its nature, is hostile to other ways of thinking about data, but just that disciplinary systems aspire to completeness. They try to take account of, and explain, the data of the particular universe with which they are concerned, and when they cannot do this, they see themselves as not yet fully developed. Given their mutual aspirations to completeness, therefore, it is not surprising that science and law do not reach out to, or interlock with, one another conceptually. Because they are equally self-contained bodies of thought, there is no reason for either to look to anything beyond itself to explain the universe over which it rules. If they meshed to
any substantial degree, it is more likely that they were parts of the same disciplinary system to begin with, and not separate bodies of thought.

The cultural divide between law and science seems definitional, and thus a permanent feature of the landscape. Legal systems will always need to act more quickly, and explain decisions more simply, than science would prefer, so that law and science will always be relegated to some sort of oil and water relationship. If the two systems cannot be linked conceptually, however, perhaps they can cross-pollinate, so to speak, in practitioner discourse. If lawyers and scientists could learn to talk with one another across disciplinary boundaries, explaining themselves and their methods as they work, the two systems of thought might be able to work out satisfactory ad hoc accommodations to one another’s concerns in the process of resolving individual problems. The cultural dimension of the law/science problem has been analyzed extensively, but this inter-professional communication corollary has not.75

In the next section I will take up this topic,76 using a case study

75. There is a large literature on professional (particularly legal) education, which in recent years has included the study of professional communication practices within its scope, but for the most part this work is limited to the study of professionals’ communication with clients and adversaries. Rarely is inter-disciplinary professional communication the focus. But see Naitone, supra note 5, at 312-15 (describing a model course on “interdisciplinary cooperation”). One exception occurs in the literature comparing instructional methods from one professional educational system with instructional methods from another, including inter-disciplinary professional education. The clinical movement in legal education, for example, owes much of its success to arguments based on the success of the clinical method in medical education. I discuss this literature in Robert J. Condlin, Learning From Colleagues: A Case Study in the Relationship Between “Academic” and “Ecological” Clinical Legal Education, 3 CLIN. L. REV. 337, 346-49 (1997).

76. I will not be interested in related issues (about which a lot has been written) such as the way in which the legal world view swallows up other perspectives, forcing all who would use the legal system to transform their problems into legal problems, and relive and solve those problems in terms of legal words, ideas, categories, outcomes, and the like; and the equally complicated and important topic of the effectiveness of lawyer communication in conversations with non-professionals such as clients. See, e.g., DOUGLAS E. ROSENTHAL, LAWYER AND CLIENT: WHO’S IN CHARGE? (1974); Alex J. Hurder, Negotiating the Lawyer-Client Relationship: A Search for Equality and Collaboration, 44 BUFF.
of lawyers and scientists talking with one another about a complicated inter-disciplinary problem. In the process, I hope to identify some of the possibilities and difficulties presented by a "communication" remedy to the law/science incompatibility problem.77

I should reiterate, the need for law and science (and a fortiori lawyers and scientists) to work together is important for several reasons. When law and science do not coordinate law is denied the benefits of the different insights, theories and methods science brings to bear on the complicated problems citizens bring to courts, agencies, legislatures, and law offices in the modern day. As a result, legal resolutions are less intelligent, less stable, and less relevant. This failure to incorporate science, accurately understood, into legal resolutions also undercuts the legitimacy of those resolutions. When judges, legislators and lawyers make inescapably fact-based decisions based on blatantly or demonstrably mistaken understandings of empirical fact, or misapplications of scientific theory and method, it is difficult for those who recognize the mistakes to take such decisions seriously, and to abide by them. Just the opposite, scientifically sophisticated observers are more likely to criticize law as vulgar or unintelligent,


77. I will focus mostly on lawyers in my analysis of the case study. This is not the only perspective one could take, or necessarily the most important one, but it is the perspective I know best and the one about which I have the most to say. Someone coming at the case from a different background, or a different set of experiences, could more productively examine it from another angle and perhaps someone will. Opportunities to use the lessons of such an analysis abound. See, e.g., Symposium 2000, a joint convocation of the American Bar Association's Section on Science and Technology and the American Association for the Advancement of Science, to be held at the University of California at Berkeley in the year 2000. Described by its sponsors as 'expected to generate ... a series of 'white papers' addressing models for collaboration between law and science, educational materials and a series of quadrennial programs to foster ongoing discussion of legal-technological issues." Scott F. Partridge, New Collaborations of Law and Science in New Millennium, Nat'l. L.J., Aug. 3, 1998, at C12.
and to encourage others to ignore its pronouncements. Such a scenario does not bode well for either law or science.

II. THE INTER-DISCIPLINARY CONFERENCE ON TOXIC TORTS

My interest in lawyer-scientist communication in general, and this article in particular, grew out of an opportunity recently provided to observe lawyers and scientists talking with one another about their respective disciplines at a conference on toxic torts. The conference, entitled “Lessons From A Civil Action: Environmental Torts and the Woburn Litigation,” was based on one of the most famous law-science lawsuits of recent years, the leukemia cluster litigation against W.R. Grace & Co. ("Grace"), and Beatrice Foods Co. ("Beatrice"), by a group of residents of Woburn, Massachusetts. The sponsors of the conference

78. Toxic torts appears to be a recently created substantive subdivision within the field of torts and a new category of litigation for personal injury practice. See Robert F. Blomquist, An Introduction to American Toxic Tort Law: Three Overarching Metaphors and Three Sources of Law, 26 Val. U. L. Rev. 795, 796 (1992). It is also the area of law where courts have drawn the most criticism for their misuse of science. See Jasanoff, supra note 1, at 114; see also Carl F. Cranor, Regulating Toxic Substances: A Philosophy of Science and the Law 49-82 (1993) (analyzing challenges presented by scientific evidence in toxic tort cases to present evidentiary procedures used in law); Peter W. Huber, Galileo’s Revenge 192-213 (1991) (criticizing the legal system’s methods for investigating scientific questions in mass tort cases); Jasanoff, supra note 1, at 116-37 (describing the difficulties created for the legal system by toxic tort cases); Peter H. Schuck, Agent Orange On Trial: Mass Toxic Disasters in the Courts 6 (1986) (citing key early literature on toxic torts litigation); Jack B. Weinstein, Individual Justice in Mass Tort Litigation 20-37, 126-54 (1995) (discussing substantive and procedural strategies for judicial management of mass tort cases). The literature on toxic torts is rich and extensive, but the focus of this article is lawyer-scientist communication, not toxic torts. While my interest in lawyer-scientist communication developed only recently, my interest in lawyer communication generally has been longstanding. See, e.g., Robert J. Condlin, Socrates’ New Clothes: Substituting Persuasion for Learning in Clinical Practice Instruction, 40 Md. L. Rev. 223 (1981) [hereinafter Socrates’ New Clothes]; Robert J. Condlin, “Cases on Both Sides”: Patterns of Argument in Legal Dispute-Negotiation, 44 Md. L. Rev. 65, (1985) [hereinafter Cases on Both Sides]; Condlin, supra note 75.


80. For all parts of the case, see generally Anderson v. Cryovac, Inc., 96 FRD
brought together key participants in the lawsuit — clients, lawyers and scientists alike — several years after the case had concluded, to discuss what, on reflection, had been learned from the litigation now that most of the information on its effects was likely to be in.\(^{82}\)

The underlying case itself was prompted by an outbreak of leukemia in Woburn, Massachusetts, during the nineteen-sixties and seventies.\(^{83}\) Studies eventually linked the outbreak to city well water contaminated by industrial chemicals, principally trichloroethylene and tetrachloroethylene.\(^{84}\) Thirty-three plaintiffs from eight families, including five administrators of the estates of children who died allegedly as a result of being exposed to the


81. The Conference, the 1997 Ward, Kershaw and Minton Environmental Symposium, was sponsored by the Environmental Law Program of the University of Maryland School of Law, and held on April 11, 1997, at Westminster Hall, in Baltimore, Maryland. See 1997 Ward, Kershaw & Minton Environmental Symposium, Lessons from A Civil Action: Environmental Torts and the Woburn Litigation, Baltimore, Md. (Apr. 11, 1997) (transcripts on file with author) [hereinafter Symposium]. Westminster Hall has an interesting history in its own right. Formerly a Presbyterian church, it sits on a nineteenth century graveyard housing the remains (or at least the gravesites), of Edgar Allen Poe, James McHenry, a participant in the Constitutional Convention of 1787 and signer of the Constitution, James Buchanan, President of the Bank of the United States in Baltimore at the time of McCulloch v. Maryland, and a number of major figures in the American Revolutionary War. See generally MARY ELLEN HAYWARD & R. KENT LANCASTER, BALTIMORE'S WESTMINSTER CEMETERY & WESTMINSTER PRESBYTERIAN CHURCH: A GUIDE TO THE MARKERS AND BURIALS 1775-1943 1-11 (1984). It also contains some rare examples of funerary architecture by Robert Mills and Maximilian Godefroy, two important nineteenth century architects. See id. The building, properly deconsecrated and retrofitted with a high-tech interior, is now a part of the University of Maryland School of Law.

82. See Symposium, supra note 81.

83. See HARR, supra note 79, at 81.

84. See id. at 78.
chemicals, filed a wrongful death and conscious pain and suffering action against Grace and Beatrice in federal district court in Boston in May of 1982, claiming that the chemicals contaminating the city wells had come from Grace and Beatrice plants.\textsuperscript{85} Thus began what the United States Court of Appeals for the First Circuit would eventually call a “litigatory trek of unusual length and complexity.”\textsuperscript{86}

While pre-trial proceedings were uncommonly protracted and contentious, the most difficult management problems in the case had to do with trial. As Judge Walter Skinner, the district judge assigned to the case, told the lawyers, “I’ve been trying to picture what this trial is going to look like . . . You’ve got thirty-three plaintiffs, and to submit all thirty-three of these causation and damage issues in one trial may be unbelievably cumbersome. It’s very complicated.”\textsuperscript{87} To solve the problem, on the eve of trial and after the jury had been empaneled, Judge Skinner decided to trifurcate the case, that is, to separate the trial into three distinct phases.\textsuperscript{88} The first would be the “waterworks” phase in which the issue would be whether Grace and Beatrice wells had contaminated the City wells.\textsuperscript{89} Next, the medical causation phase in which the

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\textsuperscript{85} The actual claims were a good deal more varied:
Of the 33 plaintiffs in this action, five [were] administrators [of the estates] of minors who died of leukemia allegedly caused by exposure to the chemicals. They [brought] suit for wrongful death and conscious pain and suffering. Sixteen of the 28 living plaintiffs [were] members of the decedents’ immediate families. These plaintiffs [sought] to recover for the emotional distress caused by witnessing the decedents’ deaths. Three of the living plaintiffs also contracted leukemia . . . and [were] either in remission or treatment for the disease at the time of the lawsuit. The 25 non-leukemic plaintiffs allege[d] that exposure to the contaminated water caused a variety of illnesses . . . and damage[d] their bodily systems. All of the living plaintiffs [sought] to recover for their illnesses and other damage, [for the] increased risk of developing future illness, and for emotional distress. Six of the plaintiff families still reside[d] in the area above the allegedly contaminated water, [and they sought] injunctive relief under a nuisance theory.

\textit{Anderson, 628 F. Supp. at 1222.}

\textsuperscript{86} \textit{Anderson v. Beatrice Foods Co., 900 F.2d 388, 390 (1st Cir. 1990).}

\textsuperscript{87} \textit{HARR, supra note 79, at 285.}

\textsuperscript{88} \textit{See id. at 286-87.}

\textsuperscript{89} \textit{See id. at 287.}
issue would be whether the chemicals in the wells had made the plaintiffs sick and killed the children, and finally the “damages” phase, in which the issue would be, in Judge Skinner’s words, “[h]ow much is that worth? How much compensation do you give somebody for the loss of a child?” If the defendants’ chemicals did not contaminate the City water supply, the court reasoned, there was no need to take up the issues of medical causation or damages.

In the waterworks phase of the case the plaintiffs ended up winning “half a loaf,” as one commentator puts it. The jury found the evidence insufficient to conclude that chemicals from the Beatrice plant had leached through the soil and into the City drinking water, but did find Grace responsible for contaminating the City wells. The district court entered a judgment in Beatrice’s favor, and the plaintiffs appealed. While this appeal was pending, the plaintiffs learned that Beatrice had withheld an important hydro-geological report requested during discovery, and moved to set aside the judgment on that basis. The district court denied this motion, plaintiffs appealed, and that appeal was consolidated with the original appeal on the merits.

The First Circuit affirmed the judgment on the merits in favor of Beatrice, but remanded the appeal from the denial of the motion to set aside the judgment, for a determination of whether Beatrice had knowingly or intentionally concealed the hydro-geological report. On remand, after an extensive evidentiary hearing described by one commentator as “longer than most major trials,” the district court

90. See id.
91. Id.
92. See id.
94. See Anderson v. Cryovac, 862 F.2d 910, 914-15, 915 n.2 (1st Cir. 1988).
95. See id. at 914-15.
96. See id. at 922.
98. See Cryovac, 862 F.2d at 913-22.
99. Blomquist, supra note 93, at 974. The hearing took two months, twenty-six witnesses were examined, and 236 exhibits totalling almost three thousand
concluded that “[w]hile the Report might well have been very helpful to the plaintiffs in establishing the transport of chemicals from the [defendants’ plant] to [the] wells, . . . concealment of the Report . . . did not constitute substantial interference with the [plaintiffs’] preparation of [the] . . . case.” 100 The district court then recommended that its earlier denial of the motion to set aside the verdict be sustained, and the First Circuit agreed.101 Following their loss in the waterworks phase of the case, about nine years after the lawsuit had begun, Grace settled the case with the plaintiffs for eight million dollars.102

100. Anderson v. Beatrice Foods Co., 129 FRD 394, 401-402 (D. Mass. 1989). This is the decision that incensed AR, one of the lawyer panelists whose comments are discussed in the next section, and was the basis for his charge that Beatrice’s lawyer had “cheated” and gotten away with it because both he and the judge had attended Harvard Law School, and “Boston is a Harvard town.” AR, Panel Discussion at the 1997 Ward, Kershaw & Minton Environmental Symposium, Baltimore, MD (Apr. 11, 1997) (transcript on file with author); infra note 165.


102. Each plaintiff received $375,000 in cash at the time of the approval of the settlement agreement and another payment of $80,000 in cash five years later. See Harr, supra note 79, at 453-54. There was a dispute over the legitimacy of some litigation expenses, resulting in the return of additional money—either $80,000 or $20,000, it is not clear from the book—to the two sets of plaintiffs willing to accept it. See id. The extravagance of the plaintiffs’ lead attorney in preparing and trying the case is one of the main themes in most accounts of the case. See id. The lawyer-panelists at the Conference characterized this amount as the high water mark for per capita plaintiff payoffs in toxic tort litigation in the United States. See Symposium, supra note 81. Professor Jasanoff suggests that the figure is this high because the plaintiffs were allowed to present the expert testimony of “clinical ecologists” to the jury, and the jury was influenced by this testimony. Jasanoff, supra note 1, at 132-33. Later courts, she asserts, have been much less hospitable to such testimony, finding “clinical ecology” not a board certified specialty, and not identifiable with any set of established standards of practice or body of knowledge. See id.

Some of the plaintiffs questioned whether the settlement amount was as large as it could have been if the plaintiffs had been represented more competently, and at least one plaintiff criticized the settlement for its emphasis on money. See Harr, supra note 79, at 452. This plaintiff was reported to have said that “she wasn’t after money, that what she wanted was for J. Peter Grace to come to her front door and apologize.” Id. The parish priest who helped organize the plaintiffs was “bullshit mad,” as he later put it, when he heard about the settlement, thinking that
The Woburn case is important for many reasons. To begin with, the case was one of the first multi-causal, multi-victim, toxic-tort cases brought against major corporate defendants, tried before a jury, at least in part. Such cases often settle because they are usually thought to be too expensive and too risky to try, for different reasons, by both plaintiffs and defendants. Therefore, Woburn remains a principal training ground for lawyers on both sides of the toxic tort litigation field on the difficulties inherent in full blown adjudication, to a lay jury, of cases based on complicated, and controversial, scientific evidence. The case is also important as a lesson in the limits of the law and lawyers to prevent or undo the harms occasioned by the negligent, and sometimes reckless use of science in the modern state. As much money as the Woburn plaintiffs spent, and as well as they did comparatively, no one involved in the lawsuit is likely to describe the outcome as making anyone whole. In addition, as

"taking Grace’s money without a full disclosure by the company, or any expressions of atonement, cheapened everything." Id. On the other hand, several plaintiffs expressed the conviction that "they had set out to teach corporate America a lesson, and they had succeeded." Id. Like all plaintiff groups, this one was of several minds on most of the issues surrounding the proper determination of means and ends in the litigation.

There is some evidence that the plaintiffs could have had more money if their lawyers had not been overly optimistic in placing a value on the case. See id. at 233-63. The author of A Civil Action traces this misjudgment to the influence of a Harvard Law professor who entered the case once it was underway, and convinced the lawyers that they were aiming too low. See id. He is described in the book, perhaps semi-affectionately, as "billion dollar Charlie," and has an entire chapter named after him. Id. The plaintiffs’ lawyers also passed on the opportunity to settle with Beatrice, deciding to take the case to the jury instead, only to take nothing when the jury found for Beatrice in the waterworks phase of the case. See Richard Bernstein, Of Tragedy and Truth: Caught in a Legal Tangle, N.Y. TIMES, Sept. 13, 1995, at C17.

There is much more that is noteworthy in this American popular literature version of Bleak House, see Blomquist, supra note 93, at 954 (describing the characters in the story as "Dickensian"), but this article is about the lawyer-scientist coloquy the Woburn litigation provoked, not the litigation in its own right. For those interested in the full story, Jonathan Harr’s book, for all of its controversial judgment calls about what happened, makes for a great read. See generally Harr, supra note 79.

103. See Harr, supra note 79, at 453; supra note 102.
104. See supra note 102.
105. For the varying views on the efficacy of the settlement, see supra note
pure narrative, the case had more of the qualities of high drama than most fiction. It was a David and Goliath story of sorts, played out by larger than life characters living on the edge.\textsuperscript{106} The stakes were life and death, and the state (mostly in the form of the district judge who tried the case), loomed ominously in the background, sometimes intervening, not always in an impartial manner.\textsuperscript{107} It is the sort of story that makes for a good novel, and it has,\textsuperscript{108} a good

\textsuperscript{106.} Even though the plaintiffs were outspent by almost three to one over the course of the litigation, they spent $2.6 million themselves (much of it wastefully by all accounts). See Harr, supra note 79, at 453. If the David and Goliath metaphor describes relative rather than absolute disadvantage, then it fits the Woburn case. But it is important to note that $2.6 million is a long way from slingshots. The lead attorney for the plaintiffs is sometimes depicted as a beleaguered champion of good against evil, a lone wolf trying to buck the establishment and send a message to corporate America that it will be held accountable for its poisons. See Bernstein, supra note 102. During the end stages of the protracted negotiation with Grace, for example, he is quoted as saying, “this [is] a political case. If it [was] just a personal injury case, it would have been settled long ago.” Harr, supra note 79, at 417.

