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A "DOGMA OF EMPIRICISM" REVISITED: *DAUBERT V. MERRELL DOW PHARMACEUTICALS, INC.* AND THE NEED TO RESURRECT THE PHILOSOPHICAL INSIGHT OF *FRYE V. UNITED STATES*

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I. INTRODUCTION

Daubert v. Merrell Dow Pharmaceuticals, Inc.[1] has been regarded as "the most important case on the admissibility of scientific evidence . . . [since] *Frye v. United States*."[2] *Frye*[3] was the first American case to delineate a special standard for determining when a subject matter counts as an area of scientific expertise about which a properly qualified expert can testify.[4] In 1923, the United States Court of Appeals for the D.C. Circuit held in *Frye* that to be admissible, proffered testimony by a scientific expert must be based on a discovery or principle that has "gained general acceptance in the particular field in which it belongs."[5] *Frye*'s "general acceptance" test became the majority rule in

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1. 509 U.S. 579 (1993).

2. Paul C. Giannelli, *Forensic Science: Frye, Daubert, and the Federal Rules*, 29 CRIM. L. BULL. 428 (1993). *See also* Bert Black et al., *Science and the Law in the Wake of Daubert: A New Search for Scientific Knowledge*, 72 TEX. L. REV. 715, 745 (1994) ("If nothing else, the widespread interest in *Daubert* demonstrates the crucial role science plays in modern litigation. The *Daubert* case attracted twenty-two amicus briefs from over one hundred organizations and individuals and filled the Supreme Court to overflowing the day it was argued." (footnotes omitted)); Edward J. Imwinkelried, *The Daubert Decision: Frye Is Dead: Long Live the Federal Rules of Evidence*, TRIAL, Sept. 1993, at 60, 61.

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

4. *See* Black et al., *supra* note 2, at 722 ("recognition of the peculiar problems of scientific evidence originated with [*Frye*]," citing CHARLES T. MCCORMICK, 1 MCCORMICK ON EVIDENCE § 203, at 869 (John W. Strong ed., 4th ed. 1992)).

The question in *Daubert* and *Frye* — when an area counts as an admissible subject of testimony by scientific experts — is distinct from the question of when a witness qualifies as an expert. While the issue of when someone qualifies as an expert applies to both scientific and non-scientific experts, the issue of when the subject matter of testimony is genuinely scientific applies to scientific expert testimony alone. *See* Daubert v. Merrell Dow Pharmaceuticals, Inc., 43 F.3d 1311, 1315 (9th Cir. 1995) [hereinafter *Daubert II*]. *See also* David E. Bernstein, *The Admissibility of Scientific Evidence After Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 15 CARDOZO L. REV. 2139, 2161 (1994).

As Justice Rehnquist recognized, *Daubert* left open the question of whether, in addition to determining whether an area is one of scientific expertise, its proposed criteria more generally determine whether an area is one about which the requisite specialized knowledge exists for expert testimony to be admissible. *See Daubert*, 579 U.S. at 600. While scholars have argued that *Daubert*'s criteria should apply to all areas of expert knowledge, this Article's critique implies that the *Daubert* criteria provide no basis for a rational judicial determination of whether scientific or any other kind of expertise exists. *See, e.g.*, David L. Faigman, *Mapping the Labyrinth of Scientific Evidence*, 46 HASTINGS L.J. 555, 559 (1995) ("[R]eading [*Daubert* to apply only to 'scientific knowledge'] displays a crabbed interpretation of the Court's opinion as well as a misconstruction of the principles underlying Rule 702" (citation omitted)); Linda S. Simard & William G. Young, *Daubert's Gatekeeper: The Role of the District Judge in Admitting Expert Testimony*, 68 TUL. L. REV. 1457, 1470-74 (1994).

5. *Frye*, 293 F. at 1014. Strictly speaking, a special standard for the admission of scientific evidence determines the admissibility of both (i) qualified expert testimony about an allegedly scientific theory or technique and (ii) lay testimony induced by an allegedly

both federal and state courts.[6] Nonetheless, all members of the Supreme Court agreed in 1993 in *Daubert* that, properly interpreted, the Federal Rules of Evidence had superseded *Frye*.[7]

Seven of these justices in *Daubert* endorsed an alternative approach for determining the admissibility of scientific evidence under the Federal Rules.[8] Through an analysis of the *Daubert* majority's proposed alternative to the *Frye* standard, this Article shows that the *Daubert* decision rests on a basic misunderstanding of the history and philosophy of science. In contrast, this analysis will imply that the much-maligned *Frye* standard[9] is grounded in a fundamental philosophical insight. This Article contends, however, that major modifications in prevailing interpretations are needed to rescue *Frye* from the muddle created in seventy-odd years of judicial application.

