

# A Non-Romantic View of Expert Testimony

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## I. INTRODUCTION

The public mind [in 1850s New Orleans] is bewildered by the contradictory opinions given by the Engineers in the state as to what ought and ought not to be done. One says cut-offs is the only means of protecting the country. Another says cut-offs will ruin the country, [so] make levees only. . . . A third says make outlets. Each one quotes opinions of foreign engineers and partial facts and pretended facts respecting the Mississippi [River] to support his views. No wonder the legislature does nothing.<sup>1</sup>

Last year, on the occasion of the tenth anniversary of *Daubert v. Merrell Dow Pharmaceuticals, Inc.*,<sup>2</sup> Seton Hall University School of Law held a symposium entitled, “Expert Admissibility: Keeping Gates, Goals and Promises” [hereinafter “Seton Hall Symposium”]. Thereafter, the *Seton Hall Law Review* published the symposium’s proceedings in two issues, wherein numerous leading evidence scholars, as well as practitioners and a judge, offered assessments of current courtroom expertise jurisprudence.<sup>3</sup> Given the flurry of scholarship that arose immediately following *Daubert* and the other two important opinions that, along with *Daubert*, comprise the *Daubert* trilogy (*General Electric Co. v. Joiner*<sup>4</sup> and *Kumho Tire Co. v. Carmichael*<sup>5</sup>)

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<sup>1</sup> JOHN M. BARRY, RISING TIDE: THE GREAT MISSISSIPPI FLOOD OF 1927 AND HOW IT CHANGED AMERICA 42 (1997) (quoting comments of Andrew Atkinson Humphreys) (ellipsis in original) (internal quotation marks omitted).

<sup>2</sup> 509 U.S. 579 (1993).

<sup>3</sup> Expert Admissibility Symposium: *Reliability Standards—Too High, Too Low, or Just Right?*, 33 SETON HALL L. REV. 881 (2003); Expert Admissibility Symposium: *What Is the Question? What Is the Answer? How Should the Court Frame a Question to Which Standards of Reliability Are to Be Applied?*, 34 SETON HALL L. REV. 1 (2003) (proceedings of February 21-22, 2003, Symposium).

<sup>4</sup> 522 U.S. 136 (1997).

<sup>5</sup> 526 U.S. 137 (1999).

(the “trilogy”), one might have expected the Seton Hall Symposium to be a celebration of clarity and progress. This expectation, however, went unmet. Instead, *Daubert* has spawned a series of intense debates and controversies concerning each of the trilogy opinions—debates over the types of evidence that are, should be, or should not be, admissible in court; the role of judges and juries regarding expertise; and proposed reforms.

For example, one symposium participant argued that “*Daubert* is the right [admissibility] standard because . . . [t]he central issue is scientific ‘validity,’ and the criteria suggested by *Daubert* are useful in resolving that issue.”<sup>6</sup> That argument was met with the view that the court’s “fundamental error” in adopting “science” as a legal category immediately gave rise to uncertainty as to whether *Daubert* “had made it more or less difficult for expert testimony to gain admission, a harbinger of the confusion that now surrounds the whole subject of admissibility of expert testimony.”<sup>7</sup> Likewise, the abuse-of-discretion standard for appellate review of admissibility decisions, confirmed in *Joiner*, was attacked both by critics who support a change to allow “reviewing courts to appraise claims of error in applying *Daubert* on a *de novo* basis”<sup>8</sup> (as nine states have done),<sup>9</sup> and by critics who argue that plenary review of a federal trial court’s evidentiary ruling to exclude experts is already required on appeals from summary judgments or directed verdicts.<sup>10</sup> *Kumho Tire*, the third case in the trilogy, was on the one hand praised for “making clear the . . . gatekeeping obligation in regard to non-science” and the “proper approach . . . even in regard to . . . science.”<sup>11</sup> On the other hand,

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<sup>6</sup> Christopher B. Mueller, *Daubert Asks the Right Questions: Now Appellate Courts Should Help Find the Right Answers*, 33 SETON HALL L. REV. 987, 989 (2003).

<sup>7</sup> John H. Mansfield, *An Embarrassing Episode in the History of the Law of Evidence*, 34 SETON HALL L. REV. 77, 81 (2003).

<sup>8</sup> See Mueller, *supra* note 6, at 1023 (recommending new approach).

<sup>9</sup> See *id.* at 1019 (“[N]ine states and the District of Columbia instruct appellate courts to review rulings admitting or excluding evidence presented by science by applying a *de novo* standard.”).

<sup>10</sup> See The Honorable John J. Gibbons, *Tenth Anniversary of the Supreme Court’s Decision in Daubert v. Merrell Dow Pharmaceuticals, Inc.: The Respective Roles of Trial and Appellate Courts in Daubert-Kumho Rulings*, 34 SETON HALL L. REV. 127, 134 (2003) (“This is my own view. It is not . . . the view of the United States Supreme Court.”); see also *In re Japanese Elec. Prods. Antitrust Litig.*, 723 F.2d 238, 277 (3d Cir. 1983) (holding that in summary judgment case, *in limine* evidentiary rulings to exclude evidence “involved fundamental legal error” requiring plenary review), *rev’d on other grounds*, *Matsushita Elec. Indus. Co. v. Zenith Radio Corp.*, 475 U.S. 574 (1983).

<sup>11</sup> Mark P. Denbeaux & D. Michael Risinger, *Kumho Tire and Expert Reliability: How the Question You Ask Gives the Answer You Get*, 34 SETON HALL L. REV. 15, 31 (2003).

*Kumho Tire* was also characterized as a “mismatch between tool and task” (instructing “lower courts to apply standards that simply do not apply”)<sup>12</sup> and as an “embarrassing episode” in the history of evidence law,<sup>13</sup> because it is too restrictive (i.e., “relevance is not enough” under *Kumho Tire*).<sup>14</sup> Moreover, because *Kumho Tire*’s case-specific evaluation “conflicts with widely-accepted methods of gauging validity, and guarantees that we cannot develop consistent or useful precedent,”<sup>15</sup> some have viewed it as a departure from “bedrock scientific principles.” More specifically, the reliability standards developed in the *Daubert* trilogy were, under various formulations, too high in civil cases,<sup>16</sup> too low for prosecutors<sup>17</sup> and many forensic scientists,<sup>18</sup> and too high for criminal defendants.<sup>19</sup> Furthermore, at least in practice, the reliability standards were too low for police “experts” and too high for social scientists.<sup>20</sup> Finally, some justified *Daubert* gatekeeping on the basis that jurors struggle with complex cases and statistical evidence,<sup>21</sup> or that a decision “to admit expert testimony will seem to the jury to be some kind of endorsement.”<sup>22</sup> Still others pointed out that “statements that jurors render inaccurate verdicts are not supported by much empirical evidence,”<sup>23</sup> and that there “is simply ‘no evidence that juries are incompetent to evaluate expert testimony.’”<sup>24</sup> In this robust discourse, although we read the

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<sup>12</sup> Ronald J. Allen, *Expertise and the Supreme Court: What is the Problem?*, 34 SETON HALL L. REV. 1, 2 (2003).

<sup>13</sup> Mansfield, *supra* note 7, at 84.

<sup>14</sup> *Id.* at 85.

<sup>15</sup> Joëlle Anne Moreno, *Eyes Wide Shut: Hidden Problems and Future Consequences of the Fact-Based Validity Standard*, 34 SETON HALL L. REV. 89, 96 (2003).

<sup>16</sup> See Neil B. Cohen, *The Gatekeeping Role in Civil Litigation and the Abdication of Legal Values in Favor of Scientific Values*, 33 SETON HALL L. REV. 943, 960 (stating that “law should allow in conclusions that science filters out”).

<sup>17</sup> See Paul C. Giannelli, *The Supreme Court’s “Criminal” Daubert Cases*, 33 SETON HALL L. REV. 1071, 1074 (asserting that exacting standards required in civil litigation are not being applied to criminal cases).

<sup>18</sup> See generally Michael J. Saks, *The Legal and Scientific Evaluation of Forensic Science (Especially Fingerprint Expert Testimony)*, 33 SETON HALL L. REV. 1167 (2003).

<sup>19</sup> See generally Christopher Slobogin, *The Structure of Expertise in Criminal Cases*, 34 SETON HALL L. REV. 105 (2003).

<sup>20</sup> See Jennifer L. Groscup & Steven D. Penrod, *Battle of the Standards for Experts in Criminal Cases: Police vs. Psychologists*, 33 SETON HALL L. REV. 1141, 1147 (2003) (“Our suspicion is that . . . police officers are viewed as inherently reliable by courts.”), *id.* at 1148 (“[C]ourts and commentators have been highly critical of psychologists testifying as experts.”).

<sup>21</sup> See Joseph Sanders, *The Merits of Paternalistic Justification for Restrictions on Admissibility of Expert Evidence*, 33 SETON HALL L. REV. 881, 901 (2003).

<sup>22</sup> Saks, *supra* note 18, at 1170.

<sup>23</sup> Mansfield, *supra* note 7, at 86.

<sup>24</sup> Michel F. Baumeister & Dorothea M. Capone, *Admissibility Standards as*

trilogy differently than some do, we find much with which to agree. Conversely, we also find some critiques of the trilogy less than compelling.

Elsewhere, we have offered a defense, of sorts, of the *Daubert* trilogy, or at least an interpretation of how the standards (for evaluating expertise) that emerge from the trilogy can and do work for judges and lawyers.<sup>25</sup> Briefly, there are clues in Justice Blackmun's *Daubert* opinion that the so-called four-factor test for scientific validity, because it does not constitute "a definitive checklist or test,"<sup>26</sup> should not be overemphasized.<sup>27</sup> Instead, the four factors are merely general observations, and "many [other] factors" will bear on the determination that valid science was properly applied.<sup>28</sup> Moreover, the requirement that trial judges must decide, as a preliminary matter, whether the "methodology properly can be applied to the facts in issue"<sup>29</sup> tempered the recommended focus on "methodology, not . . . conclusions."<sup>30</sup> *Joiner* resolved that apparent inconsistency. There, the trial judge's emphasis on proper application (to the case at hand) was approved on the basis that "conclusions and methodology are not entirely distinct from one another."<sup>31</sup> That is, even when an expert's methodology is scientific, an "analytical gap between the data and the opinion proffered"<sup>32</sup> may persist. For scientists and non-scientists alike, the emphasis on application was confirmed in *Kumho Tire*, as was the flexibility of the four factor "test" in *Daubert*, which "neither necessarily nor exclusively applies to all experts in every case. . . . [S]cientific foundations . . . will be at issue in some cases. [But] in other cases, the relevant reliability concerns may focus upon personal knowledge or experience. . . . Too much depends upon the particular circumstances of the particular case at

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*Politics—The Imperial Gate Closers Arrive!!!*, 33 SETON HALL L. REV. 1025, 1041 (2003) (quoting Neil Vidmar & Shari Seidman Diamond, *The Jury in the Twenty-First Century: An Interdisciplinary Conference Article: Juries and Expert Evidence*, 66 BROOK. L. REV. 1121, 1175 (2001)).

<sup>25</sup> See generally David S. Caudill & Lewis H. LaRue, *Why Judges Applying the Daubert Trilogy Need to Know About the Social, Institutional, and Rhetorical—and not Just the Methodological—Aspects of Science*, 45 B.C. L. REV. 1 (2003) (approving of federal appellate courts' generally pragmatic approach toward science).

<sup>26</sup> *Daubert*, 509 U.S. at 593-94 (outlining four factors: testability, peer-reviewed publication, low error rate, and general acceptance).

<sup>27</sup> *Id.* at 593.

<sup>28</sup> *Id.*

<sup>29</sup> *Id.*

<sup>30</sup> *Id.* at 595.

<sup>31</sup> See *Joiner*, 522 U.S. at 155.

<sup>32</sup> See *id.* at 146.

issue.”<sup>33</sup> One could thus read *Kumho Tire* narrowly as holding that it is relevant for engineers and not “real scientists.” But much like engineering, whenever science comes into the courtroom, it is as applied science, not pure theory. For example, in determining whether the blood at the crime scene is the defendant’s, the court moves from pure theory to a laboratory technician to the expert, who combines theory, lab results, personal observations, and informed judgments that can aid the trier of fact.

Accordingly, as a whole, the *Daubert* trilogy deflects attention away from abstract identifications of scientific validity, including the “demarcation” controversy concerning the elimination of alleged “junk science” from the courtroom. Instead, attention is directed toward the application of expertise to the particular “case at hand.” This emphasis on application is reflected as well in the Seton Hall Symposium proceedings, which offer three patterns or contours that provide useful guidance to judges and lawyers. First, there is a pragmatic recognition, in various forms, that the focus should be on how science is being used rather than on science in the abstract. Second, that focus must be accompanied by a modest view of science rather than an idealized version of its capacity to produce knowledge for law. Third, the focus on the application phase of expertise must also be accompanied by a modest view of law itself, including judges, lawyers, juries, and the evidentiary rules. In the post-trilogy series of debates, it is far too easy to romanticize the power of science, or the virtues of the legal system, or both, and to fail to recognize their practical limitations. Just as romantic images of law often rely on demonizations of judges untrained in science, overzealous lawyers, or emotional, uncritical and confused jurors, romantic images of science are often bolstered by demonizations of forensic scientists, plaintiffs’ experts, or social scientists. Thus, the pragmatic emphasis on application must be mediated by pragmatic views of both science and law. Fortunately, the pragmatic aspects of science and law—which we associate with their local, social, rhetorical, and institutional features—are most visible in the focus on application. Nevertheless, the limitations of law and science often recede into the background; as a result, undue attention is given to red herrings and unrealistic reform proposals.

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<sup>33</sup> See *Kumho Tire*, 526 U.S. at 141, 150.

## II. THE ACTION IS IN THE APPLICATION

[T]he answer to what question is to be asked of the expert post-*Kumho* is precisely whatever questions should have been asked post- (and for that matter pre-) *Daubert*, to-wit: Does the expert in fact possess knowledge useful to this trial that is being brought to bear upon it in a way that increases the probability of accurate outcomes?<sup>34</sup>

The proper emphasis on application in determining the admissibility of expert testimony is epitomized in the phrase “brought to bear.” One must focus on the way science is used in the courtroom, not on science or law in the abstract. Otherwise, there is a risk, in post-trilogy legal discourse, that one’s scholarly analysis or reform proposal will “smell of the lamp” and be of no use in the rather rough area that is a trial—that which looks elegant and symmetrical in the study can look deformed in the courtroom.

In contemporary post-trilogy discourse, the focus on application takes numerous forms. For example, Professors Gross and Mnookin, after noting that “thousands of pages have been written about both the proper [threshold] criteria for evaluating the reliability of expert evidence and the institutional competence of judges to evaluate scientific reliability,” recommend that we examine “another dimension: the degree of certainty that the expert posits in what she offers.”<sup>35</sup>

One of the central problems with much expert testimony introduced in court—both scientific and non-scientific alike—is that experts claim as matters of fact or probability opinions that should be couched in more cautious terms, as possibilities or hypotheses.

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... Often, whether testimony is based on scientific study or more casual forms of observation, what makes an expert’s conclusion unreliable is that it is expressed with a confidence not warranted by the evidence.<sup>36</sup>

That emphasis on levels of confidence is echoed in Professor Berger’s sense that “*Daubert* overemphasizes how the data underlying the expert’s opinion was produced and distracts courts and counsel from carefully analyzing what the evidence proves, and how it is being

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<sup>34</sup> Allen, *supra* note 12, at 7.

<sup>35</sup> Samuel R. Gross & Jennifer L. Mnookin, *Expert Information and Expert Evidence: A Preliminary Taxonomy*, 34 SETON HALL L. REV. 141, 143 (2003).

<sup>36</sup> *Id.* at 143-44.

used. *Daubert* stresses the medium over the message.”<sup>37</sup>

While we agree that careful analysis of what the evidence proves (and how it is used) is fundamental, we disagree that *Daubert*, especially as interpreted in the remainder of the trilogy, detracts from that task. Assuming that *Daubert*'s four factors are not “a definitive checklist,” and that judges must decide whether “methodology properly can be applied to the facts in issue,”<sup>38</sup> the methodological *medium* is neither overemphasized nor disconnected from the expert's *message*. To illustrate, the “peer review and publication” factor is not determinative of admissibility because, as Professor Moreno points out, that factor in the abstract “tells us nothing about . . . whether the validity of the published methods or conclusions is [relevant to] the manner in which this expert proposes to *use* the theory or technique to make inferences or draw conclusions in this case.”<sup>39</sup> Moreover, even some scholars who disagree over whether *Daubert* as applied is too restrictive or not restrictive enough agree on the need to focus on the application phase. For example, Professor Saks, who is concerned that the value of much forensic science continues to be exaggerated, summarizes the elemental conditions of admissibility of expert evidence as follows: “(a) the opinions and conclusions of the expert are accompanied by information that enables the factfinder to evaluate the likely accuracy of the expert's opinion, and (b) the information is presented in such a way that factfinders will not . . . excessively [overvalue] the testimony.”<sup>40</sup> Likewise, Professor Friedman—who, in contrast with Professor Saks, criticizes the *Daubert* regime as overly exclusionary—nevertheless recommends that “in some settings . . . courts should admit expert evidence but explain to the jury factors limiting the weight that the jury should accord the evidence. . . . Sometimes the . . . court[s] should . . . comment adversely on it.”<sup>41</sup> Therefore, reliability in the abstract may not be as important as whether “the expert witness over-claimed the significance of the . . . result” of forensic scientific inquiry.<sup>42</sup> All of these variable expositions

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<sup>37</sup> Margaret A. Berger, *Expert Testimony in Criminal Proceedings: Questions Daubert Does Not Answer*, 33 SETON HALL L. REV. 1125, 1125 (2003).

<sup>38</sup> See *Daubert*, 509 U.S. at 593.

<sup>39</sup> Moreno, *supra* note 15, at 99.

<sup>40</sup> Saks, *supra* note 18, at 1167.

<sup>41</sup> Richard D. Friedman, *Squeezing Daubert Out of the Picture*, 33 SETON HALL L. REV. 1047, 1048 (2003).

<sup>42</sup> *Id.* at 1063; see also Roger C. Park, *Daubert on a Tilted Playing Field*, 33 SETON HALL L. REV. 1113, 1114 (2003) (“I share Professor Friedman's hope that better testimony about the limits of forensic science testimony, accompanied by thoughtful instructions, will lead to better results.”).

on confidence levels—the manner in which evidence is used, and whether experts over-claim or juries overvalue—emphasize the application phase of expertise.<sup>43</sup>

Assuming that one focuses on how science is “brought to bear” in the courtroom, and that one takes a modest and non-romantic view of both law and science, what are the principal problems and greatest dangers that we face? Where could improvements be made that will lead to more accurate decisions? With these questions in mind, several suggestions made during the Seton Hall Symposium appear promising. For example, Professor Nance observed that current “practice is often overly generous to proponents in allowing opinion on case-specific material facts, ultimate or not, when those facts are not within the personal knowledge of the expert.”<sup>44</sup> In a similar vein, Professor Berger recommended that because “[a]dmissibility and sufficiency determinations rest on more than

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<sup>43</sup> An emphasis on application, rather than on abstract definitions of scientific reliability, does not necessitate the creation of a “legal science” that does not correspond to science itself. Professor Nance, for example, appropriately demystifies the concepts of reliability, testing, and error rates by showing that they are each matters of degree. Instead of stabilizing an admissibility decision, each simply generates a question about how much reliability, how much testing (and of what quality), and how low an error rate is required in the courtroom. See Dale A. Nance, *Reliability and the Admissibility of Experts*, 34 SETON HALL L. REV. 191, 199-201 (2003). Nance’s next step, however, is to question why “the final determination . . . be determined by the norms of the scientific community instead of those of the legal community . . . ?” *Id.* at 203. Like Joe Cecil, he avoids the “ephemeral” search for scientific validity by recommending that courts establish “a legal threshold for sufficiency that [is] independent of any uniform scientific standard.” Joe S. Cecil, *Construing Science in the Quest for “Ipse Dixit”: A Comment on Sanders and Cohen*, 33 SETON HALL L. REV. 967, 985, 986 (2003). That task is neither necessitated by the focus on application nor particularly helpful to the legal process. While “the reliability criterion should be relative,” and courts certainly must make policy decisions as to what level of reliability, testing, or error rates are required in court, “the evidentiary determination regarding expert evidence must take into account—it must integrate into its foundational premises—the culture of the scientific method. . . . This needs to be understood as a matter of science policy.” David L. Faigman, *Expert Evidence in Flatland: The Geometry of a World Without Scientific Culture*, 34 SETON HALL L. REV. 255, 258-59 (2003). In light of our argument that the focus on application only works when it is accompanied by a modest view of science (and of law) as a local, pragmatic enterprise, this is an especially significant point. We are in complete agreement with Professor Nance’s observation that “scientific validity is not an all-or-nothing characteristic.” Nance, *supra*, at 200. We also completely agree with Joe Cecil’s identification of “the diverse views and values that characterize the scientific academy.” Cecil, *supra*, at 985. Therefore, we see no reason to disregard the pragmatic culture of science in favor of another, and undoubtedly lesser, “legal” science. Indeed, the basis for such disregard is either an idealization of science as too good for law (i.e., its standards are too high), or an idealization of law as somehow better than science.

<sup>44</sup> Nance, *supra* note 43, at 242-43.

satisfaction of a reliability component[,] they require careful attention to what the evidence proves and how the trier of fact will use it.”<sup>45</sup> With respect to fingerprint identification expertise, Professor Saks confirmed that:

a court must determine what the fingerprint comparison problem is (a clear and complete latent print versus a tiny fragment versus a montage of numerous overlaid smeared latents, etc.) and whether the data show that the expert is likely to be able to perform that particular type of examination accurately. Under [*Kumho Tire*], a court is not to ask about a field in a general and global way.<sup>46</sup>

Hence, the recommendation by Freidman and others, mentioned above,<sup>47</sup> that “[j]udicial comment, expressing reasons to limit the significance of the evidence, [is sometimes] appropriate.”<sup>48</sup>

To be sure, the task of improving the way in which science is “brought to bear” is not trivial. Rather, it is worth our best efforts to generate creative suggestions to aid the bench and bar with the application phase of expertise in the courtroom. Many scholars, in criticizing existing practices and in their proposals for reform, acknowledge the significance of the application phase. Nevertheless, many still fail to possess modest expectations of both science and law. Too many academics idealize either law or science—respectively demonizing, on the one hand, courtroom experts, and on the other hand, judges, lawyers, and juries. Given that law and science are local and cultural enterprises with practical goals and limitations, a non-romantic, pragmatic approach to both is appropriate.

Idealizing law and/or science prevents one from focusing on the most important problem associated with the use of science (and other evidence) at trials. Our own view of the matter accords with that expressed in *Schafersman v. Agland Cooperative*,<sup>49</sup> where the Nebraska Supreme Court was

convinced that by shifting the focus to the kind of reasoning required in science—empirically supported rational explanation—the *Daubert/Joiner/Kumho Tire Co.* trilogy of cases greatly improves the reliability of the information upon which verdicts and other legal decisions are based. Because courts and juries cannot do justice in a factual vacuum, the better information the fact finders have, the more likely that verdicts will

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<sup>45</sup> Berger, *supra* note 37, at 1140.

<sup>46</sup> Saks, *supra* note 18, at 1176.

<sup>47</sup> See *supra* notes 41-42 and accompanying text.

<sup>48</sup> Friedman, *supra* note 41, at 1064.

<sup>49</sup> 631 N.W.2d 862 (Neb. 2001).

be just.<sup>50</sup>

What we like most about this statement, brought to our attention by Professor Sanders,<sup>51</sup> is its definition of the scientific method as “empirically supported rational explanation.” Notably, this form of reasoning should also be at the core of law, journalism, history, sociology, and any other form of thought that, however distant the support and uncertain the conclusions, purports to rest conclusions on facts. This sort of modest, realistic assessment of what science (and law) should hope to achieve is the only plausible way to identify the true problems that arise from the use of scientific evidence in court. Conversely, idealistic pictures of law and science stand in the way of understanding the real problems.

Furthermore, abstract theorizing about “what is science?” does not seem profitable. After all, a group of law professors, lawyers, and judges has a rather remote chance of successfully identifying a set of useful and cogent criteria that would demark science from non-science.<sup>52</sup> As noted above, we do not think that Justice Blackmun intended to construct a definition of science; furthermore, the crucial precedents do not seem to turn on that inquiry. For example, in *Daubert* on remand, *Joiner*, and *Kumho Tire*, the judges never stated that the excluded testimony was the product of “junk science.” In each of these cases, the judges accepted that the field of expertise that formed the basis of the excluded testimony was wholly legitimate.<sup>53</sup> On the other hand, in each of these cases, the judges determined that the application of the expertise, that is, the way in which the expertise was “brought to bear,” was dubious.

In the next section, we begin by identifying some idealizations of

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<sup>50</sup> *Id.* at 876.

<sup>51</sup> See Sanders, *supra* note 21, at 938 n.246.

<sup>52</sup> See Mansfield, *supra* note 7, at 81 (“The truth is that . . . there is no clear understanding and agreement about what is meant by ‘science,’ ‘good science,’ or ‘the scientific method.’”).

<sup>53</sup> In *Daubert* on remand, plaintiff’s experts were all deemed to be “experts in their respective fields”; the “animal studies, chemical structure analyses and epidemiological data” on which they relied were not in question. One expert, however, failed to show how his conclusion that Bendectin causes limb defects was reached. The others could not show causation, but merely a possibility, *in this case*. See *Daubert v. Merrell Dow Pharm., Inc.*, 43 F.3d 1311, 1317-22 (9th Cir. 1995). Likewise, in *Joiner*, the animal studies and the four epidemiological studies which the plaintiff’s experts relied on were not in question, though a gap existed between that data and the expert opinion on causation. See 522 U.S. at 144-46. Finally, in *Kumho Tire*, the issue “was not the reasonableness *in general* of a tire expert’s use of a visual and tactile inspection . . . . Rather, it was the reasonableness of using that approach . . . to draw a conclusion regarding *the particular matter to which the expert testimony was directly relevant.*” 526 U.S. at 153-54.

science in recent post-trilogy scholarship. In our analysis of each, we explore the manner in which romantic images of science deflect attention away from the application phase, resulting in proposals for reform that are unworkable. Along the way, we also set forth the contours of a modest view of science.

### III. MODEST VERSUS IDEALIZED VIEWS OF SCIENCE

The closer the empirical focus on the actual workings of science, and the more current and uncertain the area of science examined, the more difficult it is to identify simple ideal models of methods and norms.<sup>54</sup>

Certain idealizations of science are easier to detect than others. To be sure, it is difficult, in light of scientific progress, to locate persons who view science as merely a social or cultural phenomenon, not unlike religion or mythology. Similarly, it is not easy to find persons who view the scientific enterprise as simply an accumulated body of objective, universal, timeless truth. Nevertheless, between those extremes, judges and commentators may expect too much from science in the courtroom. The basis of such expectations is often a subtle idealization of the scientific enterprise. Although science is best characterized both by (i) its methodological rigor and technically efficacious outcomes, and (ii) its social, institutional, and rhetorical features, an undue focus on the former characteristics can deflect attention away from the latter.

Baumeister and Capone's account of post-trilogy expertise, for example, begins with a modest view of science in law—probabilistic, sometimes shaky, and often uncertain.<sup>55</sup> Their essential argument, that toxic tort plaintiffs are unfairly disadvantaged by post-trilogy reliability requirements, concludes by drawing an analogy between plaintiffs' experts and Copernicus, whose novel "theories and research [might not] find their way past the admissibility gates of an increasingly imperial judiciary applying a *Daubert* analysis."<sup>56</sup>

Early in the sixteenth century, great thinkers of the Western World unanimously believed that the earth lay at the center of the universe . . . . So deeply held was this belief, that it was considered heresy to think otherwise. But one man dared to

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<sup>54</sup> Gary Edmond & David Mercer, *Trashing "Junk Science,"* 1998 STAN. TECH. L. REV. 3, ¶ 44.

<sup>55</sup> See Baumeister & Capone, *supra* note 24, at 1032 (stating that epidemiological and toxicological studies are probabilistic), 1033 ("[E]pidemiological and toxicological studies are inherently incapable of establishing causation to a certainty.").

<sup>56</sup> *Id.* at 1046.

believe otherwise. Nicolaus Copernicus, sitting alone in a turret and using just his eyes . . . sketched and re-sketched his celestial observations.<sup>57</sup>

This romantic picture, offered with a citation to, *inter alia*, an *Irish Times* article,<sup>58</sup> unwittingly represents science as the product of an individual, standing alone against a community of seemingly great thinkers; the role of social institutions is either negative or non-existent. In this story, the social aspects of science belong to the mainstream scientific community, where strong commitments, leaps of faith, omission of counterarguments, political strategies, and religious devotion hinder scientific discovery and progress. Unfortunately, Copernicus was not alone, did not use only *his* eyes, had strong commitments, engaged in leaps of faith, omitted counterarguments, and was both politically astute and religious.<sup>59</sup> The analogy breaks down because the social, institutional, and rhetorical aspects of science are not simply impediments to overcome; inevitably, they are aspects of good science. That is, they make the best science possible. Naturally, in certain cases, social factors—such as fraud due to ambition, laboratory carelessness, and refusal to acknowledge data that contradict a favored theory—serve as barriers to scientific progress. Nevertheless, the “social” is a feature of both science’s failures and successes. Significantly, scientific progress relies on social interaction, institutional support, and rhetorical strategies, including consensus-building techniques, persuasion, and governing metaphors and models.

Recognition of science’s social, institutional, and rhetorical aspects, not as a critique of scientific methodology but rather as an appreciation of the pragmatic limitations of scientific expertise, would lead commentators to a modest view of the scientific enterprise. Conversely, the failure to recognize the inevitable social

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<sup>57</sup> *Id.* at 1045.

<sup>58</sup> See *id.* n.134 (citing Brendan McWilliams, *Copernicus and the Centre of the Universe*, IRISH TIMES, Feb. 19, 2003, available at 2003 WL 12226971), *id.* n.136, *id.* at 1046 n.141.

<sup>59</sup> See Edmond & Mercer, *supra* note 54, ¶ 63 (“[T]he history of science reveals that it is common for scientists to have strong commitments to their views during the early phases of new research. Evaluated in the context of their own time and [place], the theories of . . . Copernicus relied upon leaps of faith and observations at the threshold of theoretical plausibility, together with the deliberate omission of counter arguments.”); see also STEPHEN F. MASON, A HISTORY OF THE SCIENCES 127-34 (1962) (discussing Copernicus’ reliance on conventional methodology; religious, purposive, and teleological arguments; medieval as well as modern explanations; and promotion of new values). With respect to Copernicus’ rhetorical strategies and his interaction with other scientists, see generally JEAN DIETZ MOSS, NOVELTIES IN THE HEAVENS: RHETORIC AND SCIENCE IN THE COPERNICAN CONTROVERSY (1993).

context of scientific inquiry leads to the subtle idealizations of science that persist in post-trilogy scholarship. Such romanticizations of science come in several forms, including reliance on a “deficit model” of scientific knowledge for law, an overemphasis on methodology as the marker of good science, and a belief that rooting out “junk science” is the primary goal of post-trilogy admissibility standards.

Scholars who rely on a deficit model of science tend to view the courtroom as an institutional space for social conflicts, wherein interested lawyers hire interested experts and attempt to persuade judges and juries who are deficient; that is, they lack scientific knowledge.<sup>60</sup> Science, on the other hand, is viewed as a source of stable, interest-free knowledge. From that perspective, science is not social like law; at its core, science is neither institutional nor rhetorical, but is an adjudicator of social conflicts. Critics of the deficit model, on the other hand, point out that science *is* like law—science is just another community, with its own institutions, language, rhetorical techniques, internal controversies, gatekeeping procedures, and credentialing processes. While few would disagree with that assessment, some scholars in their post-trilogy discourse write as though science is better than that. Professor Moreno, for example, who challenges the “task at hand” approach in *Kumho Tire*, bases her critique on a perceived lack of judicial understanding of basic scientific concepts.<sup>61</sup> We have elsewhere challenged the Gatowski study upon which Moreno relies,<sup>62</sup> precisely because that survey romanticizes scientific methodology, and Moreno’s critique of the case-specific evaluation of science “as applied” becomes a case in point. Instead of recognizing that science itself is a local enterprise with practical goals and limitations even before it enters the courtroom, Moreno seemingly idealizes science as a linear story of progress:

A legal decision that is grounded in fact-specific validity (e.g., a conclusion based on the specific scientific data relied upon by *this* expert to reliably explain *these* facts) is the antithesis of science. Science, in all of its disciplines, is cumulative and based on a continuing aggregation of new data.<sup>63</sup>

Such an exaggerated view of science is surprising for two

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<sup>60</sup> For a brief discussion of the deficit model, see Caudill & LaRue, *supra* note 25, at 7-8.

<sup>61</sup> See Moreno, *supra* note 15, at 96-98 (citing Sophia Gatowski et al., *Asking the Gatekeepers: A National Survey of Judges on Judging Expert Evidence in a Post-Daubert World*, 25 LAW & HUM. BEHAV. 433 (2001)).

<sup>62</sup> See Caudill & LaRue, *supra* note 25, at 8-20.

<sup>63</sup> Moreno, *supra* note 15, at 102 (emphasis added).

reasons. To begin with, the first sentence of the above quotation seems to actually describe scientific or laboratory inquiry at the pre-publication phase—*this* scientist reaches *this* conclusion on the basis of *this* data to explain *these* facts. The second sentence quoted above conflicts with the history of scientific progress: New data is not always cumulative, but often revolutionary.<sup>64</sup> To be fair, Moreno makes the point that an expert potentially can “prove” something to a jury that no other scientist would consider valid. In that case, however, there actually has been no reliable application to a set of facts. Therefore, when Moreno says that “whether a particular scientific theory or methodology has been reliably *applied* to a given set of facts [is] scientifically meaningless,”<sup>65</sup> this only makes sense if a “reliable application” of methodology to given facts is potentially unreliable by reference to something else. That something else, according to Moreno, is *global* reliability, as opposed to *local* reliability. To support that view, Moreno quotes Professor Allen, who observed that a “local” argument in court must rest on a global epistemological warrant. In other words, a condition of “local” testimony is global expertise, and “without global reliability, one has gibberish.”<sup>66</sup> Unlike Moreno, however, Allen does not explicitly say that an application could be locally *reliable* and globally *unreliable*. Allen says that “accurate outcomes locally” must rest on a global warrant, and that global validity is a condition for appropriate local testimony.<sup>67</sup> Using the term “reliable” to refer to an *unreliable* (because local) application, as Moreno does, is a misreading of *Kumho Tire*’s focus on application. Such a misinterpretation can only be explained by a romantic view of science as a global source of cumulative knowledge against which

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<sup>64</sup> See generally THOMAS S. KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (1962).

<sup>65</sup> *Id.* (emphasis added).

<sup>66</sup> *Id.* n.54 (quoting Allen, *supra* note 12, at 6).

<sup>67</sup> See *id.* We even have our doubts about Professor Allen’s formulation of the importance of global reliability. Professor Berger, for example, points out that while microscopic hair analysis can be considered “unreliable because it has not been tested adequately to satisfy *Daubert*[,] trained examiners do have some proficiency in comparing samples accurately . . . .” Berger, *supra* note 37, at 1134. Even Allen qualifies his argument:

When I say that it is necessary to establish the “global” issue of reliability, I am merely saying that virtually all trial testimony will be embedded in or a part of some larger body of knowledge, and that the reliability of the testimony will depend in part on the reliability of the inferentially prior propositions or methodologies involved.

Allen, *supra* note 12, at 10; see also *id.* n.24 (“I suspect that this is true of all testimony, actually.”). Moreover, Allen adds, “[t]he precise contours of the necessary [global] background will depend on the precise testimony directly relevant to the case that is being proffered.” *Id.* at 10.

local scientists check their conclusions. *That*, we think, is the antithesis of science. Science is properly characterized by its conjectures and refutations, and by its willingness to challenge, in local settings, the current body of received scientific knowledge.

Reliance on the deficit model leading to idealization of science is also evident in Professor Sanders' otherwise insightful article on "paternalism" toward the jury in cases involving expert testimony.<sup>68</sup> Arguably, compared to Moreno's, Sanders' idealization is much more subtle and harmless. Given that Sanders seems to view the gatekeeping effort, which keeps confusing and potentially misleading expert testimony away from the jury, as a means to "protect" the jury, his use of the term "paternalism" is problematic. In actuality, the party against whom evidence is offered is the party requiring protection.<sup>69</sup> Given that "shap[ing] another's preferences in ways that bypass the other's capacity to resist"<sup>70</sup> does not sound protective, "paternalism" may simply be poor word choice. In any event, Sanders is concerned with jurors' ability to understand science. Even as he provides a superb summary of the conflicting research on jury confusion, Sanders distinguishes between "central or systematic processing [wherein] people examine the content of a communication to assess its validity," and

peripheral or heuristic processing, [wherein] people do not attend to the quality and validity of arguments. Rather, they adopt shortcuts to determine the value of a message. People rely on factors such as the number of arguments (rather than their quality), the attractiveness of the communicator, and the communicator's credentials.<sup>71</sup>

Throughout the remainder of his article, Sanders focuses on juries' ability to process scientific knowledge. Sanders' analysis, however, presumes that we are trying to deliver scientific knowledge, of the right quality and validity, into this deficient setting known as the jury. That approach seems to lack any critical reflection on the nature of scientific knowledge and discourse. Even among scientists,

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<sup>68</sup> See generally Sanders, *supra* note 21.

<sup>69</sup> Sanders explains that "paternalism exists even when the class of persons whose good is involved is not the same as the class of persons whose freedom is restricted." *Id.* at 897. "Requiring medical doctors to be licensed . . . [is] paternalistic in this sense. [Such regulation is] intended to protect consumers. Admissibility restrictions that are justified because they are best for the parties to the litigation, or for the legal system itself, are paternalistic in the same way." *Id.* at 897-98. Perhaps, then, the exclusionary rules in Sanders' analysis are *not* protecting the jury.

<sup>70</sup> *Id.* at 897 (citing DONALD VANDEVEER, PATERNALISTIC INTERVENTION: THE MORAL BOUNDS ON BENEVOLENCE 19 (1986)).

<sup>71</sup> *Id.* at 909.

both types of processing exist—scientists adopt shortcuts, such as reliance on a fellow scientist’s credentials or fidelity to a paradigm, which the deficit model tends to ignore. Again, rather than viewing science as “content” to be delivered into a deficient receptacle, we prefer to see two communities—one scientific, the other “public”—both characterized by surplus and deficit.

In the end, Sanders finds the evidence inconclusive, despite numerous studies, as to whether reliability requirements are justified to “shelter jurors from their own shortcomings.”<sup>72</sup> Though we contend that reliability requirements shelter *the litigants* from juror shortcomings, we understand Sanders’ point: If jurors struggle with complex scientific arguments such that their decisions are incorrect, judges ought to intervene as gatekeepers. Of course, Sanders concedes, not only is there “no research that addresses” the correctness of decisions “under various admissibility regimes,”<sup>73</sup> but “we probably do not have much agreement about what constitutes a ‘correct’ outcome.”<sup>74</sup> Here, Sanders has stumbled upon the deficit model’s problem.

To illustrate, if a hypothetical observer (who is not a scientist) had to decide whether complex expertise generally confuses and misleads jurors, there is no secure scientific knowledge of which the observer is deficient. Moreover, there is no “central or systematic” process that can attend to the validity of the arguments pro and con. Instead, there is a scientific controversy among social scientists, who, in their respective conclusions, attend not only to methodological standards, but also to persuasiveness, credentials, and consensus. Simply stated, this is the manner in which science works. Scientific practices are

components of craft, or tacit knowledge, over which there is a negotiated consensus for given times and places during settled periods of science. . . . Judgments as to what constitutes “good” . . . science . . . are social judgments open to dispute and negotiation, and are affected by considerations such as the status of relevant scientists [and] their research backgrounds.<sup>75</sup>

Once a controversy in science is settled, the result can be explained, in a sort of *post hoc* revisionism, as a methodological victory, and the social, institutional, and rhetorical aspects of science will recede into the background. In the controversial literature

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<sup>72</sup> *Id.* at 891.

<sup>73</sup> *Id.* at 899.

<sup>74</sup> Sanders, *supra* note 21, at 931.

<sup>75</sup> Edmond & Mercer, *supra* note 54, ¶ 30.

(concerning juries and expertise) discussed by Sanders, however, the *social* is quite visible. Due to Sanders' persistent focus on juror deficits, however, the *social* is also ignored.

Even among scholars who do not fall prey to the deficit model, characterizing science *singularly* as methodological often eclipses science's social, institutional, and rhetorical aspects. Professor Neil Cohen, for example, in his commentary on evidentiary gatekeeping by judges, uses the popular metaphor of the "black box" to describe the process by which data is given to an expert, who draws conclusions by an "analysis [that] takes place out of sight of the factfinders."<sup>76</sup>

An example of that model might be . . . a . . . handwriting expert, who is given samples[,] . . . analyzes them, and pronounces the document [in question] to have been written (or not . . . ) by the defendant. While the expert might recite the factors that lead . . . to the conclusion, the process by which those factors are weighed and balanced, as well as the justification for using those factors and not others, takes place in the expert's mind.<sup>77</sup>

By contrast, for Cohen, "the testimony of epidemiologists and scientists using similar methods . . . is based on expertise that takes place in a 'clear box' in which the entire thought process of the expert can be monitored and assessed."<sup>78</sup> Significantly, however, the metaphor of the black box can be used in many different ways. Among sociologists (of science), the conventional use of the term "black box" is to describe the manner in which scientific discourse hides its social, institutional, and rhetorical aspects.<sup>79</sup> From a scientific perspective, one should focus on hypothesis, data, methodology, and result. Consequently, the "surrounding" or "contextual" factors such as personality, markers of credibility, funding, consensus-building processes, and values are "black-boxed" as insignificant. In Cohen's account of the black box, however, once we focus in court on the thought process of the expert—theory, data, methodology—we now have a "clear box." From the perspective of a sociologist or historian of science, this is a "black-boxing" maneuver. In other words, science *itself* has been idealized as an almost mechanical producer of knowledge, while the expert, along with the

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<sup>76</sup> Cohen, *supra* note 16, at 960.

<sup>77</sup> *Id.*

<sup>78</sup> *Id.* at 961.

<sup>79</sup> See David S. Caudill, *Barely Opening, Then Slamming Shut, Science's "Black Box" in Law: A Response to Beecher-Monas's Heuristics*, 23 CARDOZO L. REV. 1795, 1796 (2002) (citing Langdon Winner, *Social Constructivism: Opening the Black Box and Finding It Empty*, 16 SCI. AS CULTURE 427, 431 (1993)).

expert's social authority, institutional status, and rhetorical strategies, has disappeared. Following his idealization of science, Cohen argues that since scientific standards are too high for law, expert opinions that would not qualify as "real" science should be allowed into court.<sup>80</sup>

Joe Cecil, in his response to Cohen's analysis, adopts an appropriately modest view of science. Cecil acknowledges the ephemerality of "scientific validity" and "the diverse views and values that characterize the scientific academy—one science's accepted methodology may be another science's *ipse dixit*."<sup>81</sup> In his analysis of *Soldo v. Sandoz Pharmaceutical Corp.*,<sup>82</sup> Cecil highlights the disagreements among the three independent experts in the case, whose varying standards resulted in different assessments of causation.<sup>83</sup>

*Soldo* reveals [that a] court must reconcile conflicting values of numerous sciences, each with differing intellectual processes, differing assumptions, and differing degrees of tolerance for extrapolation from scientific studies to human circumstances. . . . Even when free of distortions imposed by the legal forum [e.g., party sponsorship or adversarial presentation], distinguished scholars from different disciplines will invoke diverse standards and practices in assessing evidence.<sup>84</sup>

The above statement both criticizes the notion that scientific methodology is a uniform or singular marker of reliability or validity, and confirms the scientific enterprise's pragmatic and socially contingent character. Cecil, however, focuses on the *Soldo* trial judge's rejection of the notion that experts need only meet "the same level of intellectual rigor that characterizes the practice" of others in the field.<sup>85</sup> In the court's words, while

it is sometimes necessary in a clinical, regulatory, or business practice to make decisions based on less than sufficient and/or reliable scientific evidence due to practical demands requiring immediate decision-making, such guesses, although perhaps reasonable hypotheses based on the best available evidence, do not constitute a scientifically reliable approach when used to assess causality via the scientific method.<sup>86</sup>

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<sup>80</sup> See Cohen, *supra* note 16, at 949 ("Science . . . routinely uses filters that prevent its experts from reaching exactly the sort of opinions . . . that should be utilized in a civil trial.").

<sup>81</sup> See Cecil, *supra* note 43, at 985.

<sup>82</sup> 244 F. Supp. 2d 434 (W.D. Pa. 2003).

<sup>83</sup> See Cecil, *supra* note 43, at 973-80.

<sup>84</sup> *Id.* at 984.

<sup>85</sup> See *id.* at 985.

<sup>86</sup> *Id.* at 984.

That judicial perspective conflicts with the views of scholars like Cohen, who advocate lowering post-trilogy reliability requirements.<sup>87</sup> Nonetheless, that perspective is shared by those trial judges who tend to idealize science and who are reversed (by appellate panels who have a pragmatic view of science) for demanding more of science than it can reasonably offer.<sup>88</sup> From the standpoint of those who idealize science, the post-trilogy reliability standards are too low. But why would Cecil, having recognized that “validity” is ephemeral and methodologies diverse, agree with such a distinction between the lofty notions of reliability/method and the mere “guessing” that scientists engage in everywhere except in the courtroom? One possible explanation is that when describing the variations between the independent experts in *Soldo*, Cecil focuses primarily on their *methodological* variations, not their extra-methodological intellectual processes, assumptions, and values. Instead of acknowledging the social, institutional, and rhetorical variables that drive science, each discipline of science, viewed in isolation, is seen as primarily methodological. Perhaps Cecil does not take seriously the notion that some of science’s best processes, assumptions, and values are not methodological. On the other hand, Cecil realizes that the trial judge’s call for “a scientifically reliable approach” using “the scientific method” is not scientific at all, but is a *legal* standard dressed up as a scientific standard. Therefore, a more cogent explanation is that Cecil does, in fact, recognize the instability and pragmatism of science. On that point, Cecil does not completely share the *Soldo* court’s idealization of science. Instead, Cecil admires the court’s unwitting idealization of law as better and more demanding than science. We will return to Cecil’s idealization of law in the next section, but its contrast with Cohen’s idealization of science is evident. In Cohen’s view, since science filters out too much of what law needs, law should apply lower reliability standards than those of science itself. In Cecil’s modest view of science, on the other hand, since science’s own standards are too low for law, law should apply higher standards. While we agree with Cecil’s modest view of science, we believe his idealization of law, as capable of doing a better job, leads him away from a proper focus on application and toward an abstract ideal of validity, albeit legal instead of scientific.

Finally, a third type of idealization of science in post-trilogy scholarship takes the form of casual references to so-called “junk science.” James Shellow, for example, in his commentary on the

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<sup>87</sup> See Cohen, *supra* note 16, at 963.

<sup>88</sup> See Caudill & LaRue, *supra* note 25, at 24-36.

limits of cross-examination of experts, begins by identifying “junk scientists” who were supposed to be exposed in “*Daubert* and *Kumho* hearings,” who “[u]nfortunately . . . appear on the stand well-dressed and articulate,” and who “believe in their junk science.”<sup>89</sup> Shellow subsequently refers to “the now legendary junk science,”<sup>90</sup> but his use of the term “legendary” is more appropriate than he might realize. Professor Mansfield has already condemned the “campaign of sloganeering, employing such labels as ‘junk science’ . . . aimed at casting scorn on those who testified to opinions thought to warrant these labels. It is embarrassing to concede that this kind of sloganeering may have influenced the course of the law.”<sup>91</sup> The point, of course, is not that all proffered expertise is adequate. There is widespread acknowledgement that some experts and some expertise are not worthy of the courtroom. Rather, the “model that posits junk science as distinguishable from . . . ‘good science’ . . . is a flexible, politically charged framework that . . . plays a strategic, rhetorical role in the agendas of many who attempt to address the pervasive perception of an ongoing legal crisis.”<sup>92</sup>

As Professors Edmond and Mercer demonstrate, the “junk science” model relies on “untenable images of efficacy, methods, norms, and motivations as hallmarks of ‘good science.’”<sup>93</sup> The term “junk science,” lacking any consistent meaning, functions more as an ideal or image for those who

- (i) oversimplify the relationship between scientific knowledge and technically efficacious outcomes;<sup>94</sup>
- (ii) view “factors like financial opportunism [as] hallmarks of junk science, [when] in reality such factors provide powerful motors for many fields of contemporary science”;<sup>95</sup>
- (iii) consider scientific norms prescribing that scientists be detached, impersonal, self-critical, and open-minded as “necessary

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<sup>89</sup> James M. Shellow, *The Limits of Cross-Examination*, 34 SETON HALL L. REV. 317, 317 (2003).

<sup>90</sup> *Id.* at 319 (quoting Allen, *supra* note 12, at 5).

<sup>91</sup> Mansfield, *supra* note 7, at 82.

<sup>92</sup> Edmond & Mercer, *supra* note 54, ¶¶ 1, 3-4.

<sup>93</sup> *Id.* ¶ 9.

<sup>94</sup> *See id.* ¶ 16 (“Any simple linkage between science and practice is . . . undermined when we consider the reworking and simplification of scientific knowledge as it moves from abstract theorizing into standardized forms sufficient to fulfill a technological function.”).

<sup>95</sup> *Id.* ¶ 23 (“Unlike the impression conveyed by junk science model proponents, in actuality scientists frequently find themselves in a competitive environment where strong emotional commitment to their views and sensitivity to finance and funding are essential to career progression—even academic and institutional survival.”).

feature[s] of doing scientific work”;<sup>96</sup> and

(iv) assume that “a simple, identifiable, universal scientific method . . . guides [scientific] activity.”<sup>97</sup>

In short, the very mention of junk science, unless it is qualified immediately with carefully examined examples, signals a romanticization of science as method and a corresponding failure to acknowledge the social context of *even the best science* as constitutive.

In recent appraisals of the use of forensic science in criminal cases, the recognition of the “social” as positive or fruitful is evident. Professor Margaret Berger, for example, describes DNA typing as “the by-product of cutting-edge science,” as opposed to “forensic specialties which originated within the law enforcement community . . . to facilitate investigations and prosecutions.”<sup>98</sup> The distinction between cutting-edge science and “courtroom” science might appear to support an argument that the latter is “social”—motivated, biased (toward prosecution), and interested—as opposed to the former as relatively objective or “natural.” In her historical narrative concerning DNA typing, however, Berger speaks of that method’s universal ratification by the scientific community, of scientists serving on committees to monitor the use of DNA typing in trials, of reports issued by the National Academy of Sciences, of shifts in laboratory technologies, of impliedly valid disputes concerning “appropriate probabilities and . . . how they should be expressed,” of the need for proper collection and analysis of genetic markers, quality control, and documentation protocols in laboratories, and of “proficiency testing of laboratory personnel.”<sup>99</sup> Two observations about this list can be made. First, each of the listed phenomena is decidedly social, institutional, or rhetorical (not simply methodological): community ratification, committees, institutional reports, evolving technologies and protocols, disputes, document writing, and credentialing. Second, these social features of DNA typing have generated good, not bad, science; without the elaborate social process that Berger documents, DNA typing would not have made the progress that it has. Note, however, that Berger does not idealize nuclear DNA testing—“the gold standard for expert proof”—because it “may, under some circumstances, produce results that are

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<sup>96</sup> *Id.* ¶¶ 25-26 (citing MICHAEL MULKAY, SCIENCE AND THE SOCIOLOGY OF KNOWLEDGE 64 (1979)).

<sup>97</sup> *Id.* ¶¶ 28-29 (“[S]tandards of proof, models, acceptable error rates, and observation . . . vary substantially from one branch of science to the next.”).

<sup>98</sup> Berger, *supra* note 37, at 1126.

<sup>99</sup> *Id.* at 1126-29.

completely wrong.”<sup>100</sup> Reliability in the abstract, therefore, is not enough. For admissibility and sufficiency determinations, the focus should be on “what the evidence proves and how the trier of fact will use it.”<sup>101</sup>

In his critique of experts who peddle “tainted or fraudulent science,” Professor Paul Giannelli offers a similar description of good science.<sup>102</sup> Giannelli summarizes the recommendations of the Inspector General’s 1997 report on the Federal Bureau of Investigation (FBI) laboratory, which criticized inaccurate, incompetent, and poorly documented testimony, as including:

- (1) seeking accreditation of the FBI laboratory by the American Society of Crime Laboratory Directors/Laboratory Accreditation Board;
- (2) requiring examiners . . . to have scientific backgrounds . . .
- (3) mandating the preparation . . . of separate reports instead of having one composite report . . .
- (4) establishing report review procedures . . .
- (5) preparing adequate case files . . .
- (6) monitoring court testimony . . . and
- (7) developing written protocols for scientific procedures.<sup>103</sup>

Again, the keys to legitimate scientific inquiry are the social, not just methodological, aspects of science (institutional accreditation, credentialing, review of documentation, and procedural conventions). Institutions, community oversight, persuasive documentation, and the social “capital” represented by credentials are the cure for mediocre science and the route to scientific progress. In light of these accounts, the counterargument, that the social, institutional, and rhetorical aspects of science are secondary to science’s *real* or methodological work (hypothesis, data, and testing), rings hollow.

Finally, we should mention Professor Christopher Slobogin’s recent commentary on expertise in criminal cases, which concedes the difficulties faced by defendants using social scientists in the exclusionary post-trilogy regime (for example, that error rates are hard to generate because of multiple variables, and the practical and ethical limitations on experimentation).<sup>104</sup> Slobogin notes that

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<sup>100</sup> *Id.* at 1140.

<sup>101</sup> *Id.*

<sup>102</sup> Giannelli, *supra* note 17, at 1107 (quoting BARRY SCHECK ET AL., ACTUAL INNOCENCE: FIVE DAYS TO EXECUTION AND OTHER DISPATCHES FROM THE WRONGLY CONVICTED 246 (2000)) (internal quotation marks omitted).

<sup>103</sup> *Id.* at 1108 (citing OFFICE OF INSPECTOR GENERAL, U.S. DEP’T. OF JUSTICE, THE FBI LABORATORY: AN INVESTIGATION INTO LABORATORY PRACTICES AND ALLEGED MISCONDUCT IN EXPLOSIVES-RELATED AND OTHER CASES (1997)).

<sup>104</sup> See Slobogin, *supra* note 19, at 108-16.

“[r]esearch requires money[,]” and “[t]he state has more of it”:<sup>105</sup>

The state not only has more money, but it is better equipped, in an institutional sense, to use it. . . . [T]he state is better able to anticipate the scientific issues that will arise and act accordingly. Indeed, *Daubert* and *Kumho Tire* have already stimulated massive federal efforts to validate the type of forensic evidence typically relied upon by the prosecution.<sup>106</sup>

The resources of defense-oriented academic researchers “pale when compared to the government’s.”<sup>107</sup> In this context, Slobogin is not *criticizing* prosecution-oriented science for its interest, bias, or motivation; rather, Slobogin contends that methodology *needs* institutional support. Money and other resources are, almost always, conditions for the production of scientific expertise.<sup>108</sup>

Given that most idealizations of science identified in this section are quite subtle, they resemble unfortunate tendencies more than dangerous narratives concerning courtroom expertise. To illustrate, Professors Gross and Mnookin readily acknowledge that “the level of confidence the expert witness expresses” is as important as methodology,<sup>109</sup> which helps refocus attention on the application phase rather than threshold reliability. Nonetheless, as they begin to discuss non-scientific evidence, Gross and Mnookin observe:

At least compared to alternative forms of knowledge-production, research science involves formalized methodological norms, articulated standards, and conscious research design. By contrast, many forms of potential expert knowledge—from the clinical doctor’s diagnosis to the historian’s description to the tire safety expert’s analysis—are based on experience, tacit knowledge, even hunch.<sup>110</sup>

While recognizing that qualitative differences surely exist between highly replicable research designs and mere theories based on interesting but minimal data, Gross and Mnookin hint that the best science rises above subjectivity. The contrast is not so sharp, however, because “experience, tacit knowledge, [and] even hunch” alternatively generate our “formalized methodological norms,

<sup>105</sup> *Id.* at 116-17.

<sup>106</sup> *Id.* at 117.

<sup>107</sup> *Id.*

<sup>108</sup> Moreover, while Slobogin comes close to idealizing the “positivistic” hard sciences by repetitive contrast to the “socially-constructed” soft sciences, he is actually a critic of strict reliability standards. Slobogin maintains that the standards lead to unfairness (toward criminal defendants) and to less reliable outcomes because less-than-ideal science is better than no defense-oriented science at all. *See id.* at 118.

<sup>109</sup> Gross & Mnookin, *supra* note 35, at 188.

<sup>110</sup> *Id.* at 142-43.

articulated standards, and conscious research design.”<sup>111</sup> Consensus concerning methodological norms and standards *is* tacit knowledge. Research designs draw on experience and sometimes rely on a hunch as to what might work better. The appropriate distinction, therefore, is not between methodology and experience, but rather between experience that leads to promising research and experience that does not. The unwitting attempt to sanitize science of its fruitful and supportive social context not only leads to a romanticized vision of the scientific enterprise, but unjustifiably minimizes the application phase’s significance. The application phase is a social context wherein the practical goals and limitations of methodology and “reliability” are visible.

A focus on the application phase of expertise is both appropriate and useful, so long as one maintains a modest view of both science and law. In light of this thesis, we should also note that a modest view of science by itself—one without a focus on application and/or a modest view of law—is less than helpful. This point can best be demonstrated by comparing three Seton Hall Symposium articles whose authors share a modest view of science. Other than this shared viewpoint, however, these authors disagree and become side-tracked into positions that do not help solve the problems of admissibility of expertise in the courtroom.

To illustrate, Professor Mansfield’s position epitomizes the modest view of science. He was an early critic of *Daubert’s* emphasis on Popper’s falsifiability criterion,<sup>112</sup> and remains convinced that adopting “science” as a legal category was “a fundamental error”:

*Daubert* [held] that to be “science,” evidence had to be “scientifically valid” or “good science,” and that this sort of science could only be the result of the “scientific method.” The truth is that . . . there is no clear understanding and agreement about what is meant by “science,” “good science” or “the scientific method.”<sup>113</sup>

In response to recent concerns about “scientific validity” and reliability, a certain probative value is now required prior to admissibility. For Mansfield, this requirement is “a grave impairment of jury trials,”<sup>114</sup> and unnecessary because the rules of evidence already excluded evidence that might mislead the jury.<sup>115</sup> Prior to

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<sup>111</sup> *Id.* at 143.

<sup>112</sup> See generally John H. Mansfield, *Scientific Evidence Under Daubert*, 28 ST. MARY’S L.J. 1 (1996).

<sup>113</sup> Mansfield, *supra* note 7, at 81.

<sup>114</sup> *Id.* at 84-85.

<sup>115</sup> See *id.* at 84; see also *id.* at 79 (“Exclusion on the grounds of prejudice,

*Daubert*, admissibility, based solely on the expert's qualification and the relevance of his testimony, was easier.<sup>116</sup> Even *Frye's* "general acceptance" criteria "appeared to be restricted to 'novel' scientific evidence."<sup>117</sup> But now, under the force of (i) powerful economic interests whose servants initiated the campaign against "junk science," (ii) judges who want to be associated with prestigious scientific knowledge, (iii) scientists who disapprove of inferior courtroom science, and (iv) an ideological distrust of juries,<sup>118</sup> we are left with a "conceptual muddle"<sup>119</sup> and with "[c]onfusion and conflict."<sup>120</sup>

We admire Professor Mansfield's modest assessment of science and the scientific method in his critique of *Daubert*,<sup>121</sup> and will address what we perceive to be his idealization of law and especially the jury, in Part IV, below. Notably, Professor Christopher Mueller, instead of criticizing *Daubert*, actually finds *in that opinion* a modest view of the scientific enterprise:

[Alongside] an apparent belief that science is a static body of objective knowledge reflecting certainty . . . we also find in *Daubert* suggestions that (a) science is a process, hence anything but static; (b) scientific knowledge does not reflect certainty, but is uncertain and contingent; and (c) scientific expertise is affected by the forces that generate litigation, hence [it is] *subjective* in some respects, and socially constructed.<sup>122</sup>

In contrast to Mansfield, however, Mueller argues that we need a *Daubert*-type validity standard, as "[w]e can make the judgment that not all evidence that is presented as science, even by qualified witnesses, is of such quality that it can be relied upon . . . . We can believe that such evidence varies in quality, and that sometimes it is *not* reliable enough."<sup>123</sup> Mueller, predictably, observes that juries "have trouble with complex cases, and with scientific evidence,"<sup>124</sup> and that more educated and experienced judges "can do better than juries in separating what should count from what should not."<sup>125</sup> Even

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confusion of issues, waste of time and so forth [are] catalogued in Rule 403.").

<sup>116</sup> *See id.* at 77-78.

<sup>117</sup> *Id.* at 79.

<sup>118</sup> *Id.* at 82-83.

<sup>119</sup> Mansfield, *supra* note 7, at 77.

<sup>120</sup> *Id.* at 87.

<sup>121</sup> *See id.* at 83 ("[T]here does not exist outside the law any settled meaning for [these] terms.").

<sup>122</sup> Mueller, *supra* note 6, at 1007 (emphasis added).

<sup>123</sup> *Id.* at 991; *see also id.* at 1001 ("[P]roof should be excluded when it is thin, and looking directly at the science seems a good thing, not a bad thing.").

<sup>124</sup> *Id.* at 992.

<sup>125</sup> *Id.* at 993 ("[T]he very fact that a court *admits* evidence that is daunting or

though Mueller and Mansfield agree that “reliability is not an all-or-nothing concept, but a relative concept,”<sup>126</sup> that assessment standing alone does not lead to agreement concerning the vices and virtues of the *Daubert* trilogy.

Indeed, a third possibility might follow from a modest assessment of the scientific enterprise, namely the (technically correct, but easily exaggerated) view that mainstream science sometimes produces “junk” and, impliedly, novel science is often superior:

The real issue is determining what junk science is, especially during an era of constantly evolving scientific developments. Remember, it was not too long ago that . . . [a]rguments were made that research linking cigarettes to lung cancer were “junk.” Similarly, . . . [i]t was not until the late seventies that the world learned that [asbestos] was lethal in dust form. Again, early studies . . . would have been labeled “junk” science by today’s standards.<sup>127</sup>

Here, the views of Baumeister and Capone are distinct from those of both Mueller, whose validity standard makes no distinction between mainstream and novel science, and Mansfield, who sees the utility of a validity standard *only* with respect to novel science.<sup>128</sup> For Baumeister and Capone, reliability standards are dangerous; lives are lost to diseases like cancer and asbestosis when “incriminating expert evidence [is] subjected to a preliminary review by a trial court today[.]”<sup>129</sup>

Apart from demonstrating why a modest assessment of science does not entail any particular view of the *Daubert* trilogy, the foregoing examples also variously illustrate both why we should focus on application, not on science in the abstract, and why a modest view of science must be accompanied by a modest view of law. We have no criticism of Mansfield’s view of science. However, his idealization of the jury, discussed in Part IV, below, interferes with his ability to offer guidance to judges and lawyers. In effect, Mansfield posits that the

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complex conveys to jurors and unspoken message of invitation, suggesting that they *can* rely on it.”).

<sup>126</sup> *Id.* at 1010; *cf.* Mansfield, *supra* note 7, at 81 (“[A]mong . . . scientists, there is no clear understanding or agreement about what is meant by . . . ‘good science’. . . . Furthermore scientists who might be willing to give an account of how they go about their work, would probably disclaim responsibility for attaching any great significance to their account beyond its justifying the decisions they make regarding further research.”).

<sup>127</sup> Baumeister & Capone, *supra* note 24, at 1026.

<sup>128</sup> See *supra* notes 117-23 and accompanying text.

<sup>129</sup> Baumeister & Capone, *supra* note 24, at 1044.

*Daubert* trilogy is hopeless: misguided, unpredictable, and unconstitutional.<sup>130</sup>

Mueller, on the other hand, is wildly hopeful. We have no criticism of Mueller's *initial and overt* modest assessment of science—that scientific evidence offers possibilities and probabilities, and “much scientific knowledge is fluid and contestable.”<sup>131</sup> Two aspects of Mueller's analysis, nevertheless, concern us. At the outset, Mueller appears to have no proposals for reform. He defends the *Daubert* trilogy's restrictive regime on the bases that judges are more competent than juries, that gatekeeping is necessary for reliability, and that the Court's definition of science is balanced.<sup>132</sup> Nevertheless, as discussed in Part IV, below, Mueller rejects the abuse-of-discretion standard on the basis of an idealized notion of appellate review. More to the point, with respect to his initial modest view of science, Mueller's analysis proceeds toward a subtle idealization of science. This takes the form of downplaying the social, institutional, and rhetorical aspects of science in favor of its methodological rigor. For example, at the end of his defense of *Daubert*, Mueller confirms his seemingly modest view of the scientific enterprise:

Behind the numbers [the product of analysis of the quantification of scientific data] are more and real uncertainties—the ones that go with designing tests, selecting cohorts, trying to eliminate differences *apart from the factor in issue* that might account for observed differences. . . . [I]t is hard or impossible to eliminate confounding variables, and . . . even promising results might not be replicable. . . . And there [are] self-serving human motivations . . . .<sup>133</sup>

These aspects of science, however, are not presented by Mueller as characteristics of genuine science; rather, they are reasons to be cautious, and reasons why “science insists on impressive numbers.”<sup>134</sup> Even more telling, Mueller's footnote to the above quotation does not cite to historians, philosophers, or sociologists of science who elucidate the inevitable social, rhetorical, and institutional aspects of “good” science. Instead, Mueller refers to a *New York Times* article

<sup>130</sup> See Mansfield, *supra* note 7, at 77 (explaining that our “state of affairs . . . may fairly be described as a conceptual muddle containing within it a threat to liberty and popular participation in government”), 87 (“It is difficult to predict the future. It is impossible to imagine that the Court will dismantle the *Daubert-Kumho* regime . . . . There seems little possibility of legislative intervention or of any remedial proposal . . . . Confusion and conflict may increase . . .”).

<sup>131</sup> Mueller, *supra* note 6, at 990-91.

<sup>132</sup> See *id.* at 989-1018.

<sup>133</sup> *Id.* at 1018.

<sup>134</sup> *Id.*

decrying epidemiology as “crude and inexact,” and to a *Chronicle of Higher Education* article about the seven signs of “bogus science”—pitching claims to the media (rather than to peer-reviewed journals), identifying a powerful establishment that suppresses novel research, claiming hard-to-detect effects, and so forth.<sup>135</sup> A distinction, therefore, between “genuine” and “junk” science—and not merely between “better” and “worse” science—creeps back into Mueller’s analysis. In defending *Daubert* against critics who argue on philosophical grounds that the case is analytically defective and incoherent, Mueller praises the mediation or compromise between the goals of objectivity and the more modest views of Popper and Kuhn<sup>136</sup>—neither of whom are representative of contemporary science and technology studies, the sociology of scientific knowledge, or of rhetoricians and social historians of science. All of this suggests that Mueller does not take seriously the view that all of science, even as it succeeds in modeling nature and making accurate predictions, is social, historical, and rhetorical. Although this idealization of science is subtle, it is just enough to prevent Mueller from seeing the problem with focusing on demarcation and admissibility rather than application.

#### IV. MODEST VERSUS IDEALIZED VIEWS OF LAW

In the Seton Hall Symposium, some solutions to the perceived challenges (for lawyers and judges) generated by the trilogy often took the form of idealizing a particular element of the trial—perhaps the process of appellate review, the trial judge, the jury, or even the capacity of experts to communicate appropriate legal standards to the jury. Like idealizations of science, idealizations of law tend to deflect attention away from the application phase. In the “case at hand,” notions of reliability and validity intersect with confidence levels and the pragmatic goals and limitations of scientific inquiry. Moreover, just as idealizations of science are bolstered by downplaying or demonizing (as unscientific) the social aspects of science, idealizations of particular features of law often rely on demonizations of other aspects: appellate panels correct the unruly discretion of trial judges; trial judges correct the deficiencies of the jury; juries correct adversarial excess or exaggerated expertise, and so forth. Failure to recognize the realistic limits of law, as well as science, will result in impractical reform proposals.

In his defense of the trilogy, Professor Mueller provides a

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<sup>135</sup> *Id.* at 1018 n.80.

<sup>136</sup> *See id.* at 1007-10.

valuable “reality check.”<sup>137</sup> In an acute dissection of professorial fallacies in post-trilogy scholarship, Mueller illustrates how some of the “bad” rules supposedly established in the trilogy simply do not exist.<sup>138</sup> But then, in a less realistic moment, Mueller suggests that appellate review of admissibility decisions (concerning expertise) should be *de novo*.<sup>139</sup> Mueller is convinced that the abuse-of-discretion standard, confirmed in *Joiner* and *Kumho Tire*, will lead to an unfortunate lack of uniformity:

[I]ssues relating to the validity of theories and techniques transcend the facts of individual cases. This observation applies . . . to the question whether DNA profiling can reliably identify a blood or fluid sample as having very likely come from one person . . . . It applies to the question whether proffered statistical proof should satisfy the standard that scientists would require, to the question whether differential diagnosis[,] . . . animal studies . . . [or] . . . similarities between . . . chemical structures . . . can prove causation. Questions of this magnitude need steadier guidance than the abuse-of-discretion standard provides, and the answers that courts reach should be applied in similar cases. . . .<sup>140</sup>

In our view, the phrase “abuse of discretion” is indeed notorious for its elasticity of meaning; appellate courts regularly reverse trial judges and administrative agencies under this heading. Lack of uniformity, however, in deciding cases is not an evil unless the cases are indistinguishable. If the appropriate question in cases involving scientific expertise is primarily how science is “brought to bear” in a particular case, and not scientific reliability in general, then the reliability ruling in any particular case is likely to be easily distinguishable from other rulings.

At the Seton Hall Symposium, the Honorable Chief Judge Gibbons of the United States Court of Appeals for the Third Circuit, now retired, engaged in a more careful and nuanced discussion of “the respective roles of trial courts and appellate courts with respect to the admissibility of . . . expert . . . evidence . . . .”<sup>141</sup> Judge Gibbons begins his discussion by noting three *complications* concerning appellate review of evidentiary rulings—namely, the harmless-error

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<sup>137</sup> See generally Mueller, *supra* note 6.

<sup>138</sup> See, e.g., *id.* at 996-1001 (explaining that supposed rules that (i) require doubling of incremented risk for epidemiological evidence and (ii) disallow differential diagnosis evidence to show causation, are not settled).

<sup>139</sup> See *id.* at 1019-22.

<sup>140</sup> *Id.* at 1020-21.

<sup>141</sup> Gibbons, *supra* note 10, at 128.

rule, the trial judge's fact-finding on preliminary questions, and the abuse-of-discretion standard.<sup>142</sup> Judge Gibbons then contrasts these complications with the rather straightforward legal principle that appellate review of a summary judgment or a directed verdict is plenary.<sup>143</sup> The contrast between review of evidentiary rulings and the plenary review of summary judgments is generally defensible. As Judge Gibbons points out, however, a trial judge's decision to exclude an expert's testimony may remove all of the evidence on a critical element of a claim.<sup>144</sup> In such a case, there is little or no practical difference between the evidentiary ruling and a summary judgment ruling.

One response to this difficulty would be to argue, like Professor Mueller does, that the distinction should be abolished. Judge Gibbons is tempted to simply do so.<sup>145</sup> But instead, he reviews the relevant precedent in his own circuit, especially the opinions of Judge Becker, and concludes that the scope of review for evidentiary rulings "is not a simple matter."<sup>146</sup> While criticizing *Joiner's* hard line as a "pure, simple, unvarnished abuse of discretion" standard,<sup>147</sup> Judge Gibbons identifies ambiguities in that opinion due to "the absence of a nuanced definition of abuse of discretion, a definition including legal error, procedural irregularity, disregard of evidence that should have been considered, and clearly erroneous factual determinations. Any one of these ought to lead to a statement that there was an abuse of discretion."<sup>148</sup> Moreover, explaining that Rules 702 and 703 of the Federal Rules of Evidence<sup>149</sup> "require the determination of what, in most or many cases, will be a mixed question of law and fact," Judge Gibbons concludes that review of such determinations should be plenary.<sup>150</sup> If the abuse-of-discretion "standard" is already as elastic as Judge Gibbons claims, however, his conclusion need not follow.

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<sup>142</sup> *Id.*

<sup>143</sup> *Id.* at 129.

<sup>144</sup> *Id.*

<sup>145</sup> *See id.* at 134.

<sup>146</sup> *Id.* at 133.

<sup>147</sup> Gibbons, *supra* note 10, at 136.

<sup>148</sup> *Id.* at 139.

<sup>149</sup> *See* FED. R. EVID. 702 (requiring that testimony that will assist trier of fact to understand evidence or to determine fact in issue is admissible if qualified expert bases testimony on sufficient data, uses reliable methods, and applies methods reliably); FED. R. EVID. 703 (stating that facts or data relied upon for expert opinion need not be admissible, but if their probative value outweighs their prejudicial effect, otherwise inadmissible facts or data may be disclosed).

<sup>150</sup> Gibbons, *supra* note 10, at 139.

Professor Mueller argues for plenary review on the bases that (i) “issues relating to the validity of theories and techniques transcend the facts of individual cases” and require uniformity;<sup>151</sup> (ii) three (or more) minds are better than one;<sup>152</sup> and (iii) trial judges need guidance.<sup>153</sup> Nevertheless, Mueller concedes that with respect to the application phase, “some degree of deference” to trial judges is warranted:

FRE 702 indicates that judges are to consider “principles and methods” and the sufficiency of underlying “facts or data,” *and also* the question whether the expert “has applied the principles and methods reliably to the facts,” and *Kumho Tire* makes it clear that the focus is the “task at hand” . . . . To the extent that the admissibility decision actually focuses . . . on . . . laboratory protocol . . . or . . . discrepancy in the data . . . some degree of deference to the decision of the trial judge is in order. It is with larger questions, including those of theory and technique . . . that closer scrutiny is warranted.<sup>154</sup>

On the other hand, if the action is in the application, then most cases require discretion. Mueller’s plenary review is justified only by his twin idealization of the appellate judiciary as more relaxed and reflective<sup>155</sup> *and* of science as a source of transcendent knowledge of valid theories and techniques. Judge Gibbons’ idealization of appellate review is more measured. But unlike Professor Mueller, Judge Gibbons justifies plenary review *especially* in cases mixing law and fact—in his words, “in many if not most cases.”<sup>156</sup> Both Mueller and Judge Gibbons are correct *as to some cases*. Still, as the all-purpose solution, invoking plenary review is an oversimplification based on an idealization of the appellate judiciary. Although “abuse of discretion” is a flexible concept worthy of Professor Mueller’s and Judge Gibbons’ critical attention, plenary review is also a flexible practice.

In addition to idealizations of appellate review (to correct deficient judges), some scholars at the Seton Hall Symposium idealized the jury. Professor Mansfield, for example, argued that the rules of evidence have been, are, and should be oriented to the

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<sup>151</sup> Mueller, *supra* note 6, at 1020.

<sup>152</sup> *See id.* at 1021.

<sup>153</sup> *Id.*

<sup>154</sup> *Id.* at 1022.

<sup>155</sup> *See id.* at 1021 (“[A]ppellate review goes forward in a setting less subject to severe scheduling pressures. . . . Reviewing courts can even take judicial notice of technical books, articles, and other materials.”).

<sup>156</sup> Gibbons, *supra* note 10, at 139.

admission of all relevant evidence.<sup>157</sup> Thus, the trilogy's reliability requirement for admissibility ends up as an embarrassing error.<sup>158</sup> Moreover, given that the reliability requirement leads judges to invade the province of the jury, for Mansfield, this error has constitutional dimensions.<sup>159</sup>

Mansfield's idealization of the jury is clearest in his distinction between Rule 403 and the trilogy's reliability requirement.<sup>160</sup> Mansfield sees Rule 403 as directed against the evils that

the jury will ignore the substantive law, the jury will disregard the burden of proof, the jury will be swept away by emotion, and so forth. These evils can be seen as such and taken into account through the exclusion of evidence without attacking the very reason for having [a] jury trial: that the verdict may reflect beliefs about the world held by ordinary people and the working of average intelligences.<sup>161</sup>

The distinction, however, between the listed "evils" and the *reason* for jury trials is difficult to sustain. Suppose an attorney attempts to inflame the jury's passion so that jurors will "ignore the substantive law" and "disregard the burden of proof." Why might the attorney succeed? Would it not be because jurors had "beliefs about the world" that would lead them to be susceptible to a passionate appeal? "Inflammatory" remarks only work by appealing to stereotypes held by ordinary people. Thus, insisting that we preserve ordinary beliefs about the world seems naive.

To bolster his idealization of the jury, Mansfield also demonizes those who hope the trilogy's reliability requirements will improve the accuracy of adjudication. As Mansfield sees it, the trilogy is the product of

powerful economic interests[,] . . . the desire of some judges to be associated with science, . . . the anger and scorn [of] elite scientists [toward law's use of 'bad science' and] an ideology, far from decisively eliminated in our political debates, which cannot see the sense in entrusting to twelve persons picked at random from the general population important and difficult questions of fact.<sup>162</sup>

Singled out as an ideologue, Professor Allen is allegedly guilty of the fourth count in Mansfield's indictment. Allen's emphasis on

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<sup>157</sup> See Mansfield, *supra* note 7, at 77-84.

<sup>158</sup> See *id.* at 84.

<sup>159</sup> See *id.* at 83-84.

<sup>160</sup> See *id.* at 79.

<sup>161</sup> *Id.* at 84.

<sup>162</sup> *Id.* at 82-83.

accuracy in trials rests on the “conviction that jury verdicts are not as accurate as other forms of adjudication and that there are no good policy reasons why an inferior form of fact-finding should be accepted.”<sup>163</sup>

Mansfield’s modest view of science is one of the best.<sup>164</sup> His exaggeration of the jury’s role, however, interferes with his ability to recognize the virtues of *Kumho Tire*’s focus on the jury trial’s application stage. Though Mansfield suggests that *Kumho Tire* is incompatible with respect for the jury, one can admire both *Kumho Tire* and the jury system: Judge plus jury is superior to judge or jury alone. The rules of evidence, enforced by a judge, can lead to presenting better evidence to a jury than otherwise would be presented. Better evidence is more likely to lead to better decisions. The difficulty lies in formulating the best working relationship between counsel, judge, and jury.

Insofar as Baumeister and Capone’s idealization of the jury relies on the argument that judges and juries are equally inept when it comes to science, it perhaps should not be referred to as an “idealization.” In their words, “[w]hile there is little research data in the area, studies suggest that the ability of judges and jurors to make correct inferences from probability data are both poor, and specifically, that the judges are *not* superior to jurors.”<sup>165</sup> Instead of acknowledging, as Sanders does,<sup>166</sup> the conflicting evidence regarding juror confusion with respect to scientific testimony, Baumeister and Capone select their sources to claim, in contradiction to the above assertion, that “[t]here is simply ‘no evidence that juries are incompetent to evaluate expert testimony’ or that if permitted to review all expert evidence . . . that there is a greater potential for unsupported, exorbitant damage verdicts.”<sup>167</sup>

To be fair, Baumeister and Capone offer several concrete suggestions to address the problem of juror confusion, including better lawyer communication and better jury instructions, providing a written synopsis to the jury, back-to-back expert testimony (to allow comparisons between conflicting experts), and allowing juror questions. These suggestions, nevertheless, are lost in a polemic against the “chilling” and “erosive” effect of judicial gatekeeping on

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<sup>163</sup> Mansfield, *supra* note 7, at 86 (citing Allen, *supra* note 12, at 7).

<sup>164</sup> See *supra* notes 112-13, 121 and accompanying text.

<sup>165</sup> Baumeister & Capone, *supra* note 24, at 1040.

<sup>166</sup> See generally Sanders, *supra* note 21.

<sup>167</sup> Baumeister & Capone, *supra* note 24, at 1041 (quoting Neil Vidmar & Shari Seidman Diamond, *The Jury in the Twenty-First Century: An Interdisciplinary Conference Article: Juries and Expert Evidence*, 66 BROOK. L. REV. 1121, 1175 (2001)).

the adversary system.<sup>168</sup> “[*Daubert* has] effectively weakened the parties’ control over litigation . . . and eliminated the trial judge’s ‘neutrality’ by empowering them [sic] to exclude critical evidence from the jury’s consideration . . . .”<sup>169</sup> Baumeister and Capone suspect that the current restrictive regime, which is not in their view justified by the texts of the trilogy, has more to do with judicial desire to reduce case dockets and “an attempt by large corporations to deflect some of society’s more difficult issues.”<sup>170</sup>

In a similar but more sophisticated argument that plaintiffs in civil suits are treated unfairly, Professor Cohen suggests that the trilogy’s reliability standard is too high.<sup>171</sup> Since a scientist’s “burden of proof” for causation within the scientific community is higher than “preponderance of the evidence,” Cohen contends, it is inappropriate in a civil trial.<sup>172</sup> Though he proceeds by focusing on epidemiology, Cohen makes clear that his argument applies to any science that uses similar statistical reasoning.<sup>173</sup> Unfortunately, Cohen’s argument oversimplifies the complexities of legal judgments; as a result, he ends up idealizing the burden of proof and proposing an impractical framework for civil trials.

To be concrete, Cohen’s argument can be restated by supposing that one wants to know whether eating broccoli causes cancer. (President Bush the Elder had different reasons for not wanting to eat it, but suppose he had asked the National Institutes of Health to investigate the question.) A scientist would first assume that broccoli does not cause cancer. This assumption is the so-called “null hypothesis.”<sup>174</sup> At this point, there is no deviation from the legal system, which also starts with the assumption that a civil defendant will not owe the plaintiff any money until the plaintiff comes forward with proof. Next, the scientist would investigate the incidence of cancer among those who eat broccoli and those who do not. The empirical data may well show that cancer is higher among broccoli eaters than among those who shun it, but this difference could be the result of chance—the random nature of many events. To rule out

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<sup>168</sup> See Baumeister & Capone, *supra* note 24, at 1042.

<sup>169</sup> *Id.* at 1043.

<sup>170</sup> *Id.* at 1044.

<sup>171</sup> See generally Cohen, *supra* note 16.

<sup>172</sup> See *id.* at 949 (“Science, particularly empirical science that relies on statistical or other probabilistic methods, routinely uses filters that prevent its experts from reaching exactly the sort of opinions as to the truth of ultimate facts that should be utilized in a civil trial governed by the preponderance of the evidence rule.”).

<sup>173</sup> See *id.* at 951-55.

<sup>174</sup> See *id.* at 952.

chance, epidemiologists set a high standard; typically, the odds must be better than twenty to one in favor of the broccoli–cancer link before they will assume that the null hypothesis has been disproved.<sup>175</sup>

What should courts make of this twenty-to-one standard? Neither Cohen nor we would say that epidemiologists are not doing their job well—they know their business better than we do. Convinced that the legal system cannot integrate scientific procedures into the courtroom, Cohen would have scientists alter their normal criteria when they testify in court.<sup>176</sup> Cohen has based his proposal on an idealization of the clarity with which lawyers, judges, and jurors understand the burden of proof. “More probable than not” is given the standard law school translation of “greater than fifty percent,” and that fifty percent is assumed to mean the same thing that fifty percent means in statistics. Not only does this move radically oversimplify the process of analysis that ordinary people use when they decide something has been proven,<sup>177</sup> but it leads Cohen to suggest that the scientific expert should testify as a non-scientist. To illustrate, Cohen’s model for expert testimony includes the following:

I would not proclaim in an academic paper the existence of a link between the medication and high blood pressure because the [probability that data will suggest a link even when there is none] is greater than 5% [;] . . . rather, I would write that the link is suggested by the data but does [not] meet stringent scientific standards designed to minimize . . . proclamation of inaccurate findings . . . . But you have not asked me to present an academic paper . . . . In this setting, I would set the threshold somewhat lower, which I believe more accurately reflects the balance of considerations in this setting.<sup>178</sup>

The entire script of the model testimony—over 500 words—is difficult to follow. In fact, the script could even be utilized to settle the empirical debate over juror confusion in favor of jury critics; that is, Cohen seems to join those who idealize the jury. More importantly, it makes little sense to use scientists as courtroom

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<sup>175</sup> See *id.* (“[E]pidemiologist[s] . . . will typically not place weight on observed results that are not significant at the 5% . . . level.”).

<sup>176</sup> See *id.* at 961-62.

<sup>177</sup> The research concerning jurors’ understanding of burdens of proof is inconclusive. It suggests, however, that the preponderance of evidence burden of proof is not understood the way that law professors often suggest; that is, a fifty-one percent likelihood. See generally Joel D. Lieberman & Bruce D. Sales, *What Social Science Teaches Us About the Jury Instruction Process*, 3 PSYCHOL., PUB. POL’Y, & L. 589 (Dec. 1997); Rita James Simon & Linda Mahin, *Quantifying Burdens of Proof: A View from the Bench, the Jury, and the Classroom*, 5 LAW & SOC’Y REV. 319 (1971).

<sup>178</sup> Cohen, *supra* note 16, at 962.

“expert” legal theorists who proclaim reliability standards that are not based on scientific methodology.

Joe Cecil is more generous in his response to Cohen, crediting Cohen with “demonstrat[ing] that the conservative values implicit in declaring the existence of an [epidemiological] effect place an awesome barrier in the path of the plaintiff who wishes to present expert evidence to the jury.”<sup>179</sup> On the other hand, Cecil believes the problem of reliability standards is more complicated than Cohen realizes, since sciences other than epidemiology “endorse other value systems . . . requir[ing] judges to resolve issues that the sciences themselves have left unresolved.”<sup>180</sup> Indeed, referencing Cohen’s alertness to the danger of scientific values overriding legal values, Cecil begins his own analysis of scientific expertise by stating:

If only it were so easy. The problem is that there is not just one science and not one scientific method. . . . [T]he values of science vary across the individual disciplines. Each one has its own norms and standards that vary greatly in the rigor they impose in declaring a finding to be “scientific knowledge.”<sup>181</sup>

Then, just as the reader is convinced that Cecil will not oversimplify the problem of reliability, he changes the question. Cecil recommends that judges focus on sufficiency rather than reliability:

The courts cannot resolve the diverse views and values that characterize the scientific academy; one science’s accepted methodology may be another’s *ipse dixit*. A court can, however, specify a minimum threshold [of sufficiency] for admissible scientific evidence, and make clear that in doing so it is establishing a legal standard and not assessing the ephemeral concept of “scientific validity.”<sup>182</sup>

Given his use of the word “sufficiency,” Cecil seems to suggest that the best solution to the problems generated by the trilogy may be to stop worrying about reliability and focus instead on whether evidence is sufficient to support a verdict.<sup>183</sup> Or, perhaps Cecil is suggesting a special use of the term “sufficiency” to denote a legal

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<sup>179</sup> Cecil, *supra* note 43, at 968.

<sup>180</sup> *Id.* at 969.

<sup>181</sup> *Id.* at 968 (responding to Cohen, and also to Sanders, *supra* note 21, who attempt to “anticipate the shortcomings of jurors in assessing complex evidence . . . in a way that strengthens the accuracy of the process”).

<sup>182</sup> *Id.* at 985-86.

<sup>183</sup> See Nance, *supra* note 43, at 214 (“Faced with the very difficult task of coming up with a coherent scheme of . . . reliability . . . there is some tendency for trial judges to try instead to answer a different, more readily answerable . . . question. . . . Courts might, for example, treat the admissibility decision as a sufficiency decision on the merits . . .”).

standard for admissibility of expertise that is different from scientific reliability. In either event, we fail to understand why anyone would believe that the problem of sufficiency is more tractable than “reliability.” The assumption seems to be that judges can unproblematically handle such *legal* questions—an apparent idealization of trial judges. Cecil, however, has simply re-labeled the very same problem: Will not a judge deciding sufficiency have to decide whether the scientific evidence is reliable? Indeed, if we all agreed with Cecil that courts should specify minimum admissibility thresholds,<sup>184</sup> every debate in the field of post-trilogy discourse would continue to flourish.

Cecil’s analysis provides another answer to the question of how to ensure that only reliable evidence is admitted: to place more responsibility on the trial judge. Professor Faigman’s similar view is, perhaps, remarkable for its idealization of the trial judge’s role. With respect to those scholars who tend to idealize the jury and seek “to retain a prominent role for jurors in the evaluation of scientific evidence,”<sup>185</sup> Faigman is dismissive:

The issue of judge versus jury . . . is largely irrelevant. It is the judge’s task to evaluate the validity of proffered expert testimony, and that is all there is to it. . . . The only question is what is the nature of the judge’s job in this regard—the rest will be done by the jury.<sup>186</sup>

According to Faigman, moreover, trial judges should not simply attend to what happens to expertise in court, but also “to what occurred before the expertise reached the courtroom and . . . what might happen to the expertise subsequently.”<sup>187</sup> While we agree that an “admissibility decision necessarily requires a policy judgment,”<sup>188</sup> insofar as the scientific community does not provide answers to legal questions, Faigman believes “that the evidentiary determination regarding expert evidence must . . . integrate into its foundational premises [ ] the culture of the scientific method.”<sup>189</sup> Since we reject any notion of “legal” science disengaged from actual scientific practices, even that sounds agreeable. But for Faigman’s trial judge, it is a two-way street. Judges should not only “ask whether better evidence is available, [but] whether better evidence should be

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<sup>184</sup> Cecil, *supra* note 43, at 985.

<sup>185</sup> Faigman, *supra* note 43, at 264.

<sup>186</sup> *Id.* at 265.

<sup>187</sup> *Id.* at 267.

<sup>188</sup> *Id.* at 256.

<sup>189</sup> *Id.* at 258.

available.”<sup>190</sup> If handwriting experts lack data, “[t]he issue is whether the courts should expect the scientific community (broadly defined) to have produced better data on handwriting.”<sup>191</sup> Faigman’s trial judges, therefore, have the responsibility of “consider[ing] the ramifications of their admissibility decisions both in regard to the development of the respective expertise and in terms of the costs of errors . . . for society at-large.”<sup>192</sup> Anticipating criticism of this tall order, Faigman concedes that “many will complain about the difficulty of the task. It is true that [this] complicates the judge’s job. But so be it. Science is complicated.”<sup>193</sup>

Faigman’s analysis is directed against Professor Nance, who is particularly attentive to the limited “institutional capacities” of the judge and jury.<sup>194</sup> Nance posits that problems generated by the *Daubert* trilogy are best approached by “disavow[ing] a binary, all-or-nothing concept of reliability”—or sufficiency, testability, or validity—“in favor of a gradational concept.”<sup>195</sup> We agree that the “*fundamental problem*” for the doctrines concerning scientific expertise “*is how to map from a gradational epistemic conception of reliability to a dichotomous legal choice on admissibility.*”<sup>196</sup> The key to Nance’s aphorism and to understanding the complexity of expertise in the courtroom is the distinction between legal concepts and legal choices. Fundamental legal concepts of evidence are indeed gradational. For example, relevance under Rule 401, or an issue such as “motive,” is not easily sorted into a “Yes or No” dichotomy. Sometimes the category is clearly a “Yes,” sometimes clearly a “No,” and often times is somewhere in between. Given that the law is forced to make morally, legally, or politically painful dichotomous choices, we are tempted to imagine that the concepts guiding our choices draw sharper distinctions than they do. In that light, we happily endorse Nance’s general thesis, which may lightheartedly be hyperbolized as the First Commandment of evidence scholarship: *Thou shalt not dichotomize, except from necessity.* And so, Nance argues:

- (i) that the reliability determination, necessitated by current jurisprudence but necessarily gradational, involves more than, and therefore cannot be stabilized by, the requirements of

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<sup>190</sup> *Id.* at 261.

<sup>191</sup> Faigman, *supra* note 43, at 262.

<sup>192</sup> *Id.* at 267.

<sup>193</sup> *Id.* at 264.

<sup>194</sup> See Nance, *supra* note 43, at 209.

<sup>195</sup> *Id.* at 193.

<sup>196</sup> *Id.* at 200-01.

relevance or expert qualifications;<sup>197</sup>

(ii) that “sufficient reliability,” “testing,” “peer review and publication,” and “low error rates” are as gradational as “reliability,” and therefore provide it no stability;<sup>198</sup>

(iii) that deference to scientific validity, itself a gradational concept, provides no stability (and there is no good reason to defer even if it did);<sup>199</sup> and

(iv) that almost all imaginable dichotomies developed as proxies for reliability *without deference to science* seem to be incompatible “with established doctrine and institutional capacities.”<sup>200</sup>

Nonetheless, “there is a germ of insight in such substitute approaches”—namely, Nance’s idea that we should “think in terms of *comparative* evaluation of . . . reliability.”<sup>201</sup> For Nance, this leads to a “better evidence” requirement—the appropriate basis for exclusion is that better evidence is available.<sup>202</sup>

In the opening pages of Nance’s study on the concept of reliability, he engages in some interesting “boundary work” by indicating his position on some of the debates in post-trilogy scholarship. First, Nance states that “concerns about jury misuse of expertise are less important than concerns about controlling advocates . . . .”<sup>203</sup> Thus, in the debate over juror shortcomings, Nance sides with those who subtly idealize the jury (and subtly blame lawyers and experts who need controlling). This position is often accompanied by a critique of the Rule 702 reliability requirement<sup>204</sup> as perhaps too restrictive.<sup>205</sup> Nance’s entire purpose, however, is to provide “a workable interpretation of the reliability requirement in Rule 702.”<sup>206</sup> Second, in passing, Nance mentions that “greater use of court-appointed experts [ ] may well be more important to the administration of justice” than his own interpretive argument.<sup>207</sup> His statement betrays not only humility, but also a subtle idealization of

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<sup>197</sup> See *id.* at 195.

<sup>198</sup> See *id.* at 197-200.

<sup>199</sup> See *id.* at 201-03.

<sup>200</sup> See Nance, *supra* note 43, at 209.

<sup>201</sup> *Id.* at 217.

<sup>202</sup> *Id.* at 240-43.

<sup>203</sup> *Id.* at 193.

<sup>204</sup> See, e.g., Mansfield, *supra* note 7, at 80 (arguing that Rule 702 of Federal Rules of Evidence need not be read as requiring “reliability,” though many tend to adopt that interpretation).

<sup>205</sup> See Cohen, *supra* note 16, at 963.

<sup>206</sup> See Nance, *supra* note 43, at 193.

<sup>207</sup> *Id.*

science as relatively objective if you can just get it out of the hands of advocates. Such a move is often accompanied by distrust of the jury. In Nance's case, however, his romantic image of the jury is counterbalanced by his romantic image of science. He is able rhetorically to proceed as if he is between the extremes of post-trilogy scholarship. Indeed, Nance subsequently takes the position that legal norms of reliability need not correspond to scientific norms of reliability.<sup>208</sup> This approach is typical of those who idealize law as capable of setting its own reliability standards. But when he contends that "[s]cientific validity, as understood by scientists, should not be considered necessary in all cases for adjudicative helpfulness,"<sup>209</sup> Nance is not adopting a modest view of science. Instead, Nance glamorizes the notion of "scientifically well-grounded conclusions"<sup>210</sup> as perhaps too high a goal for, and therefore irrelevant with respect to, the courtroom. In this Article, we have attempted to avoid idealization of law or science, focusing on application. In sharp contrast, Nance idealizes law and science. He worries that the focus on the "task at hand" would, if "pressed to its logical conclusion . . . make determinations of reliability all but impossible, for the particular task at hand in a lawsuit is never replicated in research."<sup>211</sup>

Still, there is much in Nance's study which we admire. He (i) rejects the notion of a "threshold of reliability" as a blind alley;<sup>212</sup> (ii) acknowledges the "disagreement among scientists and philosophers of science regarding the norms of scientific disciplines";<sup>213</sup> and (iii) recognizes that in the absence of a universal standard of validity, the scientific "community has developed dichotomous rules of thumb that, while over- and under-inclusive in some cases, roughly serve to further" the interests of science.<sup>214</sup> Thus, Nance reaches an appropriately modest assessment of the scientific enterprise. As to his analysis of the courts' capacity to discern reliability, Nance offers some modest recommendations that follow from his "gradational" thesis. Few would find his proposals controversial. For example, Nance states that "[c]urrent practice is often overly generous to proponents in allowing opinions on case-specific material facts, ultimate or not, when those facts are not within the personal

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<sup>208</sup> See *id.* at 203.

<sup>209</sup> *Id.* at 205.

<sup>210</sup> *Id.*

<sup>211</sup> See *id.* at 211.

<sup>212</sup> See Nance, *supra* note 43, at 220-21.

<sup>213</sup> *Id.* at 202.

<sup>214</sup> *Id.* at 203.

knowledge of the expert.”<sup>215</sup> In other words, Nance advocates more rigorous enforcement of the traditional rule that confined an expert’s opinion to the scope of his or her expertise (for example, an internist may not have expertise in oncology).<sup>216</sup> We think that such traditional principles enhance the focus on application.

Nance also condenses another version of the “scope of expertise” principle, explaining: “Many claims to science are really assertions of policy wrapped in the guise of science.”<sup>217</sup> Nance, like Professor Faigman, from whom Nance borrowed this language,<sup>218</sup> identifies a difficult and non-obvious aspect of the “scope” principle. Traditionally, this aspect took the form of a prohibition against experts instructing the jury on matters of law. To illustrate, when an expert opines that Practice *X* is safe while Practice *Y* is not, there is always a danger that the expert is confusing the normative with the statistical.<sup>219</sup> The temptation to permit experts to go beyond science and to broach matters of policy is powerful, and Nance’s advice is consistent with the views of those scholars who focus on degrees of confidence by experts.

Alternatively, consider Professor Imwinkelried’s criticism of Nance’s version of the ancient “best evidence rule”: Proffered expertise should be evaluated in comparison with alternative expertise, and excluded only when more reliable expertise is reasonably available to the proponent and not to the opponent.<sup>220</sup> Imwinkelried agrees that the “reliability-is-relative” (or “gradational”) principle works with respect to the validity of an expert’s specified theory or technique, the particular use of the expertise by a lawyer, and the definiteness or degree of certitude of the expert’s opinion.<sup>221</sup> Nevertheless, Imwinkelried observes that when Nance attempts to extend his thesis by invoking a best evidence rule, he begins to

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<sup>215</sup> *Id.* at 242-43.

<sup>216</sup> In response, Professor Imwinkelried endorses this part of Nance’s argument, albeit in different terms. See Edward J. Imwinkelried, *The Relativity of Reliability*, 34 SETON HALL L. REV. 269, 277-78 (2003) (“The degree of allowable definiteness of the expert’s final opinion should vary with the reliability foundation laid by the expert’s proponent.”).

<sup>217</sup> Nance, *supra* note 43, at 245 (quoting DAVID L. FAIGMAN, *LEGAL ALCHEMY: THE USE AND MISUSE OF SCIENCE IN THE LAW* 71 (1999)).

<sup>218</sup> See *id.*

<sup>219</sup> The well-known opinion of Justice Learned Hand in *The T. J. Hooper* is the *locus classicus* for this distinction. See 60 F.2d 737, 740 (2d Cir. 1932).

<sup>220</sup> See Imwinkelried, *supra* note 216, at 279 (citing Nance, *supra* note 43, at 237).

<sup>221</sup> See *id.* at 269 (stating that “Professor Nance . . . constructs a persuasive case that there is no invariant reliability threshold or uniform, minimum reliability level” for admissibility).

idealize the courtroom:<sup>222</sup>

The proposal multiplies the number of foundational issues . . . before [the] final ruling . . . . The judge must decide: (1) whether the proponent's evidence is "reliable"; (2) whether other expert techniques address the same question; (3) whether those techniques are better . . . ; (4) whether a better technique is reasonably available to the proponent [and] (5) . . . to the opponent; and (6) whether the proponent is a repeat player. The administration of this rule during a jury trial will necessitate either horrendously long sidebar conferences or prolonged recesses.<sup>223</sup>

As we agree with Imwinkelried that Nance takes his relativity principle too far, we obviously disagree with Faigman that Nance does not go far enough.<sup>224</sup> Faigman, even more than Nance, idealizes the competence of judges well beyond what is reasonable to expect. Faigman would have the trial judge participate in the culture of science, and consider the effect that judicial decisions have on creating incentives for developing better science.<sup>225</sup> Indeed, Faigman correctly argues that judicial decisions will offer incentives to out-of-court players in the game of science.<sup>226</sup> Nevertheless, it does not follow that judges should attempt to construct decisions that maximize the social utility of science.

As our review of post-trilogy discourse demonstrates, the idealizations of particular aspects of law—of appellate panels, of juries, and of trial judges—has resulted in impractical proposals. Sometimes, as with Professor Mansfield, idealization of the jury interferes with the ability to appreciate *Kumho Tire's* emphasis on the "case at hand." For Mansfield, *any* reliability standard, even one that realistically acknowledges the pragmatic character of the scientific enterprise and the diversity of methodologies, poses a threat to the jury's role.<sup>227</sup> With respect to the other scholars discussed in this

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<sup>222</sup> See *id.* at 283 (arguing that Nance's proposal "would . . . render Rule 702 unworkable . . .").

<sup>223</sup> *Id.* at 283-84.

<sup>224</sup> See Faigman, *supra* note 43, at 261 ("Where Professor Nance would have judges ask whether better evidence is available, I would have them ask whether better evidence *should be* available.").

<sup>225</sup> See *id.* at 258 ("[T]he evidentiary determination regarding expert evidence must . . . integrate . . . the culture of the scientific method."), 261 ("Professor Nance . . . fails to take into account the law's effect on many of the disciplines that hawk their wares at the courthouse door.").

<sup>226</sup> See *id.* at 262 ("Simply put, if courts demand better evidence than what has so far been done, then, and often only then, will that work be done.").

<sup>227</sup> See generally Mansfield, *supra* note 7.

Article, idealization of appellate review or the trial judge also deflects attention away from the application phase, because the difficult problem of evaluating science is evaded by imagining an arbiter above the fray. In our view, the application phase includes fallible, but typically competent, lawyers and their experts, trial judges, juries, and appellate judges, each of whom has a corrective role to play. Neither demonizing nor idealizing any one of them is necessary (nor helpful), and avoiding such tendencies keeps the focus on the production of evidence *for the case at hand*.

## V. CONCLUSION

[T]hree points come [through] clearly from *Kumho Tire*. First, a court must review the reliability of the proffered expertise specifically as it applies to the task for which it is being utilized in the litigation in which it is offered, not in some more global sense. Second, a court is obliged to . . . select the most appropriate criteria of reliability for the kind of expertise being proffered, given the circumstances of its generation in the particular case. . . . Third, . . . the presence or absence of one or more [of the *Daubert* factors] is not necessarily dispositive of sufficient reliability to gain admission.<sup>228</sup>

Possibly because of the danger that the law may ignore “global” reliability, some evidence scholars resist the current focus on the “task at hand”; recall Professor Allen’s concern, echoed by Professor Moreno, that “without global reliability, one has gibberish.”<sup>229</sup> Even among scientists, however, “reliability” is an ephemeral concept<sup>230</sup> and, in both global and local contexts, a question of degree.<sup>231</sup> Accordingly, the task of establishing global reliability in a particular case is impractical. Global reliability is no guarantee that a particular application by an expert is reliable.<sup>232</sup> Even when a “global warrant” is unavailable, a particular application, if accompanied by an appropriate “level of confidence,” can assist a jury.<sup>233</sup>

Reliability, nevertheless, is not so vague as to be useless. We reject both the idealization of science (or scientific methodology) as a source of uncontroversial knowledge (or standards), as well as

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<sup>228</sup> Denbeaux & Risinger, *supra* note 11, at 31-32.

<sup>229</sup> See *supra* note 66 and accompanying text.

<sup>230</sup> See *supra* notes 81 & 113 and accompanying text.

<sup>231</sup> See Nance, *supra* note 43, at 193.

<sup>232</sup> See *supra* note 100 and accompanying text.

<sup>233</sup> See Caudill & LaRue, *supra* note 25, at 33-35 (discussing cases where experts’ testimony fared badly under *Daubert* guidelines but was admissible in light of practical goals and limitations of science).

idealizations of law that seem to render scientific standards superfluous. Between those poles lies a balanced respect for both law and science that allows simultaneous acknowledgement of their practical goals and limitations. In many important respects, science differs from law. Law may govern and regulate a field of science, but does much more. Likewise, science does more than produce useful knowledge for law. Nonetheless, both fields share the characteristics of a social institution: both are communities with (albeit distinct) conventions, consensus-building techniques, rhetorical strategies, gatekeeping procedures, and internal debates. When an expert is judged as to whether his or her application is reliable, an immodest view of science may lead to an unjustified restrictiveness. In contrast, those who possess modest views of science are not surprised by degrees of reliability, uncertainty, scientific conflict, alternative explanations, mere probabilities, incomplete data, or the funding of research.<sup>234</sup> The other extreme, however, is just as impractical—namely, the expectation that appellate review, scientifically astute or policy-making trial judges, or juries can function to solve or evade the problem of discerning reliable expertise. Though we share the confidence that appellate judges can correct abuses of discretion by means of evidentiary rulings, we have no illusions that, as a group, they possess the time and resources to serve as an anchor for disputes over reliability, such that *de novo* review should be the norm. Likewise, we support efforts to educate judges. In addition to scientific methodology, however, pragmatic social, institutional, and rhetorical aspects of science must also be understood.<sup>235</sup> Some argue that judges should make “science policy” and set their own standards for sufficiency apart from science. Admissibility standards are legal, not scientific, constructions; yet, this is not without risks. If scientific expertise (even modestly conceived) is disengaged from law, the courtroom might be saddled with an unscientific (and therefore unreliable) “legal science.”

Finally, with respect to those who question reliability standards by way of idealizing the jury’s role, we are concerned, like Professor Sanders, about jurors’ capacities to understand complex scientific evidence, even though the research on that problem is not conclusive.<sup>236</sup> While Professors Mansfield and Nance reject the notion that jurors are unsophisticated,<sup>237</sup> we consider this debate as

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<sup>234</sup> See *id.* at 20-36.

<sup>235</sup> See generally *id.*

<sup>236</sup> See generally Sanders, *supra* note 21.

<sup>237</sup> See Mansfield, *supra* note 7, at 86; Nance, *supra* note 43, at 232-33.

significant. Unlike Professor Faigman, we cannot casually disregard it.<sup>238</sup> This Article does not attempt to idealize jurors, trial judges, or even appellate judges. No single structure of the legal system provides a “panic room” where one can escape the problems of reliable applications.

Recognizing the practical limitations of both science and law helps scholars to avoid impractical proposals for reform. Typically, since the pragmatic aspects of science are demonized, idealizations of science result in proposals for an overly restrictive regime. Idealizations of law likewise generally produce proposals with impractical features. Romantic images of appellate courts fail to recognize their limited resources. Romantic images of trial judges overestimate their capacity to criticize expertise and make “science policy” decisions. And romantic images of the jury disengage science from law. Moreover, proposals to raise the admissibility standards for forensic scientists, or lower them for civil trial plaintiffs, if successful, could engender other changes to the legal system. To illustrate, if courts agree to go easier on plaintiffs’ experts, tort reform advocates might gain the upper hand. As Professor Lillquist humorously, and shrewdly, suggests, if

Judge Pollak had stuck to his decision in *United States v. Llera Plaza*, limiting the testimony of the government’s fingerprint examiners and forbidding them from opining that a particular print is from a particular person . . . it seems to me at least possible, if not likely, that Congress would have quickly passed legislation entitled something like the Latent Fingerprint Admissibility Act of 2002.<sup>239</sup>

For our purposes, changes in one area of the law can generate a backlash. Therefore, solutions must take into account the practical context. In the area of admissibility of scientific expertise, for example, arguments for restrictiveness that rest on idealizations of science are met with arguments that idealize the jury in order to overcome those restrictions. So long as one modestly views both science and law as pragmatic enterprises, the trilogy’s focus on application can work in its present form without raising or lowering reliability standards, and without changing the abuse-of-discretion standard or the current roles of judges as gatekeepers and jurors as beneficiaries (or victims) of a gatekeeping regime.

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<sup>238</sup> See Faigman, *supra* note 43, at 265.

<sup>239</sup> See R. Erik Lillquist, *A Comment on the Admissibility of Forensic Evidence*, 33 SETON HALL L. REV. 1189, 1203-04 (2003).