\textsuperscript{107.} A complaint that the judge was biased could be expected from the plaintiffs’ lawyers, considering that they lost on a number of important procedural rulings. See, e.g., Harr, supra note 79, at 464 (quoting plaintiffs’ lawyer as “beginning to think the fix is in”). But both the chronicler of the Woburn case, and reviewers of his book, seem to agree. See id. at 287 (describing how Judge Skinner’s management order overtly favored the defendants); Blomquist, supra note 93, at 971 (describing how “Judge Skinner’s approach to the practical cumbrousness of the case unfairly prejudiced the plaintiffs”), and 976 (stating that “[i]t is clear that Judge Skinner’s ... thinly described favoritism towards Beatrice’s lawyer] had a tremendous impact on the outcome of the case”); Bernstein, supra note 102 (describing Judge Skinner as “a judge whose exercise of authority ... is not always easy to understand”); but see Frederick P. Gabriel, His Life After ‘A Civil Action,’ ” Nat. L.J., Dec. 8, 1997, at A26 (describing the book’s portrayal of Judge Skinner as “very narrow.” The author was a plaintiffs’ lawyer in the Woburn litigation). In an interview for an Arts & Entertainment Network documentary several years later, Judge Skinner acknowledged that there was “no question that [his] patience ran out” with the plaintiffs’ lead lawyer. American Justice: A Civil Action (A&E Network television broadcast, Dec. 30, 1998) (transcript on file with author).

\textsuperscript{108.} See Harr, supra note 79; see Blomquist, supra note 93, at 985 (stating that “In my view, it [the Harr book] is analogous to a novel.”). As Professor Blomquist points out, the Harr book belongs more accurately to the new literary genre of “faction,” that is, “fiction which is based on and combined with fact.” J.A. Cuddon, A DICTIONARY OF LITERARY TERMS AND LITERARY THEORY 505-506 (1991). For a discussion, see Blomquist, supra note 93, at 984-85 (finding the book a “significant” example of popular American literature “on at least three
television documentary, and it has several times, and a good movie that is now out with John Travolta in the lead lawyer role. Moreover, as the topic of an inter-disciplinary conference on law and science, it was a natural.

The one-day conference consisted of three formal panel presentations, each followed by an audience question and answer session. The first panel described the clients’ perspective on the lawsuit. The principal named plaintiff in the litigation, generally agreed to be the person most responsible for calling attention to the contaminated water problem in the first instance, and a Woburn parish priest who helped her to organize the plaintiff class and convince federal and state regulatory agencies to take the problem seriously, were among the panelists.

The second panel described the lawyers’ perspective. It included three attorneys experienced at toxic tort litigation, two of whom had participated in the lawsuit, and one of whom had read the book. The first was the director, at the time of the lawsuit, of one of the largest and most influential plaintiff, public-interest litigation law firms in the country. The second was a partner in a major, big-city law firm specializing in large-scale personal injury

different levels”). As Professor Blomquist explains, it is “a window on the emerging cultural obsession with the American tort system,” brilliantly depicting “how ‘big money’ talks and everyone else walks” in American civil justice. Id. at 985. It also is “full of riveting and humorous references to lay perceptions of the pomposity of the law, lawyers and legal institutions,” providing “an intriguing view of how laypersons often regard the law.” Id. And finally, its “highly-wrought satirical and political characterization . . . delicately and indirectly ridicules, censures, and derides the egotistical and money-grubbing world of the ‘big shot’ trial lawyer,” in a fashion “reminiscent of George Orwell’s . . . Animal Farm,” and Tom Wolfe’s Bonfire of the Vanities. Id. at 987 (emphasis added).

109. See, e.g., 60 Minutes (CBS television broadcast, Mar. 2, 1986) (transcript on file with author); American Justice: A Civil Action, supra note 107; A Civil Action: In Pursuit of Justice (Court TV Network television broadcast, Jan. 8, 1999) (transcript on file with author).

110. See Gabriel, supra note 107, at 1 (reporting that “Mass. plaintiff’s lawyer will be played by John Travolta in movie”); Bernstein, supra note 102, at C17 (quoting dustjacket of the book).

111. See Symposium, supra note 81.

112. See id.

113. See id.

114. See id.
The third was a partner in another major, big-city law firm representing corporate defendants, including some of the most powerful corporations in the country, in toxic torts litigation. The third panel provided the scientists’ perspective, and included environmental and public health professors from two major Boston area universities, each of whom had been retained as an expert by the plaintiffs in the Woburn case.

All of the lawyer and scientist panelists were highly regarded. Each was a major figure in his (the lone women scientist dropped out on the day of the conference) particular field. Each had written extensively about the generic issues involved in the lawsuit, and one of the scientists had even produced original research data for the Woburn case based on an innovative new theory for proving causal relationship. Each had an appointment at a major research university or, in the case of the lawyers, was a well-known instructor in national continuing education programs. Each had well defined and sophisticated views on the substantive and methodological issues in the “two cultures” debate, and four of the five had participated in the Woburn litigation in one capacity or another. It was a blue-chip, or gold-plated, panel of experts in any sense of the term. The audience was almost equally expert, containing as it did, scientists and lawyers of considerable repute in their own right, many of whom had worked on cases or projects raising issues similar to those in the Woburn litigation.

The conference discussions provide particularly attractive data for the study of lawyer-scientist interaction, not only because they examined such interaction explicitly, but also because they modeled it, or tried to, at the same time. The participants in the conference, panelists and audience members alike, were engaged in the very behavior that they also were attempting to analyze, giving an observer an independent vantage point, free from the

115. See id.
116. See id.
117. See id.
118. This study was later published. See S. Lagakos et al., An Analysis of Contaminated Well Water and Health Effects in Woburn, Massachusetts, 81 J. AMER. STAT. ASS’N 583 (1986).
weaknesses latent or explicit in the speakers’ espoused theories, from which to judge the efficacy of what they said. One can compare the panelists espoused views with how they acted, to determine what to believe about the possibilities and limits of lawyer/scientist cooperation. It also helps that all of the conference panelists, and several members of the audience, were experienced at inter-disciplinary cooperation, most having worked with members of the other profession in a wide variety of law related activities (e.g., drafting legislation, commenting on administrative rules, testifying in court, and the like). Each of the panelists also was generally regarded as very good at this process. At several instances during the course of their respective presentations, for example, panelists commented favorably on the sophistication of the other panelists’ understandings of the issues under discussion, and excepted them explicitly from the criticisms then being made of the other profession. The conference participants, panelists and audience members alike, were aware of the obstacles to lawyer/scientist cooperation, and were committed to finding ways to make the process work. As a group, they believed in the possibility of inter-disciplinary work, and thought that none of the difficulties encountered in producing it were insurmountable. In short, the conference participants were an ideal group for making the process of inter-disciplinary cooperation work.

III. LAWYERS AND SCIENTISTS IN CONVERSATION

A number of times during the conference, the lawyers and scientists in attendance, both on the panels and in the audience,
joined issue, or almost joined issue, over major substantive questions in the “two cultures” debate. In this section I will reproduce and analyze one extended example of such a discussion. In analyzing this discussion I will sometimes comment on the substantive issue under consideration (i.e., the role of uncertainty in law and science), only because it is not possible to talk about what the panelists said without talking about what they said, so to speak. But my primary focus will be on how the panelists analyzed the issue of uncertainty, not with whether they got the analysis right. The uncertainty issue is one of the most difficult in the intersection of law and science, and it is not realistic to expect to resolve it, or even give it an adequate airing, in this article. But it is possible to determine whether the manner in which the speakers discussed the issue made their conversations more productive, and made each side’s position clearer to the other.

A. The “Uncertainty” Exchange

The following discussion is taken from a question-and-answer session following the formal presentations by the three members of the lawyers’ panel. The panelists had been asked to comment generally on what they thought were the most significant lessons to be learned from the Woburn litigation. The first panelist (“AR”), was the only one who had participated in the Woburn case directly. He began his presentation with a kind of “only in America” point, extolling the “wonders of a civil justice system” which allows a dispute like Woburn to be resolved in a court, on the basis of law, rather than in the streets, on the basis of force, as it would have

120. See Symposium, supra note 81.
121. The conference was videotaped and the exchanges reproduced here are verbatim transcriptions from that tape. I will identify the participants in each excerpt by their initials, but will not name them. Confidentiality was not promised to the panelists, the conference was open to the public, was advertised widely, and a program of the proceedings listing the speakers is readily available. Those interested in learning names have plenty of options through which to do so. For my purposes, however, names add nothing to the analysis. For a brief summary of the Conference, see Maureen O’Doherty, 1997 Symposium—Lessons From A Civil Action: Environmental Torts and the Woburn Litigation. ENVIRONMENTAL LAW AT MARYLAND, Summer-Fall 1997, at 8.
122. See Symposium, supra note 81.
been in some “third world country.”  

He then spent a few minutes making a plea for the defeat of tort reform proposals in Congress, seemingly as a matter of habit. When he turned his attention to the Woburn case, he praised the outcome as a “bonanza recovery” for the plaintiffs, notwithstanding that two plaintiff representatives from the previous panel had excoriated the outcome, the lawyers who produced it, as well as AR’s assumption that money was the best standard for measuring success. He also described one of the scientists on the next panel as the finest expert in the country working on the problem of proving a linkage between exposure to toxic material and harm, and characterized the Woburn case as a kind of modern day “To Kill a Mockingbird” story, in which the lawyers involved showed how it was still possible to do both well and good in the practice of law. He concluded by speculating about who would play his part in the movie. He was charming (or insufferable, depending upon whom one asked) throughout.

The second panelist (“RS”), described himself as an advisor to the plaintiffs’ lawyers in the Woburn case, but not a direct participant in the case itself. His remarks, particularly those about the relationship between law and science, were perhaps the most provocative of any made by the panelists. He described four categories of lessons to be learned from Woburn: ethics, politics, science, and psychology. The ethics lesson was a reprise of the well known point that choosing one’s clients is as much of an ethical issue as deciding how to conduct a representation, and that, everything else being equal, one ought to choose clients who are completely (or perhaps mostly) in the right. The political significance of Woburn lay in the fact that it was the first major

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124. See id.
125. See supra note 102, for a description of the differences between the plaintiffs’ and lawyers’ views on the settlement.
126. See AR, Panel Discussion, supra note 123.
127. See id.
128. See RS Panel Discussion, infra note 143.
129. For a thoughtful and sophisticated defense of this view, see Simon, supra note 76.
effort by toxic torts plaintiffs to litigate, rather than settle a lawsuit based on complicated scientific evidence, against well financed corporate defendants.\textsuperscript{130} He agreed with AR that the outcome of the case was the high water mark in plaintiff’s success stories for such litigation to date. The science lesson in Woburn was about the nature of “uncertainty” in law and science.\textsuperscript{131} This was the most interesting of RS’s four points, and probably the impetus for the discussion set out below. Since I reproduce this discussion verbatim, I will not summarize RS’s point here. RS’s final lesson, about the psychology of Woburn, was a provocative defense of the right, even the need, for plaintiffs’ lawyers to take control of complicated and messy cases such as Woburn away from their clients when all of the parties do not agree on either optimal outcome, or preferred strategy.\textsuperscript{132} This claim appeared designed to respond to a member of the earlier client panel, who had criticized the plaintiffs’ lawyers for not taking the principle of client control more seriously.

The third and final speaker (“BB”), was the only member of the lawyers’ panel who customarily represented defendants, and who, except for reading the Civil Action book, had no connection with the Woburn case. Understandably, his views on what could be learned from the litigation were different from those of his co-panelists. He started with a series of small, discontinuous, practical points about strategic choices involved in tort representation generally, elaborating on each for a paragraph or two, without developing any in great detail.\textsuperscript{133} Many of these points had the potential to be controversial, particularly those describing the way in which the legal system “distorts” stories by translating them into legal terminology and legal categories, but BB’s cryptic treatment of the points gave a listener very little with which to contend.\textsuperscript{134}

\begin{itemize}
  \item[130.] See RS, Panel Discussion, infra note 143.
  \item[131.] See id.
  \item[132.] Under different circumstances RS’s “psychology” lesson would be worth exploring, but given this article’s focus on law-science interaction, I will put it to the side.
  \item[133.] See BB, Panel Discussion at the 1997 Ward, Kershaw and Minton Environmental Symposium (Apr. 11, 1997)(transcript on file with author).
  \item[134.] See id. There was one exception. BB made the unfortunate decision, as it turned out, to respond to AR’s comments about tort reform. Both RS and AR
\end{itemize}
Like RS, BB also raised the uncertainty issue, asking at one point, "How much certainty should the law require?" but unlike RS, he did not offer an answer to the question, or suggest ways in which one could go about arriving at an answer.\footnote{135} He was the last of the panelists to speak.

Almost immediately upon the conclusion of the panelists' remarks a scientist in the audience asked what appeared to be a heartfelt and sympathetic question about the legal system's stance toward uncertainty.\footnote{136} This question provoked a somewhat remarkable set of responses from the lawyer-panelists, and those responses are set out verbatim below.\footnote{137} This exchange between the audience member and the lawyer-panelists provides an extended look into the way lawyers explain themselves, and the legal system, to a scientist confused about the law's response to a problem central to both disciplines.\footnote{138}

Since the scientist's question was probably provoked, or at least encouraged, by the comments about uncertainty RS made during his panel presentation, I will begin with those comments.\footnote{139} Then, I will set out the scientist's question, followed by the panelists' responses.\footnote{140} Finally, I will discuss the comments of another member of the audience, also a scientist and on the next panel, made during the course of the discussion of uncertainty.\footnote{141} Though the entire exchange is quite long, lasting almost fifteen minutes at the conference, I will reproduce it in its entirety, analyzing it as I go. I do this because it is difficult to understand any single subset of the discussion without being aware of all that was said.

\footnote{135}See BB, Panel Discussion, infra pp. 236-37.
\footnote{137}See infra pp. 232-33, 236-37, 243-45, 250-51, 253-54.
\footnote{138}See id.
\footnote{139}See infra pp. 220-22.
\footnote{140}See infra pp. 229-30, 232-33, 236-37, 243-45, 250-51, 253-54.
\footnote{141}See infra p. 257.
About halfway through his panel presentation, RS made the following statement about the relationship between legal and scientific proof, and the role of certainty in each.\footnote{To make it easier to follow my analysis of the speakers' comments, I will number the lines of each of the excerpts. The excerpts are verbatim transcriptions of what each speaker said, ungrammaticalities and all. I belong to the "Non-Malcolmite" school of excerpting and quoting. I believe that anything represented as speaker comment should be speaker comment, and nothing else. See Masson v. New Yorker Magazine, Inc., 501 U.S. 496, 502 (1991) (litigating the accuracy of quotations attributed to plaintiff by a writer for defendant magazine, who reproduced what were described as actual conversations from unrecorded notes).}

RS:

(01) ... Now there's obviously an enormous difference
(02) between legal and scientific proof. Right. I think
(03) that goes without saying. And there's been an ongoing
(04) debate about how much proof is necessary. I'm not going
(05) to argue that point to you, only to point out that there
(06) are significant differences. I think the major
(07) philosophical thing I want to point out to you, though,
(08) is this: that the notion that legal and scientific proof
(09) are very different ... and this is something DO [a
(10) scientist on the next panel] ... is in and of itself
(11) based on a misconception that I think almost everybody
(12) shares. And that misconception is that science provides
(13) a kind of scientific or accounting certainty that really
(14) it doesn't provide. That people would like to believe,
(15) doctors would like to believe, scientists would like to
(16) believe, and the rest of us would believe that science
(17) involves certainty. Now, how do we know this is not
(18) true? I think most people believe that the two
(19) great leading ... among the two great scientific minds
(20) in our time are Stephen Hawking and probably Doctor
(21) Feynman. Stephen Hawking said in the introduction to
(22) his book that all physicists of any knowledge at all
(23) believe that there are two principles that govern
(24) twentieth-century physics. The first are the principles
(25) of large forces, general relativity and gravity, and the
other set of principles are those of quantum mechanics, small particles, and how they interact. And then Hawking goes on to say that every physicist, after recognizing it, also recognizes that these two theories are inherently inconsistent, and that one of them is wrong. Now that's sort of a shocking comment because it's so inconsistent with what most of us think of science. Science tells us how things are done, and we really know the answers. Now these points are important because in the legal question of how much certainty is needed, there tends to be a belief that, oh, if it's not written right there, that it's been proven somebody's found it, then it's not scientifically proven. Now what's wrong with that thinking? What's wrong with the idea that science is really more certain? Well, the answer is that when you go to the doctor, those of you that go to the doctor every day, many of the things that the doctor does are not scientifically proven. There's limited information about the effect of the drug, limited information about you, and limited information about how it might or might not work, and judgments are made. There's no proof as to what'll happen. Some doctors will tell you that the medical method that's most used is like this: the patient comes in on the first day and says that they have a condition. The doctor's never seen it before, and the doctor looks very serious, and says hmm, this is a very unusual condition, aah, have you ever had it before? The patient says, "Yes I did. When I had it the doctor told me to do X, Y, and Z." And the doctor says, "Well, if it worked before it would be a very good principle to try that again. It's not known to hurt anybody and why don't you do that." And of course the next time the patient comes in with this problem the doctor says, "Well, that's a fairly rare condition, it's not written about much in the articles, but in the past if you do X, Y, and Z,
(62) it’s been shown to have some positive effect.” And by
(63) the third time, of course, it’s a well known condition.
(64) O.K. How does this kind of uncertainty play in toxic
(65) cases? [RS then shifts to a discussion of the difficulties in
using epidemiological evidence to prove cancer.]143

RS says a number of things here that could cause a listener to
wonder about the depth of his understanding of science, or the
precision with which he uses language. In fact, concerns of this
sort may have been the principal reason for the discussion that
followed the panel presentations. RS starts out by seeming to
contradict himself. He asserts both that the differences between
legal and scientific proof are “obvious,” and “enormous,”144 and
that this view is “based on a misconception.”145 The
misconception, as it turns out, does not have anything to do with
the relationship between legal and scientific proof, but with the
fact, as RS understands it, that “almost everybody,” (everyone
other than RS?) believes that “science provides a kind of scientific
or accounting certainty.”146 This lack of connection between the
two parts of RS’s statement could confuse a listener, but even if
not, RS’s statement is a needlessly confusing way to put what is
probably not a very controversial point. More importantly, there is
a kind of arrogance in the way he states his point that could
antagonize as well. Many first rate minds have thought about the
relationship between legal and scientific proof over the years, and
there is not much likelihood that RS has discovered something
these thinkers have missed. Claims to having a lock on the one,
true understanding of any subject do not have a good track
record—they tend to be made more by mystics and charlatans than
by geniuses—and in making such a claim, RS runs the risk of
having his listeners think either that he cannot be taken literally, or
that he is a little megalomaniacal. This loaded use of language
continues in RS’s description of the misconception point—that

143. R.S., Panel Discussion at the 1997 Ward, Kershaw and Minton
Environmental Symposium (Apr. 11, 1997) (transcript on file with author).
144. See RS, Panel Discussion, supra p. 220, at lines 01-03.
145. Id. at lines 09-10.
146. Id. at lines 12-13.
science provides “accounting” [i.e., mathematical] certainty.147 “Science” is a somewhat unspecific category as discussed here. For RS’s purposes, there is not so much something called science as there are subject-matter fields of science, many of which differ from one another as much as science generally differs from law.148 In some of these fields, facts are sometimes known with what might legitimately be called certainty, at least in the ordinary language sense of that term, even though such conclusions may be no more than probability predictions with a very high likelihood of being true.149 RS’s casualness about such distinctions may have served as a kind of warning to members of the audience about difficulties to come.

Next, RS gets to the heart of his complaint by accusing scientists of indulging in a kind of willful self-deception, of believing “that science involves certainty” when it does not.150 It is not clear why he thinks this is true, and he fails to explain his reasons. However, this is a fairly serious charge, not that far removed from accusing scientists of being dishonest, at least with themselves, or perhaps even being manipulative in encouraging, or not dissuading, others from believing what scientists know to be false. RS may not have intended to make such an accusation, but it is a reasonable interpretation of his comments, and from my conversations with conference participants, some listeners gave the comments this interpretation. RS’s proof for this claim151, seems to consist of a paraphrase of Stephen Hawking’s description of the well known problem of reconciling general relativity and quantum

147. See id. at line 12.

148. See, e.g., KARIN KNORR CETINA, The Care of the Self and Blind Variation: The Disunity of Two Leading Sciences, in THE DISUNITY OF SCIENCE, supra note 60, at 287 (discussing the “different kinds of empiricism” to be found in different fields of science, using high-energy physics and molecular biology as examples).

149. BB uses one of the most common examples of such a prediction in his discussion of the uncertainty topic, when he points out that, while only a probability, we are “pretty certain that the earth revolves around the sun and not vice versa, as people used to think.” See BB, Panel Discussion, infra p. 237, at lines 23-26.

150. RS, Panel Discussion, supra p. 220, at lines 16-17.

151. See id. at lines 16-17 (responding to the “[H]ow do we know this...” question.).
mechanics,\textsuperscript{152} and an anecdote about medical diagnostic technique.

\textsuperscript{152} RS’s comment probably refers to HAWKING, supra note 68, at 11-12. Compare Hawking’s statement of the point with RS’s paraphrase. Hawking says:

Today scientists describe the universe in terms of two basic partial theories—the general theory of relativity and quantum mechanics. . . . The general theory of relativity describes the force of gravity and the large-scale structure of the universe. . . . Quantum mechanics, on the other hand, deals with phenomena on extremely small scales. . . . Unfortunately, however, these two theories are known to be inconsistent with each other—they cannot both be correct. One of the major endeavors in physics today . . . is the search for a new theory that will incorporate them both—a quantum theory of gravity. We do not yet have such a theory, and we may still be a long way from having one, but we do already know many of the properties that it must have. And . . . we already know a fair amount about the predictions a quantum theory of gravity must make.

Id.

RS uses the inconsistency between general relativity and quantum mechanics to establish that science must be “wrong” in some fundamental sense, and thus not worthy of any special respect. See RS, Panel Discussion, supra p. 221, at line 31. Science, he argues, does not provide “certainty” anymore than other fields of knowledge, and this means that we should not trust scientific knowledge to any greater extent than we would trust knowledge generally. See id. at lines 12-14. Hawking, on the other hand, makes the point that general relativity and quantum mechanics, by themselves, are not so much wrong as incomplete, that they lack an overarching framework that would “incorporate them both,” and he expresses confidence that such a “final unified theory” might be possible. HAWKING, supra note 68, at 11-12. A final unified theory might modify relativity and quantum mechanics rather than reject them, or it might not, but it would build upon them in either case. See id. In this sense, Hawking illustrates how science is different from other types of disciplines—how in science understanding develops cumulatively rather than in \textit{ad hoc}, self-contained, or episodic fashion—and how this might entitle it to a special kind of respect. See supra notes 54 & 55, and accompanying text. For another description of the cumulative development of understanding in science, see Weinberg, supra note 68, at 20 (stating “At least within the last century, no . . . major theory that became the consensus view of physicists or astronomers—in the way that the big bang theory has—has ever turned out to be simply wrong. Our theories have often turned out to be valid only in a more limited context than we had thought, or valid for reasons that we had not understood. But they are not simply wrong . . . .”). Weinberg also points out how scientists have “generally been able to dodge [the] problem [of reconciling general relativity and quantum mechanics] because gravitation and quantum mechanics are almost never both important in the same context. . . . It is only in the very early universe that gravitation and quantum mechanics were both important [and thus, also in the] . . . famous theorems of Roger Penrose and Stephen Hawking [which] use General Relativity to show that there must have been a definite beginning to the universe. . . .” Id.; see also STEPHEN HAWKING AND ROGER PENROSE, THE NATURE OF SPACE AND TIME (1996)(1994 Newton Institute debate
His reference to Hawking seems more for the authority value of Hawking’s name than for the substance of what Hawking said, which is not described in any detail. Even more so with the reference to Richard Feynman, who is mentioned only by name.

It does not follow from the fact that there is no grand unified theory in physics, for example, that all science-based predictions are indeterminate. Many fields are not affected strongly by the lack of a grand unified theory. It also does not follow that either relativity or quantum mechanics must be “wrong,” as RS says, in some robust sense of that term. Fine tuning of one or the other theory may be all that is needed. It is not clear why RS would want to get into this issue in the first place. He may not intend his words to be taken literally, but think what a strange thing it is for a member of a profession whose stock in trade is language, not to want to have his words taken literally, especially when speaking at Cambridge between Hawking and Penrose over whether it is possible to combine quantum field theory with general relativity to produce a theory of quantum gravity; what scientists often refer to as the “Einstein-Bohr debate”). RS’s example has significance, in other words, because his understanding of the problem comes from Hawking. Yet his use of Hawking is explained only by the claim that “most people believe that the two great leading . . . scientific minds in our time are Stephen Hawking and probably Doctor Feynman.” While Hawking and Feynman are clearly giants of twentieth century science, the concept of a “leading scientific mind,” by itself, being authoritative is problematic by science’s own standards. IAN HACKING, The Disunities of Science, in THE DISUNITY OF SCIENCE, supra note 60, at 69 (arguing that “[t]hroughout most of our century, regimes and practices of experimentation and instrumentation have been more powerful as a source of unity among diverse sciences than have grand unified theories.”)

153. See HAWKING supra note 152. For a clear but sophisticated discussion of Hawking’s contributions to solving the paradoxes of “cosmogony,” that is, the study of the origins of the universe, see FERRIS, supra note 11, at 249-54.


155. Even if a grand unified theory (or GUT) were to be developed, it would still not provide a complete theory of the universe since GUT’s do not take gravity into account. The possibility of a GUT itself, however, seems “all but dead” since the concession in 1989 by Howard Georgi, the Harvard physicist who coproduced SU(5), the only GUT to make a prediction that could be tested, that “[i]t does not seem likely that the simple version of SU(5) has been implemented by nature.” FERRIS, supra note 11, at 218, quoting Howard Georgi, postscript to A Unified Theory of Elementary Particles and Forces, in PARTICLE PHYSICS IN THE COSMOS (Richard A. Carrigan Jr. and W. Peter Trower, eds. 1989).

156. RS, Panel Discussion, supra p. 221, at lines 27-31.
with other professionals not trained in law.

RS’s basic point seems to be that knowledge in science is based on interpretive judgments about patterns and associations in data, just like knowledge in other fields of study. Such a conclusion suggests that science does not have some sort of special or unique access to a world of absolute truth.\(^{157}\) This is not a startling conclusion, of course, and it is one that the scientists in the audience would agree with. In fact, several of the scientists said something similar many times over during the course of the proceedings. The difficulty with RS’s point then, is not in what it says but in the way in which it is said. RS seems to see himself as debunking science, puncturing an over-inflated balloon and bringing it down to earth. But his principal evidence to illustrate that scientists are just like the rest of us is a joke. That is he tells an extended story about a doctor who, when he or she is unable to diagnose a patient’s condition, appropriates the patient’s self-diagnosis as her or his own (not really, since another doctor suggested the idea to the patient),\(^{158}\) and then reifies it as a diagnostic category.\(^{159}\) The doctor is presented as somewhat of a phony, passing off a home remedy as expert insight,\(^{160}\) either oblivious to what he or she is doing, or not concerned about it. If the doctor is seen as a proxy for scientists generally, as RS seems to intend, the criticism is then made global. RS tweaks science’s nose in a sense, saying “Ahah, caught you. You don’t know any more than the rest of us, and don’t pretend that you do.” The irony in this, if that is what RS is doing, is that science is faulted for failing to meet expectations that are never explained as anything more than popular (mis)conceptions of what science does. Science is hoisted

\(^{157}\) Judge Posner makes a similar point. See Posner, supra note 69, at 65, n.38 (stating that “the sense in which such refuted scientific theories as the Euclidean theory of space or Newton’s laws of motion are ‘false’ is a rather special one; for most purposes, including most technological purposes, they are true (‘true enough’).”); see also Weinberg, supra note 68, at 20 (stating that “Our theories have often turned out to be valid only in a more limited context than we had thought, or valid for reasons that we had not understood. But they are not simply wrong”).

\(^{158}\) See RS, Panel Discussion supra p. 221, at lines 39-45.

\(^{159}\) See id. at pp. 221-22, lines 46-62.

\(^{160}\) Like the proverbial snake-oil salesman.
on RS's non-scientific petard.161

Why RS would frame the issue of uncertainty in terms of an almost rhetorical question,162 is hard to understand. If one wants to establish credibility with an audience of scientists, taking up rhetorical questions and depicting scientific inquiry in cardboard cutout terms seems the wrong way to proceed. It could be expected to provoke a defensive or dismissive reaction more than a sympathetic or interested one. RS's statement looks a little like the advocacy stratagem of overstating a point to provoke a "How can you say that?" type of question from a listener (usually a judge), which one is then prepared to answer with a string of pre-planned and more measured arguments.163 But why RS would want to

161. If RS did not intend to ridicule science, and I assume that he did not, it is difficult to determine why he spoke as he did. Perhaps he had been put upon recently (or often) by scientists in conversation, or felt oppressed by the enthusiastic claims scientists can sometimes make, and was repaying the favor in kind. For such an enthusiastic claim see, Edward Wilson's "preliminary definition" of human nature.

Human nature is not the genes, which prescribe it. It is not the cultural universals, such as the incest taboos and rites of passage—they are the products of human nature. Rather, human nature is the epigenetic rules, the inherited irregularities of mental development. These rules are the genetic biases in the way our senses perceive the world, the symbolic coding by which we represent the world, the options we open to ourselves, and the responses we find easiest and most rewarding psychologically to make.

Wilson, supra note 69, at 58.

Or, perhaps RS was just insecure about being a lawyer in a room full of scientists. There was a time when the law journals were filled with breast-beating articles bemoaning the lack of a distinctive legal subject matter and method, and questioning the legitimacy of law's place in the academy. See, e.g., Areeda, supra note 11, at 1031 (stating that "we now wonder whether 'law' is anything other than a combination of economics, philosophy, political science, statistics, semantics, and other disciplines"). These concerns destroyed the confidence of many lawyers for inter-disciplinary collaboration. See id. at 1038 (arguing that "this recognition has created a crisis of confidence in what law teachers do").

162. That is, whether uncertainty exists in science.

163. The strategy is based on the belief that a judge (or anyone), is persuaded more readily when engaged in conversation and thinking along with the speaker, than when being lectured. It is a version of the Aristotelian idea of enthymematic argument. See C. PERELMAN & L. OLDBRECHTS-TYTECA, THE NEW RHETORIC: A TREATISE ON ARGUMENTATION 230, 234 (1969). Enthymeme is Aristotle's term for the syllogism of rhetoric. See ARISTOTLE, RHETORIC, I, 1, 1355(a); II, 22, 1395(b).
persuade scientists of his take on their work, rather than learn their views of what they do, or why he would think of his conference presentation as analogous to a courtroom argument, is difficult to discern.

RS’s comments are also notable for what they omit. He does not seem curious about what the scientists in the audience can tell him about uncertainty, or qualify the expression of what, on his own terms, is a controversial view. Nor does he express doubt, hesitation, or humility in analyzing a complicated subject outside his area of expertise. If, as it seems, he compensates for his lack of substantive command by putting things in stronger than warranted language, he risks reinforcing a common stereotype among scientists and others about law and lawyers, that in law it is how one argues and not what one argues that counts. Yet RS seems unaware of this risk, or the extent to which his manner of speaking could trigger it. His comments could lead the scientists in the audience to conclude that there is not much to be gained from an extended discussion with him, about the intersection of law and science. However, that does not happen. At the close of the panel presentations, and after an unrelated question to AR about his use of the term “judgment,” a scientist member of the audience asked

164. Or lawyers generally, since RS was thought to be scientifically sophisticated for a lawyer.

165. See AR, Panel Discussion at the 1997 Ward, Kershaw and Minton Environmental Symposium (Apr. 11, 1997) (transcript on file with author). After describing what he meant by the term “judgment,” AR used the bulk of this first panelist response in the question and answer session to make what seemed to be an unprovoked attack on the Beatrice lawyer in the Woburn case. See id. AR charged that the lawyer had “cheated” in the case by willfully withholding information properly requested in discovery, and that the presiding judge had failed to sanction the lawyer for this cheating out of a sense of loyalty to a fellow Harvard Law School graduate. See id. This should not surprise anyone, AR asserted, because “Boston is a Harvard town.” Id. AR also is a Harvard Law School graduate. The Beatrice lawyer was not present at the conference to offer another view, and none of those who were present seemed privy to all of the facts so as to be able to present the other side. A scientist member of the next panel was so offended by the attack that he asked during lunch whether AR’s remarks were “libelous” and whether he should tell the Beatrice lawyer about them. The story of the discovery incident, as told in the Civil Action book, could be interpreted as an instance of cheating, but this is not its most obvious interpretation, or perhaps even its most likely. On the other hand, Professor Blomquist seems to read the book’s account in this way. “Judge Skinner routinely favored [the Beatrice
the following question which led to a discussion that took the remainder of the question and answer period.

Sci:

(01) I am from the staff of a scientific association, and I’d
(02) like to discuss one of the things that our members, or
(03) some of our members, think about and talk about, and I
(04) think are truly somewhat troubled about philosophically,
(05) and I’ll let any of the panelists address it. And that
(06) is the issue of uncertainty in science. RS is
(07) absolutely right. Science is a constantly growing
(08) process, and the information we have tomorrow, next
(09) week, or next decade may make us realize that what we
(10) knew yesterday, last week, or ten years ago was wrong.
(11) Or, it confirms us even further in our belief that what
(12) we thought or said at the time, indeed, we even have
(13) stronger evidence for it today. Scientists are very
(14) comfortable with this uncertainty. It’s one of the
(15) things that makes science exciting actually, that
(16) there’s always a new discovery, or else it would be
(17) fairly boring. But it is a problem when we get into
(18) a courtroom where a decision can’t wait ten years. A
(19) decision has to be made now. And we’re somewhat puzzled
(20) about the philosophy of how you deal with this
(21) uncertainty. After all, I think, there seems to be some
(22) general agreement about the case that we’re discussing
(23) today, that these companies and some of these chemicals
(24) they used, they disposed of them in an irresponsible
(25) way. They got into the water system, they contaminated
(26) it. There’s less certainty about whether those
(27) substances cause leukemia. Indeed, many very
(28) distinguished scientists, as I read the book, think it
(29) does not cause leukemia [here, RS tries briefly to

lawyer] throughout the long course of the litigation apparently due to law school ties and prior acquaintance." Blomquist, supra note 93, at 971-72. He cites, as an example, the judge’s finding that the lawyer was not to blame for failing to turn over key documents in discovery. See id.
interrupt, saying “let me give you some comments . . .”,
but the questioner continues on, speaking over him,
and in ten years we may find that there was, indeed,
something going on in that city, but it’s something we
haven’t dreamed of yet. For example, ten years ago we
did not dream that women got breast cancer, not because
of something they ate, or bad luck, but because of a
gene that they inherited from their mother, or father,
they were fated to have that. How do you wrestle with
this, and I . . . I . . . I, this uncertainty. I’d just
like you to comment on it.\textsuperscript{166}

One of the first things one notices about this statement is that it
asks a genuine question. It does not criticize the panelists’ views
on uncertainty, contest a lawyer’s right to speak about uncertainty
in science, or try to sneak in an unflattering comparison of law with
science, \textit{sub silentio},\textsuperscript{167} under the guise of asking a question.\textsuperscript{168}
Some issues remain unclear. For instance, does the speaker want to
know how lawyers and judges reach their conclusions when they
are in doubt, how they control the emotional dissonance having to
make such decisions, how the legal system justifies decisions made
on the basis of less than full information, what mechanisms the
system has for correcting error once discovered, or all of the above.
Nonetheless, one issue is clear: The speaker is curious, not
combative. While she is puzzled about the legal community’s
response to the uncertainty problem, she is not critical of it, at least
not publicly, and not yet.\textsuperscript{169}

The questioner also understands that law is different from
science, that “a [legal] decision can’t wait ten years,” and that
courts must act even when they are not clear about what to do.\textsuperscript{170}
Because of this, she suggests, ever so slightly, that science may

\textsuperscript{166}Sci. Panel Discussion at the 1997 Ward, Kershaw and Minton
Environmental Symposium (Apr. 11, 1997) (transcript on file with author).
\textsuperscript{167}See \textsc{Black’s Law Dictionary} 1428 (6th ed. 1990) (defining \textit{sub silentio}
as “[u]nder silence; without any notice being taken.”).
\textsuperscript{168}In their generic form, these latter moves are perhaps more prevalent than
genuine questioning in conference discussion sessions.
\textsuperscript{169}Sci, \textit{supra} p. 229, at lines 04, 18.
\textsuperscript{170}\textit{Id.} at line 18.
have an easier time accommodating uncertainty than law.\textsuperscript{171} Scientists are not tied to external timetables and can wait ten years to announce their results if need be.\textsuperscript{172} More importantly, though, scientists are energized by uncertainty. Uncertainty makes new discoveries possible, and new discoveries make science "exciting," prevent it from becoming "boring," and keep it "constantly growing."\textsuperscript{173} Scientists are not bothered by uncertainty, they are "comfortable" with it, because it is an essential part of their work.\textsuperscript{174} The situation may not be the same in law, but rather than pre-judge, the speaker asks, in effect, "Is this so?"\textsuperscript{175} Her inquiry is fair-minded and generous. It is framed in open-ended terms, thereby leaving the panelists free to select and respond to those aspects of the question about which they have most to say.\textsuperscript{176} The panelists should be grateful for so friendly a question, and run with it.

RS was the first panelist to respond.

\textsuperscript{171} See \textit{id.} at lines 13-17.

\textsuperscript{172} This is true, at least, in theory. Research grants, promotions, salary increases, and other such factors often influence the speed with which even scientific work is done. The point in the text however, is that scientists do not, and given the peer review process, cannot announce conclusions until they can defend them as true. If this means they must wait for the results of research to come in, then they must wait. Judges hold themselves to a less strict standard of truth, and a timetable driven more by external needs than the results of fact investigation. For anyone who has ever worked on construction projects, this might be described as a "close enough, nail it." standard.

\textsuperscript{173} Sci, Panel Discussion, \textit{supra} p. 229, at lines 07, 14, 16. It is interesting that the audience questioner describes her position as agreeing with RS's. \textit{See id.} at lines 06, 07. RS had said only that science is not certain, that it changes its mind. \textit{See RS, Panel Discussion, supra} pp. 220-22. He had not characterized this as a positive trait—one that made science exciting, or kept it constantly growing—but just as one that made science the same as other fields of study. \textit{See id.}

\textsuperscript{174} Sci, Panel Discussion, \textit{supra} p. 229, at line 13-14. This may be another difference between science and law. It is hard to think of lawyers and judges being excited about learning that the background legal rules have just changed.

\textsuperscript{175} \textit{See id.} The audience questioner was experienced at dealing with law and lawyers, as the "when we get into a courtroom" reference indicates. \textit{Id.} at lines 17-18. Attempts to explain law's stance on uncertainty to her were unsatisfactory until then as she explained later. It is to her credit that she remained optimistic about the prospects of getting an answer to her question, and this optimism comes across in her tone.

\textsuperscript{176} In effect, the question says "discuss uncertainty in law."
RS:

(66) Let me try to give you two quick ones. I can tell you
(67) that I think the problems with uncertainty are based on
(68) two kinds of misconceptions. One, BB alluded to very
(69) correctly [in his panel presentation], when he said
(70) lawyers, when they do legal scientific research, like to
(71) look up legal cases, like the law determined that
(72) Bendectin didn’t cause this, or asbestos cause that, and
(73) so the law has this kind of pretending certainty. So
(74) for instance, the best example for those of you who
(75) aren’t practicing is, in every state there is a
(76) formulation that goes something like “to a reasonable
(77) degree of medical certainty, do you think . . . “ And
(78) every doctor that I’ve ever known says, “aaaaah . . .
(79) they didn’t teach me that in medical school, what does
(80) that mean?” And most lawyers will tell you that the
(81) answer is, “well . . . just think like a doctor and, say
(82) if you were making a decision as a doctor, don’t put on
(83) any other hat, what’s your best opinion, what’s your
(84) best guess as a doctor, what would you do?” Because as I
(85) indicated in my talk, the doctors many times don’t make
(86) purely decisions based on science. So one problem is
(87) that the lawyers have problems with this idea of
(88) certainty, they want to think that there’s a higher
(89) degree of certainty. Now the other problem is that the
(90) scientists . . . I want to take issue with what you [the
(91) questioner] said. I don’t think scientists . . .
(92) they’re split about uncertainty. Now let me tell you
(93) how they’re split. Scientists, when the doors close,
(94) all recognize the uncertainty that you recognize. But
(95) publicly, they like to take the position that there is a
(96) higher degree of certainty [BB, another lawyer panelist,
(97) interrupts here to add, “and especially in the
(98) courtroom, which is another way this information gets
(99) distorted when it gets into court”]. Doctors like to
(100) pretend that everything they do is scientific. They
like to pretend that they understand statistics when
most doctors don't have a clue about statistics, and
what the various concepts mean. So the problem is that
the scientists are kind of split as to how they feel
about certainty. Now that further reproduces itself in
the philosophy of science where there's sort of a big
split. When you look at the philosophy of science
there's two sorts of major trends, the majority
twentieth century trend which says that science is
somewhat subjective, you know, and not reproducible.
That's right. You frown [directed at the audience]
because Karl Popper, in the philosophy
field, the philosophers think that Karl Popper and the
reproducibility theory is kind of a joke. The problem is
that the idea appeals very much to scientists because it
makes it sound like what they do is certain, and they
cling to Karl Popper's notion that science is this kind
of absolute certainty, and you can prove it. Whereas
the philosophers don't. So if you go into the
philosophy of science department in any school you will
say, the people who believe in uncertainty would be
people who are primarily philosophers, and the people
who believe more in certainty will be primarily
scientists, and my view is that it's not because
scientists really produce certainty, but because it's a
more flattering view of what science is, and it's more
appealing to them.\footnote{RS, Panel Discussion at the 1997 Ward, Kershaw and Minton Environmental Symposium (Apr. 11, 1997) (transcript on file with author).}

There is enough in RS's response to provoke almost everyone,
but perhaps the best place to begin is with what the response does
not do. For all that he says, and it is considerable, RS never
answers the audience member's question. Rather than describe
how judges and lawyers decide what to do when they are uncertain,
explain how legal decisions based on incomplete or inaccurate
information could still be legitimate, or respond to some other such
interpretation of the speaker’s question, RS uses the occasion of the
question to elaborate on his earlier description of the misconception
of certainty problem in science.178 Either he is preoccupied with
the belief that scientists pretend to know more than they do, or he
thinks that the speaker somehow has defended this practice in her
question.

RS begins by criticizing lawyers for having a “kind of pretending
certainty,” when making legal claims about scientific matters, but
never says what he means by that interesting, though far from self-
evident, phrase.179 Do lawyers engage in conscious pretense, or are
they fooled themselves? Do they have legitimate reasons to
pretend, or is pretending a self-aggrandizing move? Is everyone in
the legal system in on the pretense, or just judges (or lawyers)? In
what forum is the pretense most commonly expressed, judicial
decisions, conversations with clients, witness testimony, arguments
to courts, bar association speeches, all of the above, or what? RS’s
principal example of “pretending certainty,” taken from the law of
evidence, is the “reasonable degree of medical (scientific)
certainty” standard which governs the presentation of expert
testimony to courts.180 It is not clear, however, how this standard
supports a claim about legal pretense.181 The “degree of certainty”
expression is admittedly borderline oxymoronic. There are not
degrees of certainty; something is either certain or uncertain. On
the other hand, there are degrees of probability, which is what law
and RS no doubt have in mind.182 It is also a little vague with

179. Id. at line 73.
180. Id. at lines 73, 76.
181. He uses the example of “legal cases” as authority in disputes involving
complicated science issues. Id. at line 71. The Bendectin case is given as an
The case is not, by itself, evidence of pretense. Lawyers who failed to consult or
understand science materials in Daubert or similar lawsuits may be guilty of
shoddy work, even malpractice, but unless they also hold themselves out as
science experts, they are not guilty of pretending to be anything other than what
they are.

182. The most famous “degree of” example is the expression “exactly the
same.” There are not degrees of sameness, something either is the same as
something else (in which case “the same” suffices), or it is not. Such expressions
respect to the issue of whose understanding of reasonableness controls. Yet a vague, oxymoronic expression, by itself, is not evidence of pretense, principally because it does not communicate any single clear meaning, pretended or otherwise. RS’s attempts to clarify the expression through a series of paraphrases, “think like a doctor,” give “your best opinion,” or “your best guess,” say “what would you do,” not only do not resolve the vagueness problem, they add to it.183

In the second and more extensive part of his comments, RS leaves law behind and takes on scientists. Here, he is not just confusing, he is combative. As before, RS seems set on making a pre-planned point rather than responding to what was asked. His main point is that scientists are dishonest with respect to the issue of uncertainty. In private, “when the doors close,” scientists admit that their work is filled with uncertainty, but when they go public, so to speak, all doubt is gone.184 Then, they are as authoritarian and as dogmatic as anyone.185 RS describes this point as taking issue with the speaker, but it is hard to see how. The speaker asked a question about uncertainty in law, and to the extent that she said anything about uncertainty in science, it was only to say that uncertainty is what makes science exciting. RS may hear her question as a defense of scientific deception, but if so, it is hard to see how that is a result of anything she said. RS seems primed to argue more than to respond, and the questioner appears simply to be in the wrong place at the wrong time. She has become RS’s proxy for his beliefs about scientists.

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183. RS, Panel Discussions, supra p. 232, at lines 80-84. For example, is it an opinion, a guess, or a personal preference, that is sought?
184. Id. at line 93.
185. See id. at lines 93-95. Paul Meier has described this as “aggrandizement,” that is, the tendency of experts to give definitive rather than qualified answers, to de-emphasize the importance of other schools of thought, and to exaggerate the significance of their own inferences. See Paul Meier, Damned Liars and Expert Witnesses, 81 J. Amer. Stat. Ass’n 273 (1986).
RS’s argument against scientists consists of a gratuitous slam that “doctors don’t have a clue about statistics;”\(^{186}\) a personal libel that “philosophers think Karl Popper ... is a kind of joke;”\(^{187}\) some amateur psychologizing that scientists believe in the certainty of science “because it’s a more flattering view of what science is;”\(^{188}\) and a breathless oversimplification of twentieth century philosophy of science as “science is somewhat subjective, you know, not reproducible.”\(^{189}\) It is not surprising that the questioner is somewhat dismayed. It is reasonable to feel at least a little upset when called narcissistic, duplicitous, and the unwitting captive of a philosophically bankrupt view.\(^{190}\) She also might wonder why RS is trying so hard to re-direct her attention from law to science, why he chooses to challenge her question rather than engage it, why he resorts to gratuitous ridicule and \emph{ad hominem} attack, why he summarizes a complicated body of scholarship in terms so oversimplified as to be impossible to confront, and why he filibusters by taking four minutes to respond to what had the potential to be the beginning of an extended conversation about the role of uncertainty in law. These patterns will appear time and time again, in one form or another, in most of the excerpts reproduced in this discussion.

BB was the next to respond to the speaker’s question. He spoke immediately after RS had finished and before the questioner from the audience was given a chance to respond.

BB:

(01) Scientists, by and large, have not been very reflective
(02) about what makes their work scientific, what it is
(03) that constitutes scientific validity. Most scientists
(04) get into some, I don’t want to say narrow specialty, but
(05) they get into their field and they sort of develop a

\(^{186}\) RS, Panel Discussion, \emph{supra} p. 233, at lines 101-102.
\(^{187}\) \textit{Id.} at lines 111-13. For a better understanding of the treatment of Popper, see \textit{Newton}, \emph{supra} note 13, at 156-58.
\(^{188}\) RS, Panel Discussion, \emph{supra} p. 233, at lines 124-25.
\(^{189}\) \textit{Id.} at p. 233, lines 103-11. For a more nuanced description of the various schools of thought within the philosophy of science, see Galison, \emph{supra} note 60, at 1.
\(^{190}\) See RS, Panel Discussion, \emph{supra} p. 233, at lines 111-26.
(06) sense of what goes on in that field, and they'll
(07) look at something and they'll say, "Well, that's valid
(08) because that's what we normally do." And then they'll
(09) look at something and say it isn't because it's not
(10) what we normally do. And they don't really give much
(11) thought to the philosophical basis, the logical basis,
(12) for what makes something valid or not. So scientists
(13) in some ways are the worst people to consult about the
(14) philosophy of science. While agreeing with what RS says
(15) about there being some intrinsic uncertainty, I think
(16) the important point to bear in mind is that philosophers
(17) of science, and scientists who have been reflective
(18) about this, will tell you that there are some things
(19) more certain than others. You know, we don't expect to
(20) wake up tomorrow and find that the world is flat. Are
(21) we absolutely certain that the earth is a globe, a sort
(22) of a pear-shaped globe? Well, yeah, we're pretty darn
(23) certain about that. Are we pretty certain that the
(24) earth revolves around the sun and not vice versa, as
(25) people used to think? Yeah, we're pretty darn certain
(26) about that. Are we absolutely certain? No, because
(27) nothing in science is absolutely certain. But there are
(28) certainly degrees of certainty, and what we ought to
(29) require of scientific evidence is that it be certain
(30) enough for the legal policy purposes at issue in a case.191

BB also fails to answer the speaker's question about the role of
uncertainty in law, and in his own way, continues RS's assault on
science. He makes three points: (1) because scientists are not very
reflective about what they do "[they] are the worst people to
consult" about "scientific validity"; (2) there are degrees of
certainty in science, with some scientific conclusions being more
certain than others; and (3) the legal system does not demand proof
of absolute certainty from scientific evidence, just proof that is

191. BB, Panel Discussion at the 1997 Ward, Kershaw and Minton
Environmental Symposium (Apr. 11, 1997) (transcript on file with author).
"certain enough." Even each of these points could cause the scientists in the audience to doubt BB's responsiveness, his willingness or perhaps ability to confront the difficult question about uncertainty in law and his understanding of science.

BB's first point, a criticism of scientists as expert witnesses, has two parts. The first says that most scientists are not self-conscious about how they do science, that they do not think about the "logical" or "philosophical basis" of science, as BB puts it. This is a fairly feisty way to put what, in fact, may not be a very telling point, even if true. In one sense, all professionals are not routinely self-conscious about the philosophical basis of their work. On the contrary, such self-consciousness may even be a necessary condition for good performance, even for lawyers. Take, for example, the lawyer-panelists' comments in the excerpts reproduced here. If these remarks are representative, and there is no reason to believe they are not, lawyers turn out to be not very good people to consult about the role of uncertainty in law. For example, while RS, BB, and AR may be fine lawyers overall, among the best nationally in their fields, throughout this discussion they never explain how law makes its peace with uncertainty in the legal decision process. They discuss uncertainty generally, and its role in science, but they never do the same for uncertainty in law, even after being asked. It is not likely that they lack interest in the topic or are unwilling to discuss it. They committed to such a discussion implicitly when they agreed to participate in the conference and professed their interest in the topic many times over during the course of the proceedings. Like the scientists they criticize, however, these lawyer-panelists may be so socialized into

192. BB, Panel Discussion, supra pp. 236-37, at lines 01-02, 13-14, 26-30.
193. BB, Panel Discussion, supra p. 237, at line 11.
194. In fact, whole bodies of scholarship have been built upon the assumption that this is true. For example, see the cognitive dissonance based work of Chris Argyris and Donald Schön, whose central analytical move is to point out inconsistencies between what people say they want to do, and what they do, what Argyris and Schön call the inconsistency between "espoused theory," and "theory in use." CHRIS ARGYRIS & DONALD SCHÖN, THEORY IN PRACTICE: INCREASING PROFESSIONAL EFFECTIVENESS (1974).
their professional practice that it is difficult, if not impossible, for them to step outside of that practice and describe it in terms that someone not so immersed can understand. The problem may not be with scientists or lawyers per se, but with the way professionals are trained to think about and operate in a professional role. If BB does not see the problem in these more general terms, there is some question about whether he can help analyze and resolve it.

BB's second point is his logical leap from the idea that scientists are not self-conscious about their work, to the conclusion that they are "the worst" people to consult on issues of scientific validity. Scientists form opinions on the validity of work, he asserts, based on whether the work looks like "what [scientists] normally do." Nevertheless, he continues, they cannot give any more than a "take my word for it" kind of reason when asked to explain why the conclusions they reach are credible. This leaves courts with the unsatisfying choice of either delegating scientific fact-finding to scientists, or rejecting testimony which may be true. Like many of the panelist responses during the question and answer session, there is a grain of truth in what BB says. Unfortunately, the problem of getting scientists to explain conclusions in sufficient detail may have as much to do with the way scientists are questioned about validity judgments as it does with the way they make those judgments in the first instance.

Scientists are not a special case when it comes to explaining expert judgment. Most experts are not self-conscious about the way in which they work, largely because thinking about how one performs complicated work while simultaneously performing it, slows work to a crawl and often makes it less productive. A homely but easily understood example will illustrate this idea. A golf swing is made up of many individual pieces, requiring the sequenced coordination of different parts of the body. Yet, for a swing to be successful it must be understood as a single, simple movement. The individual parts are practiced separately as the

196. Id. at lines 13-14.
197. Id. at lines 07-10.
198. Id. at lines 03-12.
199. To keep the analogy to science in the forefront, think of a golfer's swing as her or his "method" for doing golf.
swing is learned, but gradually they blend into one connected, cohesive movement as the swing is put together and made automatic over time. Once the swing is mastered, a golfer is no longer conscious of its individual parts each time he or she swings a club. This does not mean that a golfer cannot explain the swing if questioned about it. Yet, it does mean that unless a questioner knows something about how a golf swing is constructed, and is able to go more than half-way in drawing the golfer out, the golfer’s description will likely be too cryptic for some listeners to understand and will strike others as gibberish. It should not be surprising, then, that the same thing would be true about the more complicated phenomenon of scientists explaining science. Getting experts to understand and explain the tacit knowledge on which their judgments are based is a difficult task, for which no simple solution exists. It does seem clear, though, that asking someone who is not an expert to explain the judgment process does not move us closer to a solution. Who but someone who has actually done science could know fully what is involved in the process when it is done well? 200 Suggesting that scientists are not good people to ask

200. BB’s comments may have inadvertently rubbed a longstanding wound. Many scientists doubt the ability of the meta-critics (historians, philosophers, sociologists, and rhetoricians) of science to understand science accurately and to evaluate it fairly. See, e.g., the following expression of that sentiment in Max Perutz, “The Pioneer Defended,” Review of GERALD A. GEISON, THE PRIVATE SCIENCE OF LOUIS PASTEUR, in N.Y. REV. OF BOOKS, Dec. 21, 1995, at 54, 58. According to Perutz:

...it is now a commonplace among historians and sociologists of science that science, no less than any other form of culture, depends on rhetorical skills. I have known scientists who possessed great rhetorical skills which failed to conceal the shallowness of their research from their peers. On the other hand, Alexander Fleming’s lectures put everyone to sleep, while his discovery of penicillin made him one of this century’s most famous scientists. Good research needs no rhetoric, only clarity. The entire approach emphasizing “relative” truth seems to me a piece of humbug masquerading as an academic discipline; it pretends that its practitioners can set themselves up as judges over scientists whose science they fail to understand.

Id. at 54, 58; see also NEWTON, supra note 13, at 29-44 (discussing the “hubris of sociologists”); Weinberg, supra note 68, at 20 (stating that scientific “consensus is forced on us by nature itself, not by some orthodox scientific establishment; as Rees says, ‘Philosophers of science would be surprised at how many astronomers are eager rather than reluctant to join a revolutionary bandwagon,’” quoting
about science, without more, strikes most scientists as a strange thing to say.

BB’s third point, that there are degrees of certainty in science, that nothing is “absolutely certain,”201 is no doubt correct if one defines certainty to mean some probability of being true. A probability of anything can be strong or weak. While this is different from the meaning given the term “certain” in ordinary language—usually “inevitable,” “not to be doubted,” “fixed,” or “definite,”202—it is not different enough to be worth quibbling about.203 BB’s use of flat world and geocentric orbit examples may be another matter.204 The simpleness of these examples may be a little insulting given the sophistication of the audience and may cause some of his listeners to wonder how deep his knowledge of science truly runs. BB sounds a little like a non-lawyer trying to demonstrate a command of First Amendment doctrine by saying that there is no constitutional right to shout fire in a crowded theater.205 The point is true enough if properly


Richard Lewontin writes:

I share ... the belief that to write sensibly about science in-the-large one must be engaged daily in doing science in-the-small. As science is done, at least in biology, it is a social activity in which professors, professional visitors, postdoctoral fellows, and graduate students are in constant interaction in a shared physical and intellectual space. A loss of contact with this activity means, however hard one may try, the loss of contact with the present state of a science and the loss of the material basis on which science can be evaluated. People who stop doing science must soon stop talking about it.

Lewontin, supra at 59. Compare the sharper comment by Niels Bohr. “It is hopeless to have any kind of understanding between scientists and philosophers directly. All that philosophers have ever written is pure drivel.” ABRAHAM PAIS, NIELS BOHR’S TIMES IN PHYSICS, PHILOSOPHY, AND POLITY 421 (1991).


203. Earlier I disputed the idea that there could be “degrees of” certainty, see supra note 182, and while I still think that view is correct, it is only a definitional point and thus not worth pressing. There are serious problems with BB’s statement even giving him his definition of certainty.

204. BB, Panel Discussion, supra p. 237, at lines 19-23.

205. Use of the adjective “crowded” is the most obvious give away. As is well
understood, but someone truly knowledgeable in First Amendment doctrines would likely not use such a hackneyed illustration. These problems notwithstanding, this is the least controversial of BB’s three points.

BB’s attempt to clarify the reasonable degree of certainty standard for scientific testimony is another matter. The reasonable certainty standard, as we have seen, is frustrating to scientists and doctors. It is not something they study in graduate or medical school, and as an ordinary language expression, it is difficult to parse. BB attempts to solve these problems by paraphrasing the standard, presumably to put it in clearer language. Evidence is reasonably certain, he says, when it is “certain enough for the legal policy purposes at issue in a case.” However, this paraphrase does not seem to improve much upon the original articulation of the standard, and it may make it worse. For example, how much evidence is enough? What are legal policy purposes? When is something certain enough? These questions may have precise answers to BB (and other lawyers who work in the field), because he has read dozens of cases (or more) applying the standard, and has seen what quantities and kinds of evidence courts accept as adequate. He can describe a spectrum of expert scientific testimony and say where on that spectrum any present testimony would fit. Yet, this paraphrase, and others like it, is insider shorthand, and like all such shorthand, it is unintelligible to those who are not insiders no matter how expert or sophisticated they otherwise might be. To say that testimony must be certain enough is another way of saying “certain” according to the judgment of an

known among constitutional law professors, Justice Holmes’ original phrase referred only to a theater, and made no mention of how many patrons were in attendance. See Schenck v. United States, 249 U.S. 47 (1919). “The most stringent protection of free speech would not protect a man in falsely shouting fire in a theater and causing a panic.” Id. at 52. The expression seems to bear repeating every now and then, as even Justices of the Supreme Court have been known to get it wrong. See, e.g., R.A.V. v. City of St. Paul, 505 U.S. 377, 399 (1992) (reasoning that “... the Court has held that the individual who falsely shouts ‘fire’ in a crowded theater may not claim the protection of the First Amendment.”). “Crowded theater” is probably a better way to make Holmes’ point, of course, but it is not the way Holmes made it.

206. See supra note 182 and accompanying text.

207. BB, Panel Discussion, supra p. 237, at lines 29-30.
expert lawyer working in the field, but that is just what scientists are not.

While only half as long as RS's comments, and expressed in more temperate language, BB's remarks have many of the same qualities. Like RS, he fails to answer the speaker's question, criticizes science rather than explains law, discusses science in overdrawn and slightly caricatured terms, and speaks with confidence and assurance where tentativeness and qualification would have been more appropriate. He looks more like a person preaching to the choir than someone carrying on a conversation with critics. He gives no indication that he understands the truly different perspective of his scientist questioner, and makes no effort to enter into her world.

When BB finished, AR responded, speaking over RS who had begun to make additional comments, again before the questioner from the audience was given a chance to speak.

AR:

(01) I'd like to add one more thing on this question. First
(02) of all, first, the person who asked the question is in a
(03) position where getting the answer right, I think, is
(04) really important because you [to the person who asked
(05) the question] work in this area. You've got a project
(06) that's involved in this subject, and I am concerned that
(07) the question that you asked, I think, reflects a
(08) misunderstanding of the intersection between law and
(09) science in the courtroom, and it's I think really
(10) important, particularly for the project you're
(11) working on [At this point another member of the
(12) audience, who was also a scientist and a participant on
(13) the next panel, rose to ask a question. He began by
(14) saying "If I may . . .," but AR continued to speak over
(15) him.], and let me say what I think that is. In the
(16) courtroom the goal is to find out what happened in a
(17) particular event. The role of the scientist is to come

208. This is all the more remarkable given the fact that he follows RS in responding and thus has had more time to think about what to say.
(18) and offer an opinion. The role of the jury or the judge
(19) is not to decide if the scientist, on a scientific
(20) basis, is right or wrong. That would require us to do
(21) something that only scientists could do. Their job is
(22) to decide whether or not the scientist’s expression of
(23) that opinion is credible or not credible. If it’s
(24) credible, they accept it. If it’s not credible, they
(25) don’t. Now the scientist who comes to the courtroom
(26) comes with the varying degrees of certainty that BB
(27) talked about. Is the world flat or is the world round?
(28) There’s a lot more certainty to that than the question,
(29) does trichlorethylene cause leukemia? But in my
(30) judgment, both of those reach 51%, both of those are at
(31) that level, and I’ve got an expert here in the room who
(32) can testify to that. And that expert is going to go up
(33) against another expert who’s going to say, “Yes, I think
(34) it’s certain that the world is round and not flat, but I
(35) don’t think that it’s 51% likely, and they’re going to
(36) point to various scientific evidence to support it. The
(37) only criteria that the law lays down for those
(38) scientists is that when they make their judgment, and
(39) that’s what it is, a judgment, that they do it using a
(40) scientific methodology. You can’t say “I had a dream
(41) that trichlorethylene causes leukemia, and that’s the
(42) basis for my opinion.” You can’t say “I think the world
(43) is round because I had a dream.” You have to have some
(44) scientific evidence. You have to marshal it in a
(45) scientific way. You have to use the scientific
(46) methodology. But in the last analysis it’s judgment.
(47) All the court is doing is providing a forum for
(48) scientifically reliable and methodologically sound
(49) judgments to be expressed. That’s it. They let the
(50) decision-maker decide. And if you’re frustrated because
(51) sometimes the jury may conclude that the credible
(52) scientist is the one whose position is actually wrong,
(53) ask yourself this question: “How many auto accident
cases are decided on the basis of whether or not the light was red or green, and an eyewitness says it was red, and it was actually green? It happens all the time. It's inherent in the nature of the process.

People view something and they see it wrong. We shouldn't be more upset because a scientist may be wrong in testifying to it and a jury believes him. Scientists have been testifying in courts and they've been wrong a lot.

AR responds differently from the first two panelists. He does not criticize science directly, challenge the accuracy of the questioner's preconceptions, or question Sci's capacity for self-reflection, at least not explicitly. Instead, he tries to answer the audience member's question about uncertainty in law by explaining how legal decision making is different from doing science. In the end, however, AR he does not appear to be any more helpful than his more overtly adversarial colleagues. His remarks are confusing on key points and perhaps a little condescending. They signal that he might not be completely trustworthy, deprecate the importance of the uncertainty issue itself, and in the end may also beg the question.

AR begins by stating that it is important that the questioner "get the answer right," since she is working on a "project that's involved in this subject." This is a confusing thing to say. He may be speaking indirectly to RS and BB, suggesting that they have not gotten the answer right and that he, AR, will correct what they have said. If so, RS and BB might legitimately take offense at this. Alternatively, he may be speaking directly to the questioner, saying that it is important that she get the answer right, but why he would think she needed to be reminded of this is not at all clear. For example, when would one not try to get the answer right? The questioner may also have thought it a little ironic that a lawyer would lecture a scientist on the importance of accuracy, given the subject of the discussion, and see AR's response as just another,

210. Id. at pp. 243, lines 03-06.
albeit more sophisticated, example of how lawyers attack when they are confronted.

AR next characterizes the speaker’s question as based on a “misunderstanding,” though he does not identify what the speaker has misunderstood. The questioner asked only that the panelists discuss the role of uncertainty in law, without expressing or even suggesting a view about how law should accommodate uncertainty. To correct the speaker’s alleged misunderstanding, however, AR explains the nature of the courtroom process. The overall goal “in the courtroom,” he says, is to “find out what happened in a particular event.” Scientists offer “opinions” toward that end, judges and juries decide whether the scientists’ opinions are “credible,” and courts “accept” the opinions when they are “credible.” Yet, this description could raise more questions in the audience member’s mind than it answers.

For example, why does the legal system submit the analysis of experts to non-experts for review and evaluation? This confuses many scientists because to them it does not make much sense. How could a person judge the accuracy of something he or she did not understand? Scientists try to find out what happened as much as judges, yet they do not authorize someone who knows nothing about science to approve or reject their conclusions. If scientific inquiry is scientific inquiry, whoever does it, why does law use such an unscientific method? Second, how does a court determine when a scientist’s opinion is credible? Is it whenever the opinion is sincerely held? If so, why would a court care about sincerity? What does sincerity have to do with accuracy? Or is it whenever the opinion is worthy of being believed? If so, how does a court tell when an opinion is worth believing without first determining if it is correct, something AR claims only scientists can do. Finally, what does it mean to say that a court accepts a scientist’s opinion as credible? Does the court conduct an independent investigation by engaging in science itself so to speak, before passing judgment on the opinion, or does it simply ratify the

211. See id. at p. 243, lines 05-09.
212. Id. at p. 243-44, lines 15-17.
213. Id. at p. 244, lines 17-18, 21-25.
214. See id. at lines 18-21.
opinion, delegating the fact-finding task to the scientist in the process? To the extent that there is a misunderstanding present, it seems to be more AR’s than the questioner’s.

AR then describes the task of credibility assessment in more detail. Experts testify for and against particular scientific conclusions, he says, based on judgments they make about relevant evidence, and such testimony is credible to the extent it is based on reliable scientific methodology. AR is not explicit about what makes a methodology scientifically reliable, but he does offer some indirect hints. For example, science must be based on real data. A scientist cannot say “I had a dream . . . and that’s the basis for my opinion.” While no one would dispute this, the risk in this illustration is that it encourages scientists to think that courts treat all scientific testimony as credible as long as it is based on some sort of data and that the only thing a scientist cannot do in court is make up evidence. This reinforces the widespread concern among scientists that courts do not distinguish between good and bad science, that all they do is distinguish between blatantly incompetent hacks and the rest of the pack. On this view, legal decision-making about science is seen as a kind of a black box process in which all but the most blatant errors go unnoticed and uncorrected.

AR also explains the process of assessing credibility in terms of the burden of persuasion. While there are degrees of certainty, he says, repeating BB’s flat world example, a 51% chance of something being true is enough for a court to conclude that


216. See AR, Panel Discussion, p. 244, at lines 36-40, 46-49.

217. Id. at lines 40-42.
testimony is credible.\textsuperscript{218} Presumably, this is intended to describe the minimally acceptable case under a preponderance of the evidence standard, but if the questioner (and scientists in the audience generally), does not already understand this standard, which seems to be the case, AR’s statement raises additional concerns. For example, how does the legal system determine whether the probability of something being true can be measured to within one or two percentage points? Why is a fifty-one percent chance of something being true strong enough? Does AR mean to suggest that the evidence for the world being round is as convincing as the evidence for trichlorethylene causing leukemia, or that he might be able to convince a court of that conclusion, or what’s worse, would try?\textsuperscript{219} If yes, is anything else he says believable? If no, does he unwittingly demonstrate how lawyers argue almost automatically for debatable, self-interested propositions by equating them with basic truths? Finally, is AR saying that he could get an expert to testify that there was \emph{not} a 51\% chance of the world being round?\textsuperscript{220} If so, is this insulting? Does it reinforce the widely held stereotype that lawyers do not respect scientists or scientific findings, because they believe scientists will testify to anything as long as they are paid well enough?

These are questions about how the legal system, through lawyers, investigates scientific facts and ascertains scientific truths. Such questions ask for justification or explanation, not more elaborate description, of how the legal system works. Unfortunately, AR offers only the latter. In the end, he takes the legal world view as a given, and conducts his part of the conversation as if the questioner and everyone else in the audience accepted the intellectual premises underlying that view. He needs to explain why credibility is enough of a standard for accepting scientific testimony as true and not merely describe how the credibility of particular statements is established. The questioner is a scientist, not a lawyer. She comes to the topic from a different

\begin{footnotesize}
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\item \textsuperscript{218} See id. at lines 25-40.
\item \textsuperscript{219} "... both of those reach 51\%." \textit{Id.} at line 30.
\item \textsuperscript{220} See id. at lines 29-32.
\end{itemize}
\end{footnotesize}
perspective, accustomed to using different methodologies, pursuing different intellectual goals, and holding herself to different analytical standards. She does not begin committed to the legal world view, and this is one of the strengths she brings to the conversation. To tacitly demand that she carry on the conversation as if she were a lawyer, accepting what lawyers accept, is to destroy the very possibility of inter-disciplinary learning.

AR’s last point is a kind of “everyone does it” justification for not being concerned about the risk of error in legal fact-finding. Non-scientist witnesses routinely make factual mistakes in testifying, getting the color of the traffic lights wrong and making other similar mistakes.\(^{221}\) AR asserts that “[i]t happens all the time.”\(^{222}\) Cases are regularly decided on the basis of such errors and nothing can be done about it. Error is “inherent in the nature of the process . . . . We shouldn’t be more upset because a scientist may be wrong . . . .”\(^{223}\) and he adds, “[s]cientists . . . have been wrong a lot.”\(^{224}\) To take the last point first, it is not clear how AR knows that scientists have been wrong a lot. Determining whether science is right or wrong, by AR’s own admission, is not something judges and juries can do. They simply decide whether testimony is credible, whether it is arrived at through the use of reliable scientific methods.\(^{225}\) If only scientists can tell whether science is right or wrong,\(^{226}\) and AR is not a scientist, one wonders how he is able to come to this conclusion.

Conceding that scientists have been “wrong a lot,” it is still not clear how the existence of error in non-scientific testimony justifies or excuses error by scientists. With eyewitness accounts, there is often no way to tell whether testimony is accurate. The eyewitness may have been the only one present, there may be no physical evidence to corroborate or undercut the testimony, and there are no reliable standards for deciding when an eyewitness is telling the

\(^{221}\) \(\text{See id. at p. 245, lines 53-57.}\)
\(^{222}\) \(\text{Id. at lines 56-57.}\)
\(^{223}\) \(\text{Id. at lines 57-59.}\)
\(^{224}\) \(\text{Id. at lines 60-62.}\)
\(^{225}\) \(\text{See id. at p. 244, lines 20-23, 36-40.}\)
\(^{226}\) \(\text{See id. at lines 18-21.}\)
truth.\textsuperscript{227} In science, most of these conditions can be reversed.

Science is rarely done in private; other investigators abound, data on which scientific judgments are based is invariably public and plentiful, and there are well established standards such as publication, peer review, falsification, and replication for conducting scientific inquiry. The legal system may have to tolerate eyewitness error for reasons rooted in democratic and constitutional theory. Citizens have a right to provide and receive testimony by virtue of their status as citizens, wholly irrespective of their skill at observation, powers of recall, or facility at testifying. There is, however, no equivalent claim to be made for expert scientific testimony. The latter is justified to the extent that it contributes to the court’s effort to understand what really happened. When such testimony is inaccurate, it does not make such a contribution. With scientific testimony, getting it right is the whole ball game. AR’s assurance that there is no need to worry because non-scientists regularly make mistakes is not comforting.

When AR finished, BB made some additional comments, again, before the speaker from the audience was given a chance to respond.

BB:

(31) In exploring whether or not the scientist is wrong,
(32) whether it be in front of the court where you’re seeking
(33) to withhold the evidence, or in front of the jury, one
(34) of the areas that you want to explore is whether or not
(35) the scientist went about reaching the conclusion in the
(36) way that scientists normally go about reaching
(37) conclusions, and I think AR essentially agrees with me
(38) about that. Which means that you are going to be
(39) calling upon non-scientists to assess scientific

\textsuperscript{227} See Olin G. Wellborn, Demeanor, 76 CORNELL L. REV. 1075 (1991); Jeremy A. Blumenthal, A Wipe of the Hands, A Lick of the Lips: The Validity of Demeanor Evidence in Assessing Witness Credibility, 72 NEB. L. REV. 1157 (1993). The problem of eyewitness distortion is exacerbated by law’s use of an adversarial method of witness examination. See JASANOFF, supra note 1, at 52-55 (stating that “Critics ... worry that the nonepistemological determinants of credibility may carry the greatest weight with lay juries and judges.”).
validity. There is no way of avoiding that issue. And, if you are a lawyer defending a case like this, and you think that something isn’t scientifically valid, I don’t think there is any way that you can competently tell the story to the jury of why it isn’t valid unless you’ve figured it out for yourself. You can’t just rely on telling the story through your own experts. You have to know how to cross-examine the plaintiffs’ expert, in my case, or the defense expert if you’re trying to keep defense testimony out. And I further think, especially in light of Daubert, where we have an increased focus on whether or not science is really scientific, plaintiffs’ lawyers need to understand their cases better too.

Here BB makes the point that lawyers must understand the science behind the scientific evidence they offer if they are to make effective use of such evidence and cross-examine the other side’s attempts to rebut it. This is one of BB’s favorite points, as he makes it several times during his panel presentation and his response to questions. Unlike AR, however, he recognizes that courts must evaluate the accuracy of scientific evidence before deciding how much weight to give it. As he says, “There is no way of avoiding that issue.” He also seems to understand that methodology does not exist disembodied from result, in science or in any other field, and that it is not possible to separate form from content in any non-reductionist way. Reduced to its essentials then, BB’s statement says, in effect, that one cannot possibly help another understand something if one does not already understand it oneself. Sometimes an unknowing remark inadvertently triggers a new insight or produces a new level of understanding, but for the most part only understanding begets understanding. While not

228. See Daubert, supra note 181.
230. See id. at p. 251, lines 40-49.
231. See id. at pp. 250-51, lines 38-45.
232. Id. at line 40.
controversial, this is an important point and one well worth making. Lawyers, particularly trial lawyers, tend to be too confident of their ability to criticize complicated bodies of substantive knowledge after having studied them for only a brief period of time, and anything which tempers this propensity to believe in the possibility of instant expertise is all for the good.

Yet, the most salient feature of BB’s comment is the extent to which it has broken all contact with the speaker’s original question about uncertainty in law. The lawyers on the panel have spoken for a little over nine minutes since the uncertainty question was first asked, and yet not only have they not answered the question, they have not even begun to answer it. Moreover, during all of this time the speaker has not been given a chance to clarify her question, to press for an answer, nor allowed to participate in the conversation in any other way. The lawyers have used the nine minutes to deliver a lecture to the speaker and the scientists in the audience about the lack of certainty in science. Now, on the occasion of answering further, BB extends his comments to lawyers as well, making a point which, while important in the abstract, says nothing at all about uncertainty, in either science or law. While no doubt intended to be helpful, these responses had to be discouraging to anyone listening and interested in the uncertainty topic. Collectively, they teach the speaker and others who plan to ask questions, that if the lawyers have no ready response, the questioners must be prepared to wait through an extended set of didactic comments not likely to be helpful, paying close attention in the hope that something informative will be said, before having the opportunity to speak again. The message in RS, BB, and AR’s comments is that lawyers filibuster (or think out loud, if one prefers) when they do not know the answer to a question or do not want to answer it, and that a questioner must be prepared to wait

233. This is a longstanding problem, as Howard Lesnick reminds me. See, e.g., the exchange in the *Gorgias* in which Socrates notes that “orators” (read, lawyers) know nothing about the fields in which they seek to exert influence, often in contention with true experts. See *Plato, Gorgias* 459(c), (W.D. Woodhead translation), in *PLATO: THE COLLECTED DIalogues* 229, 242 (Edith Hamilton & Huntington Cairns eds., 1961). Gorgias responds: “But is not this a great comfort, Socrates, to be able without learning any other arts but this one [i.e., oratory] to prove in no way inferior to specialists.” *Id.*
through the filibuster. This is an exhausting way to converse of course and faced with such a prospect, it is not surprising that many give up on the effort, concluding that it is not worth the return.

RS then made some additional comments, again before the speaker in the audience was given the chance to respond.

RS:

(128) Let me just say one thing, ... cause I think one
(129) thing ... people talked about the policy ... in the
(130) Woburn case that was certain individuals against Grace,
(131) it's not to settle the policy answer of whether TCE
(132) caused cancer, or whether their wells were contaminated.
(133) It's to resolve a dispute between human beings and
(134) other, whether they're corporations or other people, and
(135) I think, you know ... for those of you that are
(136) lawyers, remember the first day of law school how they
(137) say to you, “Are you a citizen of Maryland?” And the
(138) answer you were taught was, “Well, it depends on for
(139) what purposes. I might be for estate tax purposes, but
(140) not for voting purposes. I might be for a driver’s
(141) license but not for certain other purposes. And so when
(142) you look at ... people seem to be all excited about
(143) the use of science, but they forget, as lawyers realize,
(144) that say, in the simplest example, in the O.J. Simpson
(145) case, the question in the criminal case was not whether
(146) or not he murdered her. It’s whether the state, in that
(147) case, proved beyond a reasonable doubt that he did. And
(148) similarly, in the civil case, a dispute between two
(149) people, whether another group proved by another
(150) standard, more likely than not, that they did. Neither
(151) one is an attempt to ultimately resolve a factual
(152) question. They’re attempts to resolve different kinds
(153) of disputes, and that’s something that we have to
(154) resolve as lawyers. And it’s ... you know, that
(155) scientists want to get all excited. This is the answer
(156) to the scientific question. It isn’t. It resolves the
(157) question, and the facts have to be before it. And all
(158) the facts determine how you resolve that dispute. 234

Read literally, this is a pretty confusing statement, full of stops and starts, asides, detours, syntax errors, ungrammatical phrasings, and the like. RS seems to begin three sentences for every one he finishes, and to develop one idea for every five he has. It may be that his mind simply outraces his ability to speak, so that he starts a new point without realizing that he has not yet finished the previous one. The effect on the listener, however, is one of conceptual overload, of being barraged with too many distinct points. It is a little like listening to the Liszt B-minor sonata for the first time (but only a little like it) and experiencing the sensation of having hundreds and hundreds of notes rain down on only two little ears.

We do not need to understand RS’s statement in conversational real time, however we have it on paper and can slow down the pace to analyze what he says. On its face, much of the statement seem unrelated to the subject of uncertainty in law, what with its references to citizenship, taxation, voting, driving, O.J. Simpson, and the differences between criminal and civil cases. 235 If we rephrase the statement to make it responsive to the original question, though, it can be seen as a variation of AR’s earlier “not to worry” response. 236 It says, in effect, that there is no need to be concerned about courts getting scientific analysis wrong because courts do not resolve issues of science. 237 Courts simply settle disputes between litigants 238 according to rules of procedure and rules of evidence which decide who wins even when it is not possible to determine what happened factually in the underlying transaction. 239

The difficulty with this paraphrase, however is that, like AR’s

235. See id. at p. 253, lines 139-40.
236. AR, Panel Discussion, supra pp. 244-45, at lines 48-59.
237. “[T]he Woburn case ... was ... not to settle the policy [question] of whether TCE caused cancer ....” RS, Panel Discussion, supra p. 253, at lines 129-32.
238. “It’s to resolve a dispute between human beings ...” Id. at lines 132-33.
239. “It’s whether the state ... proved beyond a reasonable doubt ....” Id. at lines 146-50.
response, it ducks the original question. That question asked, in part, something like “What does a court do when faced with an unresolved issue of scientific fact, such as whether TCE causes leukemia, on which the imposition or non-imposition of legal liability depends?” Does it ignore the issue altogether and base its decision on purely legal rules such as burden of proof, and presumptions, does it try to understand the issue and revert to legal rules only if confused or uncertain, does it resolve the issue by choosing one expert’s opinion over another, or what? And most importantly, why does it follow the course it does? For example, if a minimal, scientifically credible evidentiary case for causation is a precondition to liability, must not a court have some method for determining whether such a case has been made? While it need not resolve the causation issue for any purpose outside of the litigation before it, can a court be indifferent to or agnostic about that issue for purposes of imposing liability?

Seemingly, the audience questioner understands that courts resolve only legal disputes as opposed to scientific ones, but is curious about what is done when an issue of scientific fact is so intertwined with the issue of liability that resolving the scientific issue cannot be avoided. RS does not answer this question directly, though he certainly talks around it at considerable length. Like AR, he assumes the legal world view as a kind of brute fact, and then points out that within that world the concerns expressed by the questioner do not arise. Unlike AR’s, his is a longer, more garbled, more emphatic statement of the “court’s don’t do science.” However, the audience member’s question, in a sense, is “How can they not do science and still do their job when science issues are involved?” Like one giving directions to a non-native speaker who does not understand the directions the first time, RS says it again, louder.

In some settings RS’s behavior would be unambiguously strategic. For example, in the give and take of settlement negotiation, lawyers often try to overwhelm an adversary with argument, articulating too many points to be identified, understood, evaluated or rebutted individually, because they come too rapidly and too furiously. The strategy is designed to silence a listener and to have her throw up her hands in exasperation, not persuaded of
the correctness of the lawyer's arguments, but persuaded that she will not be able to convince the lawyer within the time frame of the conversation. \textsuperscript{240} Think of this as a suffocation strategy, designed to squeeze the life out of an argument by burying it under an avalanche of words. This is one of the most common lawyer argumentation strategies I have observed over the years,\textsuperscript{241} though when I describe it in these terms most lawyers deny that they use it. No doubt RS did not set out consciously to suffocate the audience member's query, but whether he did so out of habit is more difficult to determine. It may even be that he and AR cooperated in the venture. I do not mean by this that RS and AR explicitly agreed to keep talking until the questioner gave up, but just that, out of shared habit, they may have tacitly coordinated their efforts in this way.\textsuperscript{242} This seems the best explanation for the needless piling on of layers of unresponsive, critical comments, delivered in a highly excited and rapidly paced style, without providing any opportunity for the questioner to respond to, or discuss the point.

While RS's statement is the final panelist comment for the moment, it is not the last word on the subject. Another scientist in the audience, the one who tried to ask a question at the beginning of AR's response earlier\textsuperscript{243} and whom I will refer to as MZ, rose again to speak. His statement follows.

\textsuperscript{240} While a risky strategy under some circumstances, talking a point to death can be effective, even with bargainers who recognize that it is being used. Such bargainers often are faced with a version of the "reasonable" bargainer's dilemma. This occurs when the following conditions are met: 1) settlement is best for both sides, so the case should settled; 2) the speaker believes the listener is convinced by the arguments because he or she says nothing to rebut them; 3) the listener is not convinced, but cannot prove this through more passive strategies, such as refusing to change position, because there is not enough time in the negotiation for such strategies to work. Faced with this situation, a reasonable person will believe that he or she bears a disproportionate responsibility for bringing settlement about, and does this by making or accepting a more generous than usual offer.

\textsuperscript{241} See, e.g., Condlin, \textit{Cases on Both Sides}, supra note 78, at 96-125; Condlin, supra note 75, at 368-416.

\textsuperscript{242} The classic discussion of tacit coordination is THOMAS SCHELLING, \textit{STRATEGY OF CONFLICT} 54 (1960).

\textsuperscript{243} See AR, Panel Discussion, at p. 243-44, lines 11-15.
MZ:

(01) My father told me that if I wanted to have a happy life
(02) to stay away from physicians and lawyers. I think with
(03) regard to the panel, I agree with many of the things
(04) that were said, especially the truisms. I also find
(05) some of the remarks a lot of rubbish. Just like there
(06) are all kinds of lawyers, there are all kinds of
(07) scientists. Different lawyers have different
(08) qualifications, scientists have different
(09) qualifications. With regard to uncertainty, it’s very
(10) hard for people to deal with uncertainty, but one thing
(11) that’s a development in science in the twentieth
(12) century, we’ve attempted to learn how to measure
(13) uncertainty, and it’s how one measures uncertainty which
(14) is very much in dispute. Given that people agree on
(15) what uncertainty is, or the level of uncertainty, then I
(16) think it’s up to the jury or the judge to decide whether
(17) that level is high enough to come to decisions. But I
(18) do think that saying scientists do this and scientists
(19) do that really ignores how complicated issues are. But
(20) one thing is fairly clear, I think we have made lots of
(21) attempts, and it’s true in this Woburn case as well as
(22) other kinds of similar litigation, to go about how to
(23) measure uncertainty. 244

It is hard to tell which of MZ’s several points is the principal
reason he decides to speak up. He speaks slowly, in an even pitch
and in a calm, measured, and perhaps carefully controlled tone, not
giving much of a non-verbal indication of how he feels about any
of his comments. Yet his words suggest that he is piqued by, if not
angry at, the lawyers’ descriptions of science. He categorizes
“many of the things . . . [the lawyers] said” as “truisms,” or
“rubbish,”245 but he does not say which statements he objects to in
particular, or give reasons for his objections. In doing this, he may

244. MZ, Panel Discussion at The 1997 Ward, Kershaw & Minton
Environmental Symposium (Apr. 11, 1997) (transcript on file with author).
245. Id. at p. 257, lines 03-05.
simply be trying to strike a balance between registering his protest on the one hand, which he feels compelled to do, and not getting bogged down in defending it on the other, which he thinks (with good reason, after seeing the panelists’ responses to the uncertainty question), might waste valuable question and answer time.

Looked at less flatteringly, as subsequent events may warrant, however, MZ may have wanted to criticize the lawyers’ understanding of science without taking responsibility for

246. In his later panel presentation MZ often expressed points in a manner worthy of lawyers. Consider, for example, his following statement, about the general competence of courts to understand and use scientific evidence accurately, in which he is conclusory, a little condescending, and somewhat contentious. See infra at lines 21-33. The statement does not invite discussion, or make it seem as if the issue is an open one in MZ’s mind. This was perhaps the most complicated and controversial of all of the subjects raised during the conference, and one of the most important. Yet the way MZ raises it, as more of a set of conclusions to be defeated than a series of questions to be examined, all but guarantees that the ensuing discussion would produce more heat than light, and it did.

MZ: There are lots of hired guns out there who make a living being hired by industry to challenge all kinds of claims based on scientific evidence. If you have the money you can have access to people. And quite frankly, much of these studies are not perfect studies. They’re flawed. Like DO [another scientist on the third panel] said, sometimes it comes down to judgment, and if you have the funds you can get people, in a very matter of fact way, able to destroy studies just because they’re being adversarial. In fact, I think that if I was adversarial I could take either side of any study and make it look so bad in front of a jury that it would be discredited. It may not be right, but I think I could do it just because of the expertise I might bring in what goes wrong with studies. Now, I think if the Woburn study had come to court, and I understand they had gathered other evidence, I don’t believe the court and the jury would have been able to assess, really, what went on. And that brings me to concluding remarks. I think it’s a very serious matter that the courts are unable to assess very complex scientific evidence. And I think it’s important that the courts have access to experts who are objective, who can assess what the pros and cons are of any complicated study, and so advise the judge and jury. I think without this we’re going to just proceed in this very adversarial situation where people might want truth or might want justice but I don’t think it’s going to come about because of the complex nature of many of these studies, which cut across many disciplines. And I was somewhat amused this morning, in the second panel about the discussion of uncertainty by the lawyers. And I think that it might be a very good idea in the law curriculum to devote some time to discussing uncertainty. There are many kinds of uncertainty that one is likely to deal with.
explaining and defending the substance of his criticisms, thus, dismissing the lawyers’ views summarily rather than actually contesting them. If the latter is true, his strategy is worthy, one might even add characteristic, of his vision of a lawyer. He may be a kindred spirit to the lawyers on the panel without even realizing it. When one adds the “father’s advice” remark to the calculus, lawyer bashing made bipartisan by being extended to doctors, this second interpretation becomes even more plausible. Kindred spirit or not, however, such a statement does not bode well for the possibility of extended and mutually informative dialogue between MZ and the lawyers on the panel about the issue of uncertainty in law.

MZ’s second point, equally provocative but less developed than his first, has to do with evaluating the quality of scientific work. He may suggest, however obliquely, that courts should distinguish between good and bad science by looking at the qualifications of the scientists doing the work.248 The work of qualified scientists should be taken as reputable, and the work of those not so qualified should be examined more skeptically.249 It would be nice to have MZ elaborate on this concept of qualifications. Does it refer to educational background (universities attended, degrees earned, professors studied with, and the like), the location and prestige of present employment, academic or otherwise, the number and substance of scholarly publications, reputation in the field, grants awarded, prizes earned, projects or studies conducted, membership in learned societies, something else, or all of the above? However defined, it is likely to be a controversial standard because it shifts the focus in evaluating scientific testimony from the direct evidence of the scientific work itself, to the circumstantial evidence of the reputation of the scientist doing the work. Its appeal depends

247. See MZ, Panel Discussion, supra at p. 257.
248. See id.
250. The drawing of lines between good and bad work (and between good and bad workers), for claims whose factual status is still “in the making,” is described by sociologists of science as “boundary work.” JASANOFF, supra note 1, at 52-53. It is carried out by recourse to a number of social factors that go into the production of scientific knowledge, and derives its authority from a certification process resulting from a multitude of informal, often invisible, negotiations among members of relevant disciplines. See id. at 52-53.
upon the accuracy of one or the other of two highly debatable premises. Either that there is a one-for-one relationship between reputation and quality, or that reputation is an easier issue to investigate than quality, and is likely to be a good enough proxy for quality most of the time. Suggesting that a scientist’s qualifications should be dispositive is not a self-interested move on MZ’s part. His work would be reputable under either standard. Yet if he is serious about defending this standard, he owes it to his audience to give reasons. He should not, as AR did earlier, try to sneak in an argument without supporting it explicitly. Again, he may be more of a lawyer than he suspects.

In his final point, MZ returns to the topic of uncertainty in law, indicating perhaps that at least the scientists in the audience have not lost sight of what started this exchange, and what is foremost in their minds. He begins by defining the issue more precisely. It is how one measures uncertainty, not whether it exists, that is controversial. Twentieth century science has made progress on the measurement problem, but because the process is “complicated,” its final shape is still “very much in dispute.” While making it clear that he thinks the lawyers’ comments do not describe the problem accurately, he does not say how they fall short, or give examples of the complications involved in measuring uncertainty with stories from his own work. As a consequence, his manner of speaking again is hard to distinguish from that of the lawyers he criticizes, relying as it does on ex cathedra pronouncement rather than reasoned explanation. MZ has examples he might use from studies conducted for the Woburn litigation, as the next panel presentation made clear, and while he may refer obliquely to those studies in passing at the end of this statement, this reference is not detailed enough to be recognizable to anyone not already familiar with his work. If he wants to save the Woburn examples for his panel presentation, he could at least mention them at this time, and ask his listeners to wait for the details. Yet, he does not do this

250. Supra note 247.
251. See id.
252. See id.
253. See supra note 246. The panel did not disappoint. MZ described at least one new method for measuring the relationship between TCE and leukemia.
either, contenting himself instead with a one-line dismissal of the lawyers’ one-line argument. If this is an effort to out-lawyer the lawyers, he is as successful as they are in trying to out-science him.

In the end, it is never clear why MZ spoke up. Perhaps he was upset at RS’s panel remarks about uncertainty in science, believing them to be too vulgar, simplistic, and misleading to let pass without criticism. Yet, if so, he was not willing to develop his own views in enough detail so that they could be subjected to the same critical review, at least not yet, and given the manner of his subsequent panel presentation, perhaps not ever. In order to walk a line between the not fully compatible goals of criticizing RS’s comments, but not discussing these criticisms, MZ may have decided to express his objections in as cryptic a fashion as possible, so as to give those who would disagree with him very little to grab on to. Thus presented, however, and whether intended or not, his comments became just another shot across the bow in what turned into not much more than an extended inter-disciplinary stand-off. His remarks have one additional, perhaps not surprising, effect. They breathe new life into RS, as the latter almost jumps for the microphone at the close of MZ’s remarks to make the following additional statement.

RS:

(159) Well, let me just comment on that, ’cause I think
(160) Rothman’s article on epidemiology has, which is
(161) quoted in the Supreme Court case in Daubert, makes

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developed in the Woburn case, in considerable detail, although he did not claim that it could be extended to uncertainty measurement problems generally. For a write-up of the study in which the method was used, see Lagakos, supra note 118.

254. See RS, Panel Discussion, supra pp. 232-33, for RS’s initial remarks about uncertainty in science.

255. See supra note 246.

256. This is probably a reference to the amicus brief of Professor Kenneth Rothman, submitted on behalf of teachers and scholars of epidemiology, in the Daubert case. See Brief of Professor Kenneth Rothman, et al., as Amici Curiae in support of the Petitioners, in Daubert, supra note 181. Professor Rothman’s Brief is not an “article” as RS says, and it is not referred to in the Supreme Court’s opinion as he also says (the Court does refer to three other amicus briefs, out of the 22 submitted), but there is no question that Rothman is a “leading epidemiologist,” see KENNETH J. ROTHMAN, MODERN EPIDEMIOLOGY (1986), and almost certainly is the person RS has in mind.
(162) a good point. And Rothman says, and you know,
(163) people have said he’s one of the leading
(164) epidemiologists, Rothman says, that yes, we can
(165) measure uncertainty and have confidence intervals, and
(166) all sorts of techniques to measure uncertainty in
(167) general, but when it comes to an individual case did A
(168) cause B?, the answer has got to be either one way or
(169) the other. And that is a troubling thought, because in
(170) many contexts you have to come to that answer, the
(171) legal context is others. And it’s one thing to measure
(172) it in general, but in an individual case that has to
(173) have an answer, the general measurement may not fit the
(174) individual case. For instance, in law we’re taught
(175) early on in evidence that a statistical proof that,
(176) well only one of the fifteen trucks was red, does not
(177) get the defendant a summary judgment against the fact
(178) that somebody saw the red truck. That’s the difference
(179) between statistical evidence and how you resolve an
(180) individual case. There’s a paradigm there that’s sort
(181) of ineluctable. [At this point MZ spoke up from the
(182) audience, but since he did not have a microphone, what
(183) he said is not audible on the tape.] The law doesn’t
(184) accept, does it, the fact that it’s a one in ten chance
(185) that that’s the car that hit me, that therefore the
(186) plaintiff always loses. The law just doesn’t operate
(187) that way. Maybe a one in ten chance if I run naked
(188) across the street right now that nothing will happen to
(189) me. But that doesn’t prove that nothing will, or that
(190) if something does, the person isn’t responsible. So a
(191) statistical estimate, no matter how precise and how
(192) technical, doesn’t answer the question that you need
(193) answered. That’s the problem. It doesn’t answer the
(194) legal question.258

257. See Daubert, supra note 181.
258. RS, Panel Discussion at the 1997 Ward, Kershaw & Minton
RS again seems a little jumbled, both in what he says and in how he says it, probably another instance of his ability to think overtaking his ability to speak. Once again, he comes tantalizingly close to explaining the legal system’s different perspective on uncertainty. For example, he acknowledges that there are differences between scientific and legal proof, but then, just when one expects a discussion of why law follows a different path, he retreats to the safe haven of simply describing the content of legal rules. In the process, he closes down what turns out to be the conference’s last opportunities to discuss the uncertainty topic. Perhaps this was inevitable. It was late in the session, the audience seemed restless and may have been ready for lunch, or perhaps everyone had just lost hope that anything new would be said. Even the best explanation of why law acts as it does might not have registered at this point. Moreover, RS spoke excitedly, in a rapid-fire fashion, and with a great deal of force, and these are qualities which make scientists suspicious because they remind them of the “what you argue-how you argue” concern, which they mistakenly think is peculiar to law. All of this is unfortunate because RS had a number of potentially interesting things to say.

RS’s main point has to do with the difficulties of using epidemiological evidence about causation as legal proof. The problem is that epidemiological evidence is statistical and probabilistic, whereas law prefers evidence that is personal and

Environmental Symposium (Apr. 11, 1997) (transcript on file with author).
259. See id. at p. 262, lines 162-94.
261. This is just a preference. Statistical evidence can be admitted and relied on in many circumstances in law, usually to buttress a testimonial case. It just cannot be used to prove a case all by itself, and this sometimes makes the difference between winning and losing. For articles discussing the use of statistical evidence in toxic tort cases, see Ora Fred Harris, Toxic Tort Litigation and the Causation Element: Is There Any Hope of Reconciliation?, 40 SW. L.J. 909 (1989); David Rosenberg, The Causal Connection in Mass Exposure Cases: A Public Law Vision of the Tort System, 97 HARV. L. REV. 849 (1984); Glen O. Robinson, Multiple Causation in Tort Law: Reflections on the DES Cases, 68 VA. L. REV. 713 (1982); Daniel A. Farber, Toxic Causation, 71 MINN. L. REV. 1219 (1987); Troyen A. Brennan, Helping Courts with Toxic Torts: Some Proposals Regarding Alternative Methods for Presenting and Assessing Scientific Evidence in Common Law Courts, 51 U. PITT. L. REV. 1 (1989); Bert Black, A Unified Theory of Scientific Evidence, 56 FORDHAM L. REV. 595 (1988); E. Donald Elliott,
“objective.” Epidemiological evidence is about causation in the aggregate or general causation; it shows the effects of phenomena and events on large populations generally. For example, in the Woburn case, it helps answer the question of whether TCE from the Grace wells increased the incidence of leukemia in the town of Woburn. Legal proof, on the other hand, must establish causation in the individual case by showing the effects of phenomena and events on particular persons or specific causation. For example, in the Woburn case, it must answer the question of whether TCE from the Grace wells caused Brian Anderson (a named plaintiff) to develop leukemia. Because aggregate patterns admit of individual exceptions, as RS points out, epidemiological evidence cannot say anything definitive about specific cases, at least not all by itself. Thus, because it cannot tell us about specific cases, it cannot, by itself, sustain a claim in a court of law. Law does not accept the completely statistical case. Whether it should is just one of the


262. This is a loaded term of course, with all sorts of meanings, none of which, arguably, is distinguishable from the idea of probabilistic truth. But law treats some kinds of testimony as objectively true even though such testimony always has a percentage chance of being wrong. I use the term here to describe that kind of testimony. The concept of objective truth is sometimes as difficult for science to work with as law. See Ferris, supra note 11, at 254 (stating that “the wave-particle duality... threatens to make hash of the belief that there is an objective reality out there.”); Newton, supra note 13, at 166-99 (discussing the “wave-particle duality”). For Niels Bohr, experience is objectively accounted for when “it can be unambiguously communicated in the common human language.” Niels Bohr, The Unity of Human Knowledge, in Atomic Physics and Human Knowledge 9-10 (1963); see also Newton, supra note 13, at 215-21 (stating that “[s]cience is objective to the extent it avoids bias or external agendas, either because individual scientists are free of them or because the public character of science produces a balance with that effect.”).

263. The emphasis here should be on the word completely. Certainly, the plaintiffs in the Woburn case planned to include epidemiological evidence in the overall package of evidence they hoped to introduce to prove that Grace chemicals
many unexamined issues lurking in the shadows of this conversation that never was.

RS makes it clear then, that legal and scientific proof are different, but this is not news to most of the people in the room. Just the opposite, it is the usual starting point for conversations between scientists and lawyers about the difficulties of meshing science with law. Moving beyond this fact, scientists want to know why law does not accept statistical proof, proof about behavior in the aggregate, or testimony put expressly in probabilistic terms. Other fields of study rely on such evidence every day, often to support decisions affecting interests equal to or greater than those involved in legal decisions. So, it is curious that law does not use such evidence. The audience member's question about uncertainty should have been understood in this light. Scientists want to know why law requires the kinds of evidence it does, not just further descriptions of what kinds of evidence it requires. As AR before him, however, RS takes the legal world view as a given, and provides only additional information about how the legal game is played.

RS (and each of the other panelists as well) might say that his thoughts on uncertainty are complicated, and that the conference format does not allow him to describe those thoughts in all of their detail. Yet, this is the third long statement he has made on the subject, occupying nearly one hundred and twenty-eight lines of text (roughly the equivalent of a twelve hundred word essay), and the fourth statement, if one includes his prepared remarks from the panel presentation (another sixty-five lines of text, and now the equivalent of a two thousand word essay). While this is not enough time to give a crash course in the law of evidence, it is enough to begin to explain why probative evidence is sometimes excluded

causd Brian Anderson's leukemia. But they did not expect that such evidence, by itself, would be enough to sustain their burden of proof. For articles discussing the use of statistical evidence in proving toxic torts cases, see supra note 261.

264. For example, scientists are particularly mystified by law's longstanding reliance on eyewitness testimony. They think that giving eyewitness reports more credence than statistical probabilities is a serious error, almost impossible to explain, and yet law continues to privilege eyewitness accounts even in the face of such knowledge. For discussions of difficulties with eyewitness testimony, see Wellborn, supra note 227; Blumenthal, supra note 227.
from trials, how imperfect information is usually all that courts can get, why legal decisions cannot wait until everything is known, and how the appellate process acts as a check on certain types of errors. He has had time, in other words, to describe the central elements of a full view, the opportunity to flesh out some parts of that view, and the chance to tell the audience where to look for more details. Had he used his time wisely, he could have gotten a quite complicated story on the table.

Instead, he has limited his comments, phrased in different ways, to a simple descriptive point: in effect, that the law of evidence does not accept statistical proof as legally sufficient by itself. He makes a lot of other points as well, of course, but this is the one substantive idea that remains a constant element in all of his remarks, sometimes more overtly so than at others, and is in part responsive to the audience member's question. As he puts it here, "The law just doesn't operate that way." Statistical evidence "doesn't answer the legal question." This, for RS, is the end of the story. He does not seem to see the legal rule as just one approach to fact investigation, and the conference as an opportunity to reconsider and debate the relative effectiveness of that approach.

There are reasons one could give to explain law's refusal to rely solely on statistical proof, some historical, some political, and some having to do with the way in which the American culture privileges personal rights, requiring that individuals be treated as distinct persons, not statistical entities, constructs, or types. It would take a long time to work through a discussion of these reasons, of course, well beyond what the conference's question and answer session could provide. So RS's fault is not that he fails to articulate a jurisprudence of evidentiary individualism fully, so to speak, but that he does not hear the question about uncertainty in law as raising jurisprudential issues in the first place, and that he does not make a non-question begging jurisprudential response.

Equally troubling is the fact that RS does not seem interested in the question of why the legal rules on fact investigation are as they

265. RS, Panel Discussion, supra p. 262, at lines 186-87.
266. Id. at lines 193-94.
267. In the sense of being acceptable to an electorate.
are. It is hard to find in any of his statements, or those of the other lawyers as well, any sense of curiosity about why law investigates facts differently from other disciplines, or any indication that he is intrigued by the different approaches other disciplines use. Legal training and legal practice seem to have induced in RS a kind of unself-critical acceptance of legal methods, and this prevents him from meeting persons from other disciplinary worlds half way. Absent an awareness of the different ways of investigating facts, a curiosity about other methods, an understanding of the barriers to interdisciplinary communication, and the wish to understand experiences from as many perspectives as possible, there is little possibility of true interdisciplinary learning. RS’s comments, more didactic that inquisitive, more certain than contingent, more stentorian than conversational, seem destined instead to produce a kind of interdisciplinary stalemate.

BB then adds what would turn out to be the last comments in the uncertainty exchange, responding perhaps more to RS than to the original questioner.

BB:

(54) I would agree that statistics alone is not going to
(55) answer a question. I think you can take something like
(56) cigarettes and lung cancer and, what is it, ninety
(57) percent of cases of lung cancer among cigarette smokers
(58) are caused by cigarettes? I mean it’s an awful high
(59) percentage. You get that kind of information, that
(60) creates a strong inference that the individual case was
(61) caused by the cigarette smoking. On the other hand, if
(62) it’s one in ten, the inference is not as strong, and
(63) even with that strong inference you have to have other
(64) information to establish individual causation. 268

This statement adds little if anything new to the discussion. BB supplements RS’s point about the generic insufficiency of statistical evidence by giving an example of how even a strong statistical case “is not going to answer [the legal] question.” 269 Yet

269. Id. at lines 54-55.
the example only illustrates RS’s point, it does not explain it. It does not say why an overwhelming statistical case (e.g., that there is a “ninety percent” chance that X causes Y), is not good enough for a court. It simply says that it is not. While he may seem to suggest that statistical evidence is probative in proportion to the percentage chance of something being true, for example, that a ninety percent chance is convincing but a ten percent chance is not, he also points out that even the ninety percent chance requires “other information to establish individual causation.”

BB is trapped in the same descriptive mindset as RS, able to say what legal rules require, but unable to say why. While his intentions are no doubt laudable—he probably wants to make RS’s point clearer—he does not do anything with the truly hard questions on the table. After BB finished, and without entertaining any more questions or comments from the audience (there were a few hands up), the moderator of the panel ended the session and broke for lunch.

IV. WHAT DOES THIS ALL MEAN AND WHAT CAN BE DONE ABOUT IT

It is time to determine what, if anything, this extended discussion of the role of uncertainty in science and law can tell us about the nature of lawyer-scientist communication in general, and what implications, if any, it has for the ways in which professionals are educated. Most of the lessons will be for lawyers, because they dominate the discussion, though this is a function more of the excerpts selected for analysis than the nature of conversations at the conference generally. The first task is to determine whether the

270. Id. at p. 267, lines 63-64.
271. See supra note 247, for an example of scientist remarks from the conference evidencing many of the same communication patterns as those in the remarks of the lawyers. For another example, consider the following excerpt taken from the prepared remarks of an epidemiologist (whom I will refer to as DO), on the scientists’ panel. In this excerpt DO describes his understanding of the similarities and differences between scientific and legal concept of causation. DO’s overall presentation was measured, carefully expressed, tightly organized, not generally contentious, and much less spontaneous or free-form than any of the lawyer presentations. It had the appearance of a carefully structured lecture rather than extemporaneous remarks. The following excerpt starts about ten minutes into
DO's presentation, lasts for about four minutes, and is only a part of DO's overall discussion of causation. It is all that he said, however, on the subject of the similarities between legal and scientific definitions of causation.

DO: A number of speakers said this morning, including one of our panelists, something that I agree with very much, which is that the supposed distinction between legal causation and medical causation [sic]. It's there, right. People talk about the two kinds of causation in different ways, but it's somewhat bogus because what we're trying to do in both instances is, in fact, if I can use that, to find out what really happened. What in fact happened, or is really going on. Now, scientists use a different word for that, but that's what they're trying to do. They're trying to figure out how does the world work, and what really happened when somebody ingested this particular chemical, or inhaled it, or got it on their skin. What, in some metaphysical sense, is going on underneath that. And that's really what the finders of fact want to know in a court situation as well. What did really happen? We express it differently, but it's the same thing. And what sources of information does a scientist have in order to form an opinion about what, in fact, happened. [DO then continued on to list the kinds of evidence a scientist would take into account in determining causation.]

DO is wrong to some extent about law. Legal causation is partly an inquiry into what really happened, but only partly. In law, all inquiry into factual truth must be carried out consistently with protecting other values, such as efficiency, privacy, the need to make sure that all sides do their own work, and the like, even if it means that it is not possible to find out "what really happened." POSNER, supra note 69, at 205-207. Unlike scientists, judges doubt their ability to discover what really happened given the short period of time within which they have to work, and their need to rely on data from parties who tell self-interested stories. DO presumes that law and science have the same interest here, but that is probably based on the belief that they should, and that in turn on the belief that discovering truth is the overriding goal of all work. In many ways this is an unarticulated version of the scientific method as primum inter pares point all over again. It is ironic that DO would say this. He began his remarks by pointing out that his strength as a expert witness (he had testified in court many times), came from the fact that he knew he was not a lawyer, and therefore he never presumed to give lawyers advice about how to present a case. See DO, supra. Yet, here he gives the legal system advice about how to think about the nature of causation, based on a controversial and unargued view that law and science are after the same thing (and that it is science's thing). See DO, supra. I do not suggest that DO is arrogant in doing this, nothing in his manner suggested that, but just that it is very difficult, more difficult than most realize, truly to take on the perspective of another discipline, as opposed to pretend to take it on, or come close to taking it on.

The title to the present article comes from DO's felicitous description of scientific inquiry as a process of trying to determine "what in fact happened, or is really going on." Id.; see also the description of doing science by the California physicist Andrew Strominger, quoted in FERRIS, supra note 11, at 227 (quoting Strominger as saying "The important thing in my life isn't any particular belief —
patterns in the panelists' comments embody a distinctive discourse style, or instead reflect just idiosyncratic and ad hoc reactions to particular situations. At first glance, either conclusion seems plausible. For example, RS is direct and explicit when he disagrees with a point, often dismissing it straight out, while AR expresses disagreement in a more roundabout and euphemistic manner. Are these two forms of a single judgmental response, one overtly evaluative and the other tacitly so, or are they two different types of responses altogether, one indifferent to the effects of disagreement on listener feeling, and the other concerned about expressing disagreement in an easy-to-hear and supportive manner? This type of question could be asked many times over for most of the comments in the above excerpts. While there inevitably will be room for argument, therefore, in my view the differences among the comments have more to do with appearance than substance. In most essential respects, I believe, the lawyers speak in a single style, characterized by a limited number of recurring features, most of which are well known to anyone familiar with the standard criticisms made of lawyer discourse. A description of the most prominent of those features follows.

The most pervasive characteristic, and one pointed out many times over in the foregoing discussion, is the comments' insularity. Seemingly driven by a kind of disciplinary imperialism, the lawyers looked at the issue under discussion solely from the perspective of a legal world view, and required the scientists to enter into that world for conversation to occur. Even though they

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I just want to know what works."). Yet, this is hardly a precise or self-evident expression, and for non-scientists it is likely to have little meaning at all. Later in his presentation, DO objects to similarly vague legal language as not helpful, suggesting that he values precise description. But it appears that he is also sometimes willing to sacrifice precision for other values. Just like law. What is really going on is no doubt clear to scientists as a shorthand expression embodying a well-defined, if inexplicit, set of procedures and substantive norms specifying how science is to be done (in that regard it is similar to BB's "certain enough" expression, see supra p. 237, at lines 29-30), but whether the expression is useful in communicating with anyone outside of science, including other professionals, is a more difficult question to answer. This is a more serious issue for law than science because law speaks to citizens generally, not just legal professionals.

272. Compare their respective expressions of the "based on a misconception" objection to the audience member's question about uncertainty.
were provided with several opportunities to do so, the lawyers did not answer the question about uncertainty, not because they did not want to, there is no evidence of bad faith anywhere in their comments, but because they seemed not to understand what was being asked. They appeared to assume that the existing regime of legal rules and procedures was the only conceivable world, and interpreted the audience member’s question as a query into, or a dispute about, what that world requires, not what it could or should require. It was as if there was no category in the lawyers’ heads which allowed a question about the legal system’s assumptions or purposes to register. Trapped in a technician’s mindset, they could hear only a question about mechanics, not one about design.

The lawyers also were fairly combative, seeming to see conversation as a contact sport in which the goal is to silence others rather than draw them out. They could have examined the question about uncertainty open-endedly and in detail. This would have been an appropriate, even ideal, course for a conference on interdisciplinary collaboration. But instead, they made science, not law, the focus of their responses, and challenged what they took to be the questioner’s understanding of law and the legal system. Rather than inquire or explain, they attacked, often in long-winded and theatrical fashion. They criticized scientists for being pretentious, dishonest, incoherent, or ignorant, even of their own methods. They ridiculed, caricatured, and dismissed what scientists have to say generally, filibustering, or referring to arcane source material, impossible to check, as authority for their views when everything else failed. They did not allow the questioner to explain or clarify her question, or even to speak at all until so much time had passed that it no longer seemed possible to know where to begin. Not belligerent so much as rude, they used their status as panelists as a form of bully pulpit to lecture the scientists in the audience about the correct understanding of law. In the process, they silenced the audience, not by saying something indisputably responsive and true, but by smothering the question about uncertainty in an avalanche of words. They talked a potentially interesting interdisciplinary conversation to death before it had taken its second breath, a strange thing to do, it would seem, at a conference on the possibilities of inter-disciplinary collaboration.
Beyond being rhetorically combative, the lawyers also expressed their views in a somewhat overstated and consistently authoritarian manner. Rarely, if ever, did they admit ignorance, limitation, doubt, qualification, or acknowledge that they might learn something new from the audience, qualities one might expect to find in a conversation between professionals from different disciplines. They never admitted to being surprised or caught off guard, and always knew what they thought about an issue immediately, often before there was time to think about it, even when the subject was science not law. They tended to pronounce more than discuss, making points in an exaggerated or puffed up fashion, and speaking with a higher degree of certainty than they would have, as RS says of scientists, "with the doors closed."273 They took charge of the conversation, imposing their own views on how the issue of uncertainty should be understood and discussed, deciding unilaterally what the scientists needed to know. Such a style can be distracting, if not offensive, to other professionals, especially scientists, who are suspicious of lawyer discourse methods to begin with. At a minimum, it can introduce a great deal of interpretive baggage into conversation, slowing conversation down and making it less likely to be productive. It is not that I think the lawyers intended to be offensive, I have no doubt that they did not, but just that their ordinary way of speaking is readily interpreted in that way. Indeed, it may be the most natural interpretation to give their way of speaking. Looking at these transcripts, it is not hard to get a sense of why scientists do not like to talk with lawyers about matters of substance. Lawyer conversation, it seems, often is hard to make sociable.

No doubt lawyers would give the panelists' comments a more positive interpretation. They would understand, for example, that lawyers often do not mean to sound as belligerent or as dogmatic as they do, and would know the insider codes which enable one to translate lawyer statements and ways of speaking into more nuanced messages and more modulated tones. Yet interdisciplinary communication, by definition, is communication with outsiders. It cannot depend on insider codes for its effectiveness

273. RS, Panel Discussion, supra at p. 221, line 28.
because outsiders do not know the codes. That is one of the principal reasons they are outsiders. Ordinary language is usually the only vehicle available for speaking across disciplinary boundaries, and lawyers must be able to use ordinary language effectively if they are to be understood by those outside the profession. Without compromising the integrity or sophistication of their views, lawyers must be able to express their beliefs, confusions, curiosities, criticisms, doubts, concerns, and the like, in straightforward and familiar terms. They must translate complicated ideas rather than dumb them down, so the ideas can be tested and not just ratified. It would be nice if all lawyers could be trained in other disciplines, at least the one(s) with which they most commonly work, and sometimes this is feasible. Fully socialized into two or more professional worlds, lawyers would find fewer situations in which they would be likely to be confused, to misinterpret, or to overreact. But this is an impractical remedy much of the time. Most people do not have the time, money, or inclination to go both to graduate or medical school, in addition to law school. Life is short and one's remunerative career must begin. As a practical matter, therefore, widespread inter-disciplinary cooperation depends upon lawyers being able to explain complicated legal concepts and procedures to other professionals in language the others can understand and evaluate. Judged against this standard, the lawyers on this panel come up short.

Even if the above excerpts reflect a common discourse style, that does not mean that the style is used widely by lawyers in practice. The conference question and answer session may not have reproduced the conditions of the field, so to speak, so that it would

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274. But see Robitscher, supra note 3, at 303. "We have passed the period of the Universal man and reached the period of the specialist who not only has staked out a field but a narrow portion of a field for his special domain and whose methods and vocabulary have become foreign territory to all other disciplines." Id. Professor Robitscher also reports the results of a survey conducted by Professor William Curran for the Commonwealth Fund, finding that in doing law and science related work, "it is better to have two people representing separate disciplines who have the ability to work well with each other," than "one person combining two disciplines." Id. at 304. Robitscher himself thinks that this conclusion "underrates the advantages of a combination of disciplines in one person." Id. at 305.
be inappropriate to extrapolate from the symposium discussion to lawyer-scientist communication generally. The format of sitting on a stage, in front of an audience, speaking into a microphone, and being videotaped, may have invited speech-making to an extent greater than would be the case in the more informal settings of the law office or laboratory. The lawyers may simply have adjusted their manner to the occasion. On the other hand, my observations of the panelists' "private" conversations at the conference, that is, their conversations with single individuals and small groups after the panel presentations were over, show that in most respects the lawyers behaved the same way in private as they did on stage. The only major difference was that they usually made shorter statements in individual conversations than in the above excerpts. There were no forty to sixty line soliloquies, for example, though they did tend to speak for longer than others when challenged by a comment or surprised by a question for which they had no ready answer. Allowing for these minor differences, in all essential respects, the lawyers' private conversations were pretty much the same as what one sees above.

Perhaps these private conversations, coming as they did in the wake of the formal panel presentations, suffered from a kind of carryover effect which caused the panelists to view the conversations as quasi public, and to behave as if they were still on stage. Yet, there is nothing about speaking in public, on stage or otherwise, to a sophisticated audience already up to speed on the issues under discussion, which encourages overstatement, ridicule, false certitude, filibuster, or other features likely to discourage listeners from paying close attention and considering carefully what is said. In fact, the opposite qualities would seem to play much better, even in public. Succinctly stated and properly qualified claims, limited in scope, supported by explicitly described evidence, and offered as suggestions to be tested rather than conclusions to be ratified, not only encourage others to join in conversation more readily, but also make one look more impressive as a speaker. This was not a Fourth of July oration to citizens in a

275. I also have found similar patterns in law office work conversation. See Condlin, supra note 75, at 368-74.
public park, and the panelists were not running for elective office. It was a gathering of professionals, most of whom were knowledgeable about the topic under discussion, who expected to participate in an inter-disciplinary conversation about the relationship between law and science. The lawyers did not come to the conference to preach to the converted, or at least said that they did not, and given the audience, any other plan would have been foolhardy. In this context, it was more sensible for them to want to appear insightful, open-minded, curious, and respectful, than it was to appear authoritative or dogmatic.

The patterns also cannot be explained by ignorance, inexperience, or lack of skill. The panelists are blue-chip law-and-science experts in any sense of the term. Each is among the most well-known and highly regarded lawyers in the country at representing plaintiffs, and defendants, in science related litigation. Each is accustomed to working with scientists, has written extensively about the process in the academic journals, and has well thought out views on the issue of uncertainty in law and science. Many of their articles, in fact, have been cited by courts and academic commentators as exemplars of the best work in the field. Moreover, the patterns in their comments—an insular and legalistic world view, a pervasive combative nature, a dogmatic and authoritarian manner, and a propensity to filibuster and take unilateral control—are similar to patterns lawyers are criticized for using in non-science related contexts. This suggests that the patterns may stem from something more basic in the way lawyers are selected, trained, and socialized, than from circumstances or conditions peculiar to the conference itself. For example, the patterns reflect a set of adversarial skills that are adaptive in many situations in which lawyers work. While they do not do much to advance understanding in non-advocacy relationships, these skills play an important role in the more overtly competitive relationships of the courtroom or boardroom, where being able to take charge of a conversation and control its outcome is a useful thing. There is a

276. Panelists AR and RS.
277. Panelist BB.
278. See Socrates' New Clothes, supra note 78, at 282 n.128, and articles referred to therein.
reason then for lawyers to learn to behave as these lawyers did in the above excerpts. When skills of this sort are learned unself-consciously, however, as they are in most American legal education, so that they cannot be turned on and off on command, or even always recognized when on, it is not surprising that they would surface in situations where they do not belong. While there is no way to be certain on the basis of the conference data alone, there are many reasons to believe that the above patterns represent the way lawyers communicate with scientists generally, and that they may be the norm rather than the exception.

If lawyers, as a group, converse in ways that are difficult for scientists to interpret, thereby exacerbating the natural cultural tension between law and science, the next question is what can and should be done about it. Changing the cultures of law and science to make each discipline more open to, and respectful of, the other does not seem to be a realistic possibility. These cultures have not changed substantially in tens, perhaps hundreds, of years, in part because each, as presently configured, is reasonably well adapted to the particular objectives of its respective discipline. There is something workable in the culture of each discipline, in other words, that is likely to outweigh any countervailing pressure for change produced by the friction generated when law and science rub together in the wrong way. This does not mean that individual lawyers cannot and should not transcend their cultural conditioning in particular cases as part of the process of working with scientists to resolve joint inter-disciplinary problems. They can and should. However, it does mean that attempts to fuse the disciplines of law and science into a single, harmoniously-functioning whole have their greatest chances of success when they make practitioner behavior, and not disciplinary structure, the object of their reform.

279. See Jasanoff, supra note 1, 218-23 (describing structural and institutional responses to the law-science incompatibility problem).

280. Cf. Areeda, supra note 11, at 1039-42 (discussing the conceptual and analytical difficulties involved in joining different disciplinary bodies of thought together). But see Kass, supra note 3, at 9 (arguing that "the so-called two cultures can properly be bridged, if at all, only by a philosophical reconstruction of the scientific side of the divide").

281. Scientists should do the same, of course, but my focus here is on lawyers.
Lawyers need better conversational learning techniques, that much is clear. They need to listen more, pontificate less, draw others out when they are too inhibited to provide details on their own, explain, support and test arguments against direct evidence rather than assumptions, beliefs, and presuppositions, and generally be less quick to judge or conclude about the worth of ideas that are new or surprising until those ideas have been examined fully and fairly. Technique does not exist in a vacuum, however, it flows naturally and logically from a conception of how one ought to behave, and in this sense is the instrumental side of one's conception of professional role. So what lawyers really need is a richer, more multi-faceted conception of their professional role, one that explains what it means to act as a lawyer in the wide variety of settings in which lawyers operate outside of the courtroom. Such a conception of role should help lawyers to function with one foot in and one foot outside of legal practice, so to speak, able to use practice technique effectively, without at the same time being trapped in a static or purely technical conception of their role. It should help lawyers understand the social, political and moral dimensions of law practice, how the pursuit of justice overlaps with, but also differs from the pursuit of truth, and how good practice technique is a necessary but not sufficient condition of good practice.

Lawyers may resist this suggestion. Multi-dimensional

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282. At least the lawyers in these excerpts, and the rest to the extent that they are like these lawyers.

283. See Condlin, supra note 75, for an extended discussion of this topic.

284. There have been famous institutional examples of attempts to educate judges, lawyers, and scientific experts in each other's modes of reasoning and discourse. See, e.g., REFERENCE MANUAL ON SCIENTIFIC EVIDENCE (1994) (describing the principles of epidemiological studies, cancer bioassays, and risk analysis to federal and state judges); Areeda, supra note 11, at 1038 (describing seminars for judges on disciplines other than law organized by the Federal Judicial Center). What I have in mind here are actions by individual practitioners working together on particular cases to supplement these institutional efforts. In explaining law and legal practice to scientists, however, lawyers should take pains to emulate Niels Bohr and "never express themselves more clearly than they think." PAIS, supra note 200, at 170. They should learn to think out loud, in other words, honestly and transparently, but with "more caution and humility" than is presently the case when talking with those in disciplines outside of law. Brink, supra note 11, at 274.
conceptions of role complicate professional life by making judgments more difficult, and this makes such conceptions *prima facie* suspect in the minds of many. Understandably, lawyers would prefer to have role obligations clearly defined, even reducible to a set of routines that, when properly carried out, guarantee success.285 Lawyers think that only academics prefer complication and risk, and then mostly in theory. As a consequence, the idea of law practice as a political and moral phenomenon is likely to meet with a collective groan in many quarters.286 While this reaction is understandable, ultimately it must be overcome. People who become lawyers are sophisticated enough to understand the difference between performing within a disciplinary system, and standing outside of that system to describe and evaluate it. Working with a bifurcated conception of role, in other words, they are able to enter law practice sufficiently to be able to perform identifiably as lawyers, and yet at the same time, also remain separate from law practice sufficiently to be able to explain and criticize how it operates. This is not to say that lawyers regularly operate in this way, some do and some do not. Nor is it to say that legal education teaches about law practice from this perspective. Again this varies, but maintaining the split personality of performer and critic is something lawyers are capable of understanding as a concept and putting into practice as a skill. In

285. Ironically, they also like to have others think that legal work is full of complicated and difficult judgment calls, and something not everyone could do. This is another example of the point reputedly made by Lon Fuller about law students, that they “want to explore the universe, and know where they are every step of the way.” Who wouldn’t? Cf. Robitscher, *supra* note 3, at 307 (describing the “different temperaments” of lawyers and doctors).

286. There are lots of reasons for lawyers to take such a perspective on their work. They run a sellers’ market, so to speak, in the sense that they have the power, through law, to impose sanctions on anyone who refuses to communicate in strictly legal terms. Alone among the professions, they are both regulated and regulator. They might feel the natural tendency to speak exclusively in terms of the system they know best. Having worked hard to become effective within that system, they could be understandably reluctant to start all over again learning another. They could believe in the benefit of “bright line” rules, a benefit that would be lost by the adoption of a more complex, multi-perspectival approach to thinking about law and legal practice. It is impossible to know how large a role these and other such factors play in reinforcing the legal-dominant discourse style seen above.
fact, such dual-perspective viewpoints are routinely described and recommended in the literature on the teaching of law practice skill. It is the central idea behind the “critical reflection” mantra of clinical legal education.287

The above excerpts illustrate the dangers involved when, for whatever reason, lawyers or any group of professionals understand and try to explain a complicated social, political and moral system principally as a set of disembodied rules, or when they pursue the development of practice technique (whether analytical or interpersonal), so single-mindedly that it prevents the development of critical consciousness. For all of their talent, skill and expertise, the lawyers in the above excerpts were not able to explain or look critically at law and legal practice in non-circular terms, with a group of intelligent and interested scientists, surely not the hardest audience. Legal education cannot prevent this “reduction to technique” perspective from taking hold completely, of course, but it can weaken its grip somewhat by teaching about law and practice skill as means to an end, rather than as an end in its own right.288

The end of law practice is justice. If there is not an argument for why a particular legal rule, procedure, or practice convention advances justice, that rule, procedure, or convention is not worthy

287. See Condlin, supra note 75, at 338-39 n.3 (describing the “critical reflection” dimension of clinical legal education). For examples of other types of “practitioners’” thinking in such a dual or critical perspective, see Jon O. Newman, Rethinking Fairness: Perspectives on the Litigation Process, 94 Yale L.J. 1643 (1985)(sitting judge analyzing litigation practices and rules from the perspective of whether they promote fairness); Susan Silbey & Austin Sarat, Critical Traditions in Law and Society Research, 21 Law & Soc’y Rev. 165, 166 (1987)(promoting the development of a “sociology of the sociology of law” that would “reconstitute and reimagine the subject of socio-legal research”).

288. This recommendation is based, in part, on a belief such as that expressed by Andrew Delbanco at the end of his review of Roger Shattuck’s book Forbidden Knowledge, when he says:

In the last analysis, Forbidden Knowledge is a rebuke and challenge to our institutions of education, where no one has solved the perennially lamented problem of the cultural divide between science and the humanities. Written by someone who has bridged that gap, this book confirms that only if we create humane scientists and scientifically informed humanists will we stand a chance.

of respect, and criticisms of it by others should be taken seriously, rather than dismissed or shouted down.

Many of the objections to legal rules and practices made by scientists and others in the above conversations and elsewhere, may be difficult to listen to because they are essentially correct. Outsiders are often better positioned to identify warts in a system of thought than those with a stake in the system, and there is no reason to expect this general principal not to be true for law.289 Moreover, there is no basis to believe that legal rules and legal practices will always be sensible. Law, like any system of thought, is produced by a mixture of forces, some rational and others not, so that it is reasonable to expect aspects of the system always to be subject to legitimate criticism. The trick for lawyers is to learn to welcome such criticism as a whole, while at the same time, find within it the limited number of worthwhile ideas capable of being put into effect. This means that lawyers must learn to see critics of the profession as political associates of sorts, with whom they (the lawyers) are paired in constantly testing and updating the content and structure of law.

Since criticism is a difficult and time consuming process, lawyers also must learn to help others provide it, by listening more patiently and more generously than they do at the present, by drawing others out when they do not provide sufficient detail on their own, and by not silencing critics in the manner of the lawyers in the above discussion. Conversing with critics shares some of the features of taking the deposition of someone who answers questions in a lengthy, self-serving, and argumentative fashion. One could squeeze the argument out of the answers, of course, by forcing the person to play by the testimonial rules, but it would be smarter to hear the answers out in all of their gory detail. Get the full story in other words, and then go away and think about it, before deciding what response to make. In a world characterized by competitive rhetorical performances it is sometimes easy to mistake silencing others for conversational success, and in some situations, no doubt, that would be a fair conclusion to draw. But

289. See POSNER, supra note 11, at 98 (describing how many advances in disciplinary thought come from outside the discipline).
when the objective of conversation is shared inquiry and mutual learning, premature silence is almost always self-defeating.