Part II of this Article addresses a critical assumption on which *Daubert*'s alternative to *Frye* is based. *Daubert* assumes that judges can resolve disputes about whether expert testimony is genuinely scientific by rationally and uncontroversially determining whether the scientific method was followed in developing the theory or technique at issue. While *Daubert* suggests four factors that judges may appropriately

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scientific technique, such as hypnosis. *See* People v. Shirley, 723 P.2d 1354, 1375 (Cal. 1982) (*Frye* determines the admissibility of a lay witness's hypnotically induced testimony because "if the testimony is thus only as reliable as the hypnotic process itself, it must be judged by the same standards of admissibility."); Polk v. State, 427 A.2d 1041, 1048 (Md. 1981), *quoted in Shirley*, 723 P.2d at 1372 ("The technique of hypnosis is scientific, but the testimony itself of the witness is the end product of the administration of the technique. The induced recall of the witness is dependent upon, and cannot be disassociated from, the underlying scientific method. Accordingly, . . . the *Frye* test must be applied in the instant case.").

6. *See Daubert*, 509 U.S. at 585. *But see* Edward R. Becker & Aviva Orenstein, *The Federal Rules of Evidence after Sixteen Years*, 60 GEO. WASH. L. REV. 857, 877 (1992) (citing cases in support of the proposition that "the circuits are sharply divided on [the] applicability [of *Frye*], as are the state courts").

7. *Daubert*, 509 U.S. at 587. *Daubert* is a response to what Becker and Orenstein called "the greatest single oversight in the [Federal] Rules [of Evidence] — failure to clarify the standard for admitting novel scientific evidence." *See* Becker & Orenstein, *supra* note 6, at 877.

8. Chief Justice Rehnquist, joined by Justice Stevens, dissented from *Daubert*'s delineation of an alternative to *Frye*. 509 U.S. at 598 (Rehnquist, C.J., concurring in part and dissenting in part).

9. *See, e.g.*, *Daubert*, 509 U.S. at 585 (stating that the D.C. Circuit set forth the *Frye* standard "[i]n what has become a famous (perhaps infamous) passage"); *Developments in the Law — Confronting the New Challenges of Scientific Evidence*, 108 HARV. L. REV. 1481, 1486 (1995) [hereinafter *New Challenges of Scientific Evidence*] ("[C]ommentators have argued that the *Frye* regime . . . perpetuated the legal system's failure to incorporate scientific evidence into a coherent framework."); Ronald J. Allen, *Expertise and the Daubert Decision*, 84 J. CRIM. L. & CRIMINOLOGY 1157, 1164 (1994) (referring to "the well-founded belief that [the *Frye* rule] has lost touch with modern science").

consider in this determination, only one of the factors is plausible. That factor is the falsifiability criterion that the *Daubert* majority explicitly adopted from philosopher Sir Karl Popper: "The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability."[10] Part III shows, however, that widespread criticisms of Popperian falsifiability by philosophers and historians of science reveal the intellectual bankruptcy of Popper's — and *Daubert*'s — quest for an Archimedean standpoint from which to judge whether science is being done.[11]

Part IV argues that by making acceptance by the relevant scientific community the criterion for the admissibility of scientific evidence, *Frye* implicitly recognizes that there is no extra-scientific standpoint from which judges — or any one else — can rationally assess the scientific merits of proposed scientific evidence.[12] Admissibility can be based on

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10. *Daubert*, 509 U.S. at 593 (citation omitted); *see also id.* at 600 (Rehnquist, C.J., concurring in part and dissenting in part).

11. Numerous commentators have repeated *Daubert*'s mistake of failing to realize that a broad consensus of historians and philosophers of science has discredited Popper's falsifiability criterion. Thus, Black et al. state that "the single most salient characteristic [of scientific knowledge] is falsifiability" and suggest that Popperian falsifiability is "the 'criterion of demarcation' that sets science apart from other forms of knowledge." *Supra* note 2, at 753-56. *See also* Randolph N. Jonakait, *The Meaning of Daubert and What That Means for Forensic Science*, 15 CARDOZO L. REV. 2103, 2107 & n.24 (1994) (citing Popper as a "distinguished philosopher of science" and stating that "[t]he defining touchstone of science is a testable proposition that is tested"); Susan R. Poulter, *Daubert and Scientific Evidence*, 1993 UTAH L. REV. 1307, 1319 & n.72 (1993) (recognizing the Popperian roots of the concept of falsifiability and mistakenly claiming that the concept is both easy to apply and a valid criterion of scientific status); Daniel J. Capra, *Further Open Questions After 'Daubert,'* N.Y. L.J., July 14, 1995, at 3, 13; Paul C. Giannelli, *Daubert: Interpreting the Federal Rules of Evidence*, 15 CARDOZO L. REV. 1999, 2002 & n.23 (1994); Margaret A. Berger, *Procedural Paradigms for Applying the Daubert Test*, 78 MINN. L. REV. 1345, 1353 (1994); Veronica I. Larvie, *Evidence — Admissibility of Scientific Evidence in Federal Courts — the Supreme Court Decides that Frye Is Dead and the Federal Rules of Evidence Provide the Standard, But Is There a Skeleton in the Closet?*, 29 LAND & WATER L. REV. 275, 287 n.116 (1994); Michael H. Graham, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: No Frye, Now What?*, 30 CRIM. L. BULL. 153, 162 (1994); Imwinkelried, *supra* note 2, at 63; Barry C. Scheck, *DNA and Daubert*, 15 CARDOZO L. REV. 1959, 1968 (1994); Richard M. Friedman, *The Death and Transfiguration of Frye*, 34 JURIMETRICS J. 133, 140 n.29 (1994); Amy T. Schutz, Note, *The New Gatekeepers: Judging Scientific Evidence in a Post-Frye World*, 72 N.C. L. REV. 1060, 1066 (1994); Bernstein, *supra* note 4, at 2153; *The Supreme Court, 1992 Term — Leading Cases*, 107 HARV. L. REV. 144, 257 n.3 (1993).

Social scientists have made the same mistake. *See, e.g.*, R.L. AKERS, CRIMINOLOGICAL THEORIES: INTRODUCTION AND EVALUATION 26 (1994) ("If criminological theories are to be scientific, then they must be judged by scientific criteria. The most important of these . . . is the extent to which a theory can be verified or refuted.").

12. *Daubert*'s alternative to *Frye* does not merely face practical problems having to do with limits on judicial competence. Rather, no matter how competent they are, it is in principle impossible for judges — or anyone else — to achieve *Daubert*'s goal of standing

scientific merit only if judges defer to practicing scientists' assessments of scientific merit.

Although this Article argues that *Frye* is based on a fundamental philosophical insight,[13] the current *Frye* standard nonetheless requires major changes. The vagueness of *Frye*'s notions of the relevant scientific community and of general acceptance has allowed judges to pay lip service to *Frye*, yet base admissibility decisions on their own substantive scientific judgments and/or personal biases. To prevent this surreptitious violation of *Frye*'s core dictate of judicial deference to scientists, Parts V-VII advance a philosophically revamped *Frye* standard.

This critique implicitly questions *Daubert*'s unanimous holding that, as a matter of statutory interpretation, the Federal Rules of Evidence superseded *Frye*.[14] Under both *Daubert* and *Frye*, a jury is permitted to hear proposed scientific testimony only if a judge determines beforehand

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outside science and rationally deciding whether science is being done. For the mistaken view that the problems with *Daubert*'s alternative are merely practical, see, e.g., *Daubert*, 509 U.S. at 600-01 (Rehnquist, C.J., concurring in part and dissenting in part), discussed *infra* note 48 and accompanying text; Graham, *supra* note 11, at 162, 165-66, 171; *New Challenges of Scientific Evidence*, *supra* note 9, at 1515 ("[T]estability or falsifiability may be a theoretically appealing criterion, but it may be too complicated for courts to apply."); Scheck, *supra* note 11, at 1961 ("In an era of overcrowded dockets, it will not be the natural inclination of judges to embrace with enthusiasm *Daubert*'s 'fine grained inquiry into the methodology and reasoning employed by the expert in reaching her opinion.'" (citation omitted)); Poulter, *supra* note 11, at 1320, 1333; Robert H. Sand, *The Supreme Court Comes Face-to-face with 'Junk Science' and Punitive Damage Awards . . . and Seems to Turn Away*, 19 EMPLOYEE RELATIONS L.J. 269, 278 (1993); Paul S. Milich, *Controversial Science in the Courtroom: Daubert and the Law's Hubris*, 43 EMORY L.J. 913, 918-20, 924-25 (1994); Sheila Jasanoff, *What Judges Should Know about the Sociology of Science*, 77 JUDICATURE 77 (1993); Note, *Daubert v. Merrell Dow Pharmaceuticals: Pushing the Limits of Scientific Reliability—The Questionable Wisdom of Abandoning the Peer Review Standard for Admitting Expert Testimony*, 47 VAND. L. REV. 1175, 1197-99 (1994).

13. This Article's argument is based on widely accepted criticisms of Popperian and logical positivist philosophy of science. Multiple authors have taken the mistaken view that the distinctive positions of Thomas Kuhn, as opposed to positions that Kuhn shares with a broad consensus of philosophers and historians of science, are crucial to criticizing *Daubert*'s alternative to *Frye*. *See generally* THOMAS S. KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (1970); *see also* Allen, *supra* note 9, at 1171-72 (1994); Note, *supra* note 12, at 1202-1203; Margaret G. Farrell, *Daubert v. Merrell Dow Pharmaceuticals, Inc.: Epistemology and Legal Process*, 15 CARDOZO L. REV. 2183, 2195-96 (1994).

Farrell correctly recognizes that "[u]ltimately, the answer to the question posed in *Daubert*—how can law use scientific knowledge — turns on epistemological theories about the nature of 'facts' and how we know them." Farrell, *supra* at 2189. However, Farrell's contention that *Daubert* inconsistently combines two incompatible philosophical positions rests on serious misunderstandings of Popper's, Hempel's, and Kuhn's positions. *See infra* notes 47, 65, and 128.

14. *See Daubert*, 509 U.S. at 587.

that the testimony pertains to genuine science. Contrary to *Daubert*'s assumption that judges can make such determinations without deferring to scientists, the history and philosophy of science show that it is in principle impossible for judges, or anyone else outside the scientific community, to rationally decide whether science is being done. Since statutes are to be interpreted so as to avoid absurd results,[15] the Federal Rules of Evidence should be read to incorporate the judicial deference dictated by *Frye*.[16]

This Article's arguments do not hinge, however, on issues of statutory interpretation. Even if the *Daubert* Court correctly interpreted the Federal Rules of Evidence, the *Daubert* standard rests on a mythical conception of scientific activity. This Article instead proposes a revamped *Frye* standard that accords with a realistic conception of scientific activity, which should be adopted by both state and federal courts.[17]

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15. See United States v. Turkette, 452 U.S. 576, 589 (1981).

16. This Article's argument suggests, more generally, that the "ordinary" or "plain" meaning of words in a statute can never be dispositive where the statute incorporates complex cultural concepts such as science. Ordinary speakers are only too likely to subscribe to philosophical misunderstandings of science and other complex cultural activities. *See, e.g.*, C. WRIGHT MILLS, THE SOCIOLOGICAL IMAGINATION 123 (1959) ("The everyday empiricism of common sense is filled with assumptions and stereotypes of one or another particular society . . . ."); Sabbath v. United States, 391 U.S. 585, at 589 (1968) ("[L]inguistic analysis seldom is adequate when a statute is designed to incorporate fundamental values . . . .").

17. Many states have adopted *Daubert*. *See, e.g.*, State v. Streich, 658 A.2d 37, 47 (Vt. 1995); State v. Schweitzer, 533 N.W.2d 156, 159 (S.D. 1995); Taylor v. State, 889 P.2d 319, 327-28 (Okla. 1995); State v. O'Key, 899 P.2d 663, 677-80 (Or. 1995); Commonwealth v. Lanigan, 641 N.E.2d 1342, 1348-49 (Mass. 1994); State v. Foret, 628 So.2d 1116, 1121-23 (La. 1993); State v. Springfield, 860 P.2d 435, 443 (Wyo. 1993); *see also* State v. Alberico, 861 P.2d 192, 203 (N.M. 1993) (rejecting *Frye* and holding that scientific evidence should be admitted on the basis of a reliability determination including the four *Daubert* factors and "whether the scientific technique is based upon well-recognized scientific principles and whether it is capable of supporting opinions based upon reasonable probability rather than conjecture").

Other state courts have explicitly held that *Frye* remains state law. *See, e.g.*, People v. Leahy, 882 P.2d 321, 331 (Cal. 1994); State v. Carter, 524 N.W.2d 763, 779 (Neb. 1994); People v. Wesley, 633 N.E. 2d 451, 453-54 (N.Y. 1994); State v. Bible, 858 P.2d 1152, 1183 (Ariz. 1993); Flanagan v. State, 625 So.2d 827, 828 (Fla. 1993); Fishback v. People, 851 P.2d 884 (Colo. 1993).

II. *DAUBERT*'S ALTERNATIVE TO *FRYE*

To appreciate the *Daubert* decision, it is necessary to see that both *Daubert* and *Frye* make judges gatekeepers for scientific evidence. A jury is permitted to hear proposed scientific testimony only if a judge determines beforehand that the testimony pertains to genuine science.[18] The philosophical core of *Frye* is that, in performing this gatekeeping role, judges should defer completely to scientists: acceptance by scientists is the sole criterion for determining whether an area constitutes a genuine area of scientific expertise about which a properly qualified expert can testify.[19] In contrast, the *Daubert* Court held that testimony will be classified as scientific, and thus presented to a jury as expert testimony, only if a judge first determines that it consists of inferences and assertions "derived by the scientific method."[20]

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18. *Daubert* held: "Faced with a proffer of expert scientific testimony, . . . the trial judge must determine at the outset . . . whether the expert is proposing to testify to (1) scientific knowledge that (2) will assist the trier of fact to understand or determine a fact in issue." 509 U.S. at 592 (footnote omitted). This Article's consideration is limited to the first issue.

See, e.g., Black et al., *supra* note 2, at 724 (noting that *Daubert* agreed with *Frye* in affirming that "trial judges must screen scientific evidence before admitting it, which rekindles the question of how a court should determine scientific validity"); Simard & Young, *supra* note 4, at 1458, 1465.

19. *See, e.g.*, United States v. Yee, 134 F.R.D. 161, 197 (N.D. Ohio 1991), *aff'd sub nom.*, United States v. Bonds, 12 F.3d 540 (6th Cir. 1993) (warning against converting a *Frye* hearing "limited solely to the question of whether the proponent has shown general acceptance . . . into a hearing whose outcome is dependent on the court's determination of the validity and reliability of the scientific method employed by the proponent."); People v. Kelly, 549 P.2d 1240, 1244 (Cal. 1976) ("*Frye*, and the decisions which have followed it, rather than turning to the trial judge have assigned the task of determining reliability of the evolving technique to members of the scientific community from which the new method emerges."); Scheck, *supra* note 11, at 1959 ("In theory, the *Frye* test avoids the danger of converting the courtroom into a laboratory with the judge sitting as a peer reviewer. Judges are not supposed to substitute their own assessments of scientific validity for the judgment of scientists. Rather, judges merely 'count noses.'" (citation omitted)); *see also infra* Parts IV-VII (discussing the various standards that courts have constructed around *Frye*'s philosophical core).

20. *Daubert*, 509 U.S. at 590. *See also Daubert II*, 43 F.3d at 1316 ("As we read the Supreme Court's teaching in *Daubert*, . . . though we are largely untrained in science and certainly no match for any of the witnesses whose testimony we are reviewing, it is our responsibility to determine whether those experts' proposed testimony amounts to 'scientific knowledge,' constitutes 'good science,' and was 'derived by the scientific method.'" (citations omitted)); Scheck, *supra* note 11, at 1960 ("*Daubert* requires more than just nose counting . . . . Under *Daubert*, judges are explicitly directed to make an assessment of the 'scientific validity' of the proposed scientific evidence."); Sand, *supra* note 12, at 278 ("Under *Daubert*, trial judges cannot simply rely on the judgment of the scientific community . . . . Rather, the trial judge is encouraged to actively participate in the scientific process . . . ."); Faigman, *supra* note 4, at 556 ("By replacing the general acceptance

Daubert's core premise is that judges can — and must — decide whether proferred scientific testimony is based on the scientific method without taking a position regarding the truth of particular scientific conclusions. Judges are advised that while deciding whether to admit the scientific evidence, "[t]he focus, of course, must be solely on principles and methodology; not on the conclusions that they generate."[21] This rests on the assumption that the conclusions, but not the method, of science are inherently corrigible. Thus, *Daubert* warns that science cannot be identified with a body of established truths. "Of course, it would be unreasonable to conclude that the subject of scientific testimony must be 'known' to a certainty; arguably, there are no certainties in science."[22]

However, if what the scientific method consists of were also uncertain, scientific controversy could infect admissibility decