ESSAY:

CONVENTIONS IN SCIENCE AND IN THE COURTS: IMAGES AND REALITIES

JEROME R. RAVETZ*

I

INTRODUCTION

The source of our difficulties with "conventions in law and science" is related to images. Practice as experienced in the law contrasts with conventions accepted in science. The difficulties in applying science in the law are commonly explained in terms of the differences in conventions,[1] but to some significant extent they are the result of the images by which those differences are identified. By clarifying these differences, we can perhaps leave aside those places where the differences are only apparent and concentrate instead on those where real differences do exist and cause real difficulties.

II

IMAGES OF SCIENCE

The image of forensic practice described in the Guidelines is no doubt broadly correct.[2] After all, the Guidelines image is related directly to the difficulties experienced in the ordinary work of the courts. But the image of science invoked here is quite otherwise. It is derived from a certain portrait of science as an essentially gentlemanly, honorable pursuit, whose conventions ensure unfailing courtesy and goodwill. Now, that portrait, which is derived from a traditional self-image of science, is also born of experience. But that traditional self-image of science was not the outcome of an empirical social-

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* Associate Fellow, James Martin Institute for Science and Civilization, Said Business School, Oxford University.

1. *See, e.g.*, Scientific Knowledge and Public Policy Project, Guidelines for the Fourth Coronado Conference 2007 ("In the scientific context it is customary ('conventional') to discuss the many sides of the issue, often in a stylized 'on the one hand, on the other hand' manner. On the witness stand, by contrast, the scientist is encouraged, almost required, to present one side, leaving the experts on the opposing side to present 'on the other hand.'") (on file with *Law and Contemporary Problems*).

2. *See id.* (describing scientific practice as including stylized "on the one hand, on the other hand" debate with disagreement between scientists based on substance).

science enquiry. It resulted rather more from an exercise analogous to propaganda that arose largely from what was once eloquently described as “"the warfare of science with theology in Christendom.”[3] In the opening line of the classic radical analysis of scientific practice, *The Structure of Scientific Revolutions*, Thomas Kuhn spoke of the “image of science by which we are now possessed.”[4] For him, this image was largely a fabrication for which previous generations of historians were responsible.[5]

This is not the place to analyze that image, its history, and its consequences. But it does recall the image of classical Athenian society (democratic, open, and engaged) as portrayed in Pericles' 431 B.C. funeral orations.[6] That Periclean image, too, was rooted in genuine, lived experience and it also served as a source of idealism, both for Pericles' contemporaries and then for those born millennia later. But when taken as an objective description of Athenian society for the purpose of later scholarly analysis, that image was seriously defective and misleading.

This revealing history of an image is relevant here. It can perhaps help sort out some of the difficulties of science in legal settings that have brought us to the discussion addressed in this symposium and that will require adjusting and modernizing the image of science and its conventions. By examining some of the varieties of scientific practice, defined in terms of the function of their products, we can place the forensic practice—its context and functions—as just one among many. Of course, it has its characteristic features and its points of tension and conflict with the others. But by means of such an analysis we can escape from the supposedly unbridgeable chasm between two utterly different sorts of conventions and forms of practice.

A. Varieties of Scientific Experience

This analysis is not intended to be at all definitive or refined. Its goal is only to establish a useful point. The analysis starts with the “iconic” sort of science—pure or basic research. Just to be clear about fundamentals: even this sort of research does not consist of “discovering facts” as if they were the pebbles that Sir Isaac Newton famously imagined himself picking up on the beach.[7] Of course, discovery is there, and indeed is at the core. But the actual work consists of setting and investigating problems, in the form of hypotheses that are more or less formalized, about some aspect of the workings of the world out there under study. Now, what makes this sort of work “pure” is that, in an important sense, its productions do not matter. That is, the function of the activity is

3. ANDREW D. WHITE, A HISTORY OF THE WARFARE OF SCIENCE WITH THEOLOGY IN CHRISTENDOM (George Brazille 1955) (1896).

4. THOMAS S. KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS 1 (2d ed. 1996).

5. *Id.*

6. THUCYDIDES, THE PELOPONNESIAN WAR 120–28 (Richard Crawley trans., 1876).

7. DAVID BREWSTER, MEMOIRS OF THE LIFE, WRITINGS, AND DISCOVERIES OF SIR ISAAC NEWTON 407 (1855).

internally oriented. The work is designed to produce more knowledge of the same sort as that on which it is based.

This internal orientation produces various tensions with the outside world. In particular, those who support the work need to be convinced that it is worthwhile in spite of not delivering the sorts of returns that are customary in business or statecraft. But, in other ways, that purity provides a liberation from the pressures and constraints of the ordinary world. Indeed, some eminent scientists of earlier times imagined that pure research was almost another realm of existence, far removed from the corruptions of ordinary life.[8]

That idealistic, perhaps idealized or "Periclean," self-image performed a variety of important functions, including the shaping and justification of special norms of behavior. I have argued that, in the absence of external sources of quality assurance, pure science has needed such norms to motivate scientists to do well in their work and to do good in their social practice.[9] The norms were expressed (as idealized) in Robert K. Merton's classic formulation as "the affectively toned complex of values and norms which is held to be binding on scientists."[10] The relationship of these norms as formulated by Merton to the explicit beliefs of scientists, pure or otherwise, was never entirely straightforward. Although a distinguished sociologist, Merton did not claim that his list of norms was the distillation of empirical survey work; much of his later work was devoted to showing how scientists actually behave as humans rather than as plaster saints.[11]

The Mertonian norms of pure science have had one important negative effect: they served to reinforce the preciousness, one might even say snobbery, of those university scientists who enjoyed lifetime tenure in an undemanding daytime job, with encouragement and resources to pursue their hobby in research.[12] It is not surprising that many of them believed that this good fortune was theirs by right and that those whose occupation was more directly related to the outside world were in some ways either traitors or prostitutes. Since the occupation of "pure scientist" is of extremely recent origin in the social history of science,[13] this view was really rather parochial; but for a couple of generations it was quite powerful in forming the consciousness of scientists and of analysts

8. *See* JEROME R. RAVETZ, SCIENTIFIC KNOWLEDGE AND ITS SOCIAL PROBLEMS 32 (1971) (quoting physicist Pytor Kapitsa's observation that science has lost her freedom and is now an enslaved productive force).

9. *Id.* at 273–88.

10. ROBERT K. MERTON, THE SOCIOLOGY OF SCIENCE 268–69 (Norman W. Storer ed., 1973).

11. *See, e.g.*, Robert K. Merton, *The Matthew Effect in Science*, 159 SCIENCE 56, 56–63 (1968).

12. *See* STEVEN SHAPIN, THE SCIENTIFIC LIFE 213 (2008) ("[R]esearch universities increasingly offered themselves as handmaids to industry [in the second half of the twentieth century]; helping industry was identified as public service; and the sorts of professional commercial ties that were once a source of administrative unease were now enthusiastically encouraged.").

13. *See* RAVETZ, *supra* note 8, at 37–44.

of science.[14] There was simply no other story that told scientists who they were. But we can escape from the "Mertonian" cage by realizing that the combined societal tasks of the advancement of knowledge and protection of intellectual property were recognized and managed long before the ideals of "pure science" were dreamed of.

In spite of their being largely ignored by historians and propagandists, some very important scientific-research labs have always had goals in the service of industry. Part of the power of late nineteenth-century Germany lay in its "Kaiser Wilhelm" establishments, where the best research scientists collaborated with more practically oriented colleagues in the advancement of technology.[15] Early in the twentieth century, the great U.S. labs of invention, started by Edison[16] and continued by the labs of General Electric[17] and Bell Telephone,[18] produced industry-oriented research and invention of the highest quality.[19] Their elite scientists worked on both "pure" and "applied" projects—the former yielding contributions to a branch of "the republic of letters" and the latter contributing to the profitable work of their employers.[20] The forms of intellectual property were correspondingly distinct: on one hand, there were open publications drawing "rent" in the form of citations, and, on the other, trade secrets and patents, intellectual property to be utilized or marketed to best advantage. In such cases the governing norms are a mixture of those applying to any functionary in a bureaucracy (of which the character will lie somewhere on the spectrum between the images defined by Max Weber[21] and by Franz Kafka),[22] together with those of any specialist, learned-professional group. Of course there will be conflicts between the two roles and plenty of opportunities for corruption; but experience shows that such norms can, in spite of their lack of inspirational statements, work adequately.

Another important sort of science is developed within the systems of state regulation. Here again is "mission-oriented science," but within rather tighter constraints even than in industry. For in this case the categories in which the work is done are set to a great degree by legislation, rather than (as in the "pure" case) by the possibilities of successfully exploring Nature or (as in the "applied" case) by the possibility of developing a useful device. Regulation is

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14. *Id.* (discussing the period in the late 1900s when universities began to take a greater role in industry science, thereby increasing the number of "applied" scientists and decreasing the number of "pure" scientists).

15. JAMES RETALLACK, GERMANY IN THE AGE OF KAISER WILLHELM II 64–65 (1996).

16. JOHN WINTHROP HAMMOND, MEN AND VOLTS 20–24 (1941).

17. *Id.* at 314–15.

18. SHAPIN, *supra* note 12, at 94–95.

19. RETALLACK, *supra* note 15, at 20–24; HAMMOND, *supra* note 16, at 315.

20. Wikipedia.org, Republic of Letters, http://en.wikipedia.org/wiki/Republic_of_Letters (last visited Jan. 4, 2009).

21. Wikipedia.org, Bureaucracy: Max Weber, http://en.wikipedia.org/wiki/Bureaucracy#Max_Weber (last visited Jan. 4, 2009).

22. FRANKZ KAFKA, THE CASTLE (Willa & Edwin Muir trans., 1974); FRANZ KAFKA, THE TRIAL (Willa & Edwin Muir trans., 1986).

about activities that interact, possibly harmfully, with their total social and natural environment. These interactions are complex, and, in order that regulations may be successfully framed and enforced, some aspects of that reality must be made salient and simple and the others neglected.

In the case of public-service bureaucracies, the norms are in some ways more demanding and perhaps analogous to those of pure science since the simple quantitative criteria of profit are not available (except when created artificially by imposed, internal accounting systems). In such cases, the norms have come to include a justification for whistle-blowing in recognition of the severe conflicts of principle encountered when an employee recognizes corruption that the institution tolerates or fosters.

To take a very simple example of regulatory science, in considering "safety" on the road, legislation (and the accompanying research) may focus on risks to those inside the cars. Regulators may conduct research-and-development work on belts and other restraining devices to keep people from bouncing around inside their cars in the event of a crash. But this may well involve neglecting those who get hit by the cars and whose risks might even be higher because the drivers of the better-equipped cars might feel safer and hence go faster! Also (and this point hardly needs stressing for a U.S. audience), the results of regulatory research can have implications for the severity of regulation to be imposed and, hence, for the costs of operation of those being regulated. We have quite enough evidence that those who are being regulated will try to "regulate the regulators" and with them their scientists.[23] In this sort of science we are a long way from *Our Town*.

Further, quite a bit of scientific research is done directly in connection with legal processes. Most obviously in tort cases involving damage, due diligence, and liability, much will depend on scientific information that is tailored to the needs of the case. Here the "conventional" restraints are even more severe than in the case of regulation, for the legal framework of the cases can be quite convoluted.[24] Hence, the research has little resemblance to the free exploration that characterizes traditional, "pure" science. Moreover, the whole process can be protected under "lawyer–client confidentiality," so that even the very existence of the research can be protected from outside view. Of course, this feature is not unique; military research is similarly protected. But here, in the civil sector, it is a sizeable example of tightly designed intellectual property of the most rigidly constrained sort.

The sort of science that has the most affinities to the legal process is that fed into policy. For a long time until recently, this form of practice suffered from the illusion of a total separation between science and policy. In the dominant U.S. image, the scientists provided the objective facts, the policymakers filtered them through their value commitments, and out came policy. The experience of

23. *E.g.*, CHRIS MOONEY, THE REPUBLICAN WAR ON SCIENCE 102–20 (2005).

24. SUSAN HAACK, DEFENDING SCIENCE—WITHIN REASON 233–64 (2003).

the George W. Bush Administration was very educational, in that now we all know about the full cycle of scientific advice.[25] This starts with the prioritizing of the issue and the choice of persons, then the framing of the problem, through to the adoption of appropriate methods, and finally to the control of the output. We have learned by example how, at every phase of this cycle, the process can be manipulated and corrupted. To use the terms "objective" and "factual" for its products may sometimes be more of a courtesy than a description.

Even when the game is played straight, there are significant problems of communication, or translation, between the two realms of science and policy. In particular, scientists must learn how to express their judgments of uncertainty and quality, for these can be quite crucial in what is understood by their audiences. Although the work of policy-relevant science can still be quite distinct from the decisionmaking itself (as opposed to in the courts), still there is an interaction between the two sides. This area has been well analyzed by Roger A. Pielke, Jr., with his key concept of "stealth advocacy."[26] Recognizing the problem of communication in this other sphere can prepare one for understanding courtroom practice, such as when the scientist is hired as an expert witness, ostensibly neutral, but really there to strengthen his client's case.

Another strong similarity between policy-related science and the courts is made evident by contrasting the other forms of scientific practice. Research science has well been called "the art of the soluble."[27] Research scientists have the luxury of being able to wait until a field has sufficiently matured technically to engage on the exploration of a particular topic. To rush in prematurely is a sign of bad strategy. In other sorts of science, that luxury does not exist. In technological development, meeting competition in either the civil or the military sphere, one must make do with what can be done within tight constraints of time and resources, perhaps even adopting a policy of "third best." On occasion, this can lead to confusion and resentment among traditional scientists, who believe that their calling permits only the highest standards of technical quality. One such incident occurred in the Netherlands, and caused a scandal that reverberated for many months.[28]

Finally, there is that large and essential sector of science: teaching and training. Of course, one could not expect the community of researchers to be reproduced exactly among those immature students and overworked teachers. However, what we find there is a social practice in which conventions of dialogue and openness are, if remembered at all, a bad joke. Up to now, the teaching of science has remained the most authoritarian and dogmatic activity

25. *See* MOONEY, *supra* note 23, at 17–24.

26. ROGER A. PIELKE, JR., THE HONEST BROKER: MAKING SENSE OF SCIENCE IN POLICY AND POLITICS 3 (2007).

27. P.B. Medawar, *Koestler's Theory of the Creative Art*, NEW STATESMAN, June 19, 1964, *reprinted in* 67 NEW STATESMAN: THE WEEK-END REVIEW 950 (1964).

28. Jeroen van der Slujis, *A Way Out of the Credibility Crisis of Models Used in Integrated Environmental Assessment*, 34 FUTURES 133, 133–37 (2002).

in our intellectual culture. Over the years, students learn by the example that for every problem there is just one and only one solution, the correct one that is found in the answers at the end of the textbook. One does not argue with the teacher. It is very hard to find a science exam with the sorts of questions common in social science and humanities subjects, like “compare and contrast” or “critically examine.”

For those practitioners who really know, science is a gloriously messy business, where things at the exciting research fronts are totally insecure in nearly every way and where the different sorts of scientific communities solve their communal problems imperfectly but still (we hope) well enough. The conventions of discourse in science vary enormously, of course. But the typical scientific paper is one of advocacy, wherein objections and difficulties are managed *en passant* as they are being solved rather than being paraded in an evenhanded way. Debates between scientists are of course more objective and impersonal when the stakes are lower or the fields more settled; but otherwise it is only natural for a scientist to doubt the mental and moral capacities of an adversary who refuses to see the obvious truth of “My Theory.” The published research literature does not usually function as the archive for such personal campaigns; their traces will be found in published review articles or in private reports evaluating project proposals or papers for publication.

The purpose of this rather extended discussion is to establish a continuity in several dimensions between the various conventions of scientific discourse and that of the courts. The real differences will thus be seen as relating to a family of scientific practices, rather than as defining an utterly different realm of action and discourse.

Only a minority of the social roles occupied by scientists are “pure” in either sense (that is, totally distinct from any other or uncontaminated by practical concerns). Plurality of roles, and possible conflict between their respective norms, is the condition of life of a scientist (other than a research technician) in the contemporary world. Of course such complex situations are the breeding ground for corruption, since the grey areas that lead into corrupt practices are not fenced off by simple rules. It is possible that someone could formulate a set of universal norms that would apply to the pursuit and application of knowledge in all circumstances, but that is beyond the present task. Still, during any such exploration, the traditional scientists’ goal of the pursuit of truth would need to be adapted to include the maintenance of quality. This attribute is sufficiently broad to encompass all the areas of practice and sufficiently complex to comprehend the variety and conflicts among those practices. If I were to embark on discussing the ideals that should motivate the behavior of scientists, I would argue that objectivity is now as obsolete as truth, and that in its place we should cultivate “integrity.”[29]

29. JEROME R. RAVETZ, THE NO-NONSENSE GUIDE TO SCIENCE 61–77 (2006).

B. On the Bridge: Expertise in the Courts

Now to the most contentious element in the whole scene of science in the courts: the experts. The clarifications introduced so far can also be useful in improving our understanding of these special problems. First, a distinction must be made between two very different sorts of practice employed under the same name. The great majority of cases involve "research-science expertise." Typically, a person who is knowledgeable and skilled in a particular area of research will be called on to offer an opinion on a technical question for which their special knowledge is deemed relevant. On the face of it, this would seem to be straightforward; in his job the scientist deals in facts, and here in the courtroom is just another set of facts to be discussed. Of course, it does not work that way.

Active scientists know that the border between objective facts and subjective opinions becomes fuzzy as soon as the issue becomes interesting and hence contentious. There is no reason why the materials presented in the courtroom should always be more like those in the textbooks than those in the labs. Of course, the style of debate in the courts will be different from those in the different sorts of science; and this can indeed cause confusion.

The research report is a highly stylized discourse, and its conventions are not easily translated to those of the courtroom. An active scientist accustomed to debating interesting and conflicted issues will recognize what is going on. Unlike the teacher, the active scientist does not deal with incontrovertible facts, but rather with inconclusive evidence and complex arguments. But there are important skills to be learned. Just how to manage the different sorts of qualifications of a statement respecting the strength of its own evidentiary support, its criticality for a larger argument, and the appropriate degree of generality in its claims, can be tasks for which experience of this new forum can be very important. Also, a research scientist might find quite serious difficulty in the new role of being paid directly for a statement. Although there is a whole body of discussion of the behavioral norms of the expert (thus, whether the expert serves the client or the court),[30] it is well recognized that the situation is far from satisfactory.[31] And finally, there are real problems of credentials. But for an active scientist accustomed to debate on interesting issues, the idea of introducing evidence, rather than laying down facts, should present no serious difficulty.

The other sort of expert presents quite different problems. This expert is a professional, typically from a medical field, who may enjoy an academic appointment and whose professional standing depends in the last resort on superior judgment deployed in some clinical context. These unavoidable features of the professional role produce problems of low quality that are the exact opposite of those experts whose qualifications to testify are governed by

30. *E.g.*, TAL GOLAN, LAWS OF MEN AND LAWS OF NATURE (2004).

31. *See, e.g.*, SHEILA JASANOFF, SCIENCE AT THE BAR (1997).

Daubert v. Merrell Dow Pharmaceuticals, Inc.[32] It is not a case of the courts excluding would-be scientific experts with dubious qualifications, but of accepting would-be professional experts whose eminence is bogus.

This issue is a particularly sensitive one in England just now, following the suicide of a victim of professional malpractice in the courts.[33] In 1999 Mrs. Sally Clark was convicted of the murder of her two children; after a lengthy stay in prison she was finally exonerated.[34] But she never recovered from her ordeal, and she died in March 2007.[35] In her trial, crucial evidence was given by Professor Sir Roy Meadow.[36] Using his authority as a pediatrician to conceal his total ignorance of statistics,[37] he asserted that such a pair of sudden deaths had a chance of only one in seventy-three million.[38] Sir Roy, who already had a reputation in connection with "Munchausen syndrome by proxy" (described in the House of Commons as "one of the most pernicious and ill-founded theories to have gained currency in childcare and social services in the past ten to fifteen years")[39] was subsequently struck off the medical register,[40] later regaining that status through a lawsuit.[41] There are other notorious cases of similar victims; many parents have lost their children through the (secret) family courts on the basis of "expert" evidence by such doctors.[42]

Although the testimony of such experts is considered scientific, that term is really inappropriate. Needless to say, even worse is the use of professional expertise whereby witnesses are called on to make assessments that lie outside the categories of their science, such as psychiatrists passing on the ability of a defendant to distinguish right from wrong (the "M'Naghten Rules").[43] As the Sally Clark case shows, it is all too easy for a person to capitalize on professional eminence in order to indulge in specious scientific judgments in the courtroom. The correction of such abuses lies not in improved scientific methodology, but in improved accountability of the relevant professions.

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32. 509 U.S. 579 (1993); *see* HAACK, *supra* note 24, at 242–43 ("peer-review, a 'known or potential error rate,' and 'widespread acceptance'—the so-called '*Daubert*' factors" apply to the methodology of hypothesis-based expert opinions).

33. *See Obituary of Sally Clark*, *Solicitor Wrongly Convicted of Infanticide Whose Ordeal as the Victim of a Miscarriage of Justice Became a Cause Célèbr*e, THE DAILY TELEGRAPH, Mar. 19, 2007, at 25.

34. Minette Marrin, *Sally Clark, a Mother Wronged to Death*, THE SUNDAY TIMES, Mar. 18, 2007, at 16.

35. *Id.*

36. *Id.*

37. *See* News Release, The Royal Statistical Society, Royal Statistical Society Concerned by Issues Raised in Sally Clark Case (Oct. 23, 2001) ("The well-publicised figure of 1 in 73 million thus has no statistical basis.").

38. Marrin, *supra* note 34, at 16.

39. Wikipedia.org, Sir Roy Meadow, http://en.wikipedia.org/wiki/Roy_Meadow (last visited Jan. 4, 2009).

40. Marrin, *supra* note 34, at 16.

41. *Id.*

42. Clare Dyer, *Family Courts' Veil of Secrecy Will Lift to Win Back Public Confidence*, THE GUARDIAN, Dec. 5, 2005, http://www.guardian.co.uk/media/2005/dec/05/broadcasting.politics.

43. *See* BLACK'S LAW DICTIONARY 1001 (8th ed. 2004) (*McNaghten* rules).

C. Differences Between the Discourses: Facts

An appropriate starting point for some of the differences between fields, or discourses, are those that may be less real than they seem. First, consider "fact." One may imagine a naïve scientist (of whom there are still many) coming into a courtroom as an expert, expecting to retail his facts as they are relevant to the case. He may even have heard that the jury is a "trier of fact"—and he has some good ones to offer. Imagine his dismay when, instead of his facts being attended to respectfully, he is treated by counsel as just another witness, and perhaps even one whose testimony is rather confused and not quite clear or honest. He comes away bewildered and not a little bruised. No one had ever impugned his competence and integrity like this! He naturally wants to retreat to the lab, where colleagues are at least courteous to his face, and students deferential.

This clash of illusions is worth exploring at some length. Both sorts of "fact" must be understood if the matter is to be sorted out. On the science side, the term "fact" should be used more sparingly than has hitherto been common.[44] The outputs of the problem-solving activity go through a complex and lengthy process of evolution after the lab books are closed. There is a "research report," which will be scrutinized by referees for a journal (when the work is in the "open-source" sector of science) and which may, when modified, be published. Its status is then ambiguous; a leading article in a high-prestige journal will generally be accepted as probably correct, while the contents of low-prestige journals are assessed less generously if indeed they are even noticed at all.

The story of the "fact" is only just beginning. In research science, that result lives on only if it stimulates some further research that will amplify, deepen, reinterpret, and (when appropriate) correct it. If the "item" survives this process, first by being interesting enough to be followed up and then remaining of interest after all the subsequent work, then at that point, and not before, is it justifiably called a "fact." Even then its status is not absolute. If the field of inquiry in which it is embedded falls out of fashion or is superseded, that "fact" may die with it. In experimental fields, the equipment used to generate the underlying data, the physical parameters defining its operation, and the theory in whose terms the whole operation has meaning, may all be obsolete and forgotten. The item itself may then be either no longer of interest, or of little use, or just irrelevant to practice. Only those "facts" that survive all those vicissitudes of history and become "knowledge" can be truly "factual." And then they become the materials of taught science, and are simplified or vulgarized for the benefit of immature minds. In those crude, usually confused, versions they stand as the symbols of genuine scientific knowledge, thereby both instructing and misleading us all for many generations.

In the courts analogous patterns are discernable in spite of all the pronounced differences. We left the expert with his scientific information, which he had so fondly believed to be the facts of the case. For the court, that

44. *See* RAVETZ, *supra* note 8, at 181–208.

was just his testimony, which is introduced as evidence. In this respect, the legal procedure is actually more sophisticated than the scientific. For, when the material is considered as "evidence," we know right away that it is context-dependent and subject to assessments of quality in several dimensions. Furthermore, and here is the joke of the whole story, the "fact" of which the jury is the trier is not that particular item of scientific information, anyway. The "fact" is what the "verdict" is about. This will typically relate to a legal action, mixed as it is with its intention and its effects, and refracted as it is through a framework of law. Further, that item of scientific information in the case has a history, of how and by whom it was produced; its quality as evidence is strongly dependent on the details of that history. No matter how "objective" the information, if its production is seen to have violated the relevant laws of evidence, it is of no value in the argument of the case.

And there can be a subsequent history in the courts, both of the "fact" embodied in the verdict and of the scientific information that functioned as evidence in its establishment. Although the processes of appeal are mainly concentrated on interpretations of the law, occasionally the scientific materials themselves come up for challenge and review. In English criminal procedure, the jargon is that when evidence loses too much strength on reexamination, the conviction becomes unsafe. In some celebrated cases, the status of convicted IRA terrorists changed suddenly when, after years of sustained work by solicitors, the terrorists were realized to have been unjustly treated.[45] In some cases, the whole system of English criminal justice had been tainted by the corruption that was revealed retrospectively to have occurred.[46]

D. Differences: Informality of Procedure

As it happens, all those processes and judgments are carried out in all the other sorts of science. But in other sorts of science, colleagues in the same field proceed and judge largely informally, even to some extent unselfconsciously. An important part of the skill of managing scientific literature is knowing what can be safely ignored. Scientists must make instant rough judgments, based on author, funder (particularly if it is corporate food or pharma), sponsoring institution, and journal of publication. Doubtlessly, much worthy and worthwhile work suffers oblivion in this way, but some selection process, inevitably imperfect, is necessary. In this case it is done largely informally; there is no tribunal to which an aggrieved scientist can complain that he has been unfairly discriminated against when his publication has not received adequate recognition. In this way, the procedures for quality assessment and sifting in

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45. *See* Craig R. Whitney, *Faith in British Justice is Shaken by Forced Confessions and False Jailings*, N.Y. TIMES, June 2, 1991, at 10 ("[T]he Government acknowledged that forensic tests had been flawed and misleading . . . .").

46. *See* Kevin Toolis, *When British Justice Failed*, N.Y. TIMES, Feb. 25, 1990, at 32 (describing how one set of false convictions "tainted the entire British legal system" and "tarnished some of the loftiest legal and police reputations in England").

science escape the tedium and the recursive traps of litigation on rules of procedure, but there is an obvious price to be paid. The dividing line between rebels and cranks is not always distinct. (Galileo himself straddled it.) The recursive questions of who decides on questions of legitimacy—how, and why, and to whom are they answerable, et cetera—can generally be left in an amiable fuzz, but only so long as the boundaries of science are uncontested in the polity.

Two borderline cases serve to illuminate the real differences of concern here. The first is when scientists complain about their treatment as employees. The other case is closer to the situation in the courts. In regulatory science (particularly in the United States), recommendations for policy must be justified in a proper manner, for they are actually political in nature. The equivalent of due process applies, in various ways and to varying degrees. It has its costs, of course, in the length and inefficiency of the process, and also in the recursive levels of debate over the procedures. At those higher levels, the process can become highly politicized. Thus, the courts are not unique in having their difficulties with formalized rules of evidence and procedure. Whenever there is a requirement for the equivalent of due process, there will be all the problems and dilemmas of formality of procedure.

This comparative study provides a context for analyzing the intervention of the courts in the assessment of scientific information. Of course, that assessment may sometimes appear to be both clumsy and misconceived. The decision in *Daubert* has been the subject of controversy ever since it was published.[47] But such interventions are not always the result of judicial meddling in scientific issues. They are the result of necessity, the consequence of a genuine, key difference between ordinary research science and the courts. As in all products of design, either material or social, it is impossible to optimize on all characteristics. So, in the courts, fairness of procedures is achieved at the price of a growth of litigation and precedent about special sorts of "fairness," which in turn reduces efficiency and even transparency. But, for example, if the alternative to *Daubert* were for judges to have chats with leading scientists in the respective fields for advice on whom to exclude, the situation would be worse.

E. Science and Statistics in the Courts

There does seem to be a great and unbridgeable divide between the courts and science at one point: the science itself. As is commonly known, the principle behind the jury of peers is that nothing relevant to the case is inaccessible to the ordinary person. And if (as has emerged in recent years) some genuinely esoteric knowledge is in play, then the jury is assumed to be able to treat the expert like any other witness and to evaluate the content of the expert's offering

47. *See, e.g.*, Alan E. Tamarelli, Recent Development, Daubert v. Merrell Dow Pharmaceuticals: *Pushing the Limits of Scientific Reliability*, 47 VAND. L. REV. 1175, 1195–99 (1994) ("*Daubert*'s idyllic general observations do not provide a manageable and consistent method for judging the admissibility of expert testimony.").

in terms of the quality of his or her personal delivery.[48] To many observers this must seem perverse. In the United Kingdom there have been calls to exclude juries from other sorts of trials in which technical issues are decisive, as in complex fraud cases.[49] To ignore the chasm between the "common sense" of the courts and the esoteric technicalities of science could appear to be not merely imperfect, but quite fundamentally misguided.

This pessimistic judgment admits two avenues of reply, which apply to different aspects of science as it appears in the courts. First is the content itself, which might appear totally incomprehensible to any but a small corps of highly trained experts. If these were the "facts" to be tried in the courts, the situation would be desperate indeed. But that is not what happens. The technical scientific materials are just part of the evidence, embedded into an argument (on either side) that eventually is (or is not) accepted as establishing the facts to be tried. And what counts in that material is not the intricate detail that is the object of research but rather those aspects that relate directly to the questions of the actions and intentions of particular people. Hence the materials that are adduced in the courtroom are those that are inherently easier to explain and popularize. The task of the scientific expert is thus analogous to that of the science teacher: not to expound on the most arcane details of current frontline research and controversy, but to render the more solidly established materials comprehensible to beginners and outsiders.

Finally is the point where, apparently, no bridging at all can take place: statistical inference. We all know of the contortions participants in the judicial process are forced to undertake in order to make statistics amenable to inexpert minds and discourses. Confusion arises when scientific probabilities are translated into legal practice.[50] And since "probability" is so central to the process of establishing the "facts," this would seem to be a situation in which the courts really need science and in which juries really are incapable of assimilating it for effective use.

An answer to this difficulty has two prongs, one obvious and the other rather less so. The obvious answer has been made by many jurists and commentators. The sense of "probability" invoked in the courts is different from, and much older than, the subject of the current mathematical theories. "Provability" has been a well-known concept in law and theology from medieval times onwards. Seen in this context, one of the most strange and fruitful moves in the Scientific Revolution was the assimilation of that rich concept of "provability" into games of chance.

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48. *See* 31A AM. JUR. 2D *Expert and Opinion Evidence* § 114 (2002) ("There is no rule of law that requires controlling effect or influence to be given to the opinion testimony of expert witnesses.").

49. *E.g.*, David Barrett, *Legal Matters: Jury Could Be Out for Good on Complex Cases*, BIRMINGHAM POST, June 22, 2005, at 2.

50. *See* David W. Barnes, *Too Many Probabilities: Statistical Evidence of Tort Causation*, 64 LAW & CONTEMP. PROBS. 191 (Autumn 2001) (discussing differences between scientific and legal probabilities).

The less-obvious reply is one that draws on my own analysis of probability theory in the context of scientific reasoning. First, if anyone with sufficient basic mathematics and moral courage decides to master the theory of probability and statistics so that they can share in that clarity of reasoning, they are in for a shock. For although there are many formulas and computation systems that experts use routinely for the analysis of sets of data, their meaning turns out to be obscure and contested. The debates between "Frequentists" and Bayesians rival those of the Shia and the Sunni in their intensity. One sometimes gets an odd feeling on witnessing such arguments. Surely this is the sort of thing that should happen in the behavioral sciences, where things are inherently fuzzy, rather than in a mathematical science dealing with hard numbers. But it can happen here, and it does.

A personal explanation for all this takes me to my own, perhaps eccentric, analysis of scientific argument. First, there is the failure of classical philosophy of science to explicate The Scientific Method, whereby scientists achieve truth in their demonstrations. Philosophers have come to see that scientific research is a messy affair; its methods are sort of reality therapy, offering no guarantee of truth or even of steady progress.[51] And this is because as scientists penetrate ever deeper into the structure and workings of the world around us, even into matter in its less-complex organized states, they find not simplicity, but complexity and wonder. Experience and theory taken alone are either blind and empty respectively, or they interact in an intimate, everchanging dialogue.

There was a time when some leading philosophers, and scientists too, believed that mathematics, the science of quantity and magnitude, was both fundamental and simple. From Descartes and Hobbes onwards, mathematics functioned as the exemplar of assured knowledge, which would lend its certainties to any natural science that was cast in its abstract language. But after a century of "foundational" studies,[52] whatever the status of mathematical knowledge is, it is no longer simple and it had better not be fundamental!

It could be that Probability & Statistics is now due for a similar critical examination. This analysis should be made available to outsiders who use, or who are used by, its reasonings. It seems this is a field of reasoning that could never be made clear, for it needs to treat human judgments and volitions in the terms of abstract formalisms and their algebraic combinations. (But I may be prejudiced!) What needs to be realized by both theorists and users is that they are all in the same boat: they are faced with a mass of very refractory material, reflecting the grapplings of the human intellect with things that are unknown and unknowable. Every sort of paradox and contradiction will appear there. Indeed, in its attempt to formalize patterns of reasoning, thereby preventing the fuzz and fudge whereby ordinary scientific argument works, Probability &

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51. *See* HAACK, *supra* note 23, at 123–49.

52. Wikipedia.org, Foundations of Mathematics, http://en.wikipedia.org/wiki/Foundations_of_mathematics (last visited Jan. 4, 2009).

Statistics painfully expose all the underlying contradictions of our concepts and our inferences.

This discussion may appear to claim that Probability & Statistics is inherently no more capable of total clarification than the common law. For some, that is a dismal prospect; but for others it may be a liberation. All the great, fruitful advances in statistical technique were made in the context of practice, when new ways of looking at numbers were needed. Their theoretical justifications, or rationalizations, came later. But when students and practitioners remain in ignorance of the practical roots of their discipline, it becomes fatally easy for them to have a merely manipulative understanding of it and to forget the need for the craft skills of quality control in its applications. Making real sense of sets of numbers is not achieved by pressing a few buttons on the SPSS package (Statistical Package for the Social Sciences).[53] Still less will such an approach resolve the issues of uncertain knowledge in the courts.

This lengthy excursion was devoted to showing that there is no great chasm between the qualitative, complex reasonings of the courts and the supposedly clear and distinct logic of science as exemplified in Probability & Statistics. Of course there are difficult technicalities, and of course there are anomalies and paradoxes, both apparent and real. But, while outsiders (as those in the courts) look on, mystified by the formulas that seem to bear necessary truth, the insiders wallow in argument just like the practitioners of any other interesting discipline.

III

CONCLUSION

What sort of conclusion can be drawn from all this? The differences between the conventions of scientific and legal settings are not so absolute as they may seem when science is viewed from the outside, and through a certain idealized image. Of course there are pronounced differences in procedure and etiquette, as there would be between any two large social institutions defined by quite different sets of societal functions. However opaque scientific reasoning may appear to the nonspecialists, it involves no special, truth-giving quality, least of all in Probability & Statistics.

There is a more general concluding point, one that has actually motivated the writing of this essay. If science is governed by conventions that make it a perfect endeavor by honorable persons dedicated to the achievement of Truth, then it is extremely difficult to imagine how it could ever be misused and corrupted. Yet I am not alone in having witnessed, exposed, and analyzed the misuse and corruptions of science.[54] It leaves one wondering whether there is a gap between an insider's private, shared wisdom about the imperfections of

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53. SPSS, Corporate History, http://www.spss.com/corpinfo/history.htm (last visited Jan. 4, 2009).

54. *See*, *e.g.*, David Kriebel, *How Much Evidence Is Enough: Conventions of Causal Inference*, 72 LAW & CONTEMP. PROBS. 121 (Winter 2009).

science and an outsider's official public image of science that assumes its perfections. It would appear that the Periclean vision of science, as interpreted in the Mertonian theory of four norms, still influences these images, in spite of the decades of demystification.[55] Both the practices and the working norms of different forms of science are as diverse as their tasks and contexts. The boundary between science and the courts, while sharp in some respects, is by no means all smooth and linear.

My own philosophy of science has had among its aims the elucidation of how science can indeed be misused and corrupted.[56] It is a task of some delicacy; demystification and muckraking, once launched, cannot always be contained within the bounds of decorum and propriety. People will resent and resist the felling of their idols; and in their pain, when that happens, they may strike in all directions. Right now in the United States, we may be approaching the agonizing predicament of reformers in other spheres: whether, and then how, to defend worthy institutions from unprincipled attack when they may already be compromised and partly corrupted. Or one may wonder how to appeal to ideals of good behavior that may already be overtaken by hypocrisy and cynicism. These are difficult times indeed, and the problems of the use of science in the courts may not be the most salient of all just now. But, perhaps because it does lend itself to a detached analysis, this particular issue can show us how to achieve a synthesis of realism and idealism in the understanding and improvement of an important area of practice of science.

55. MERTON, *supra* note 10, at 267–78.

56. RAVETZ, *supra* note 8, at 418–22; RAVETZ, *supra* note 29, at 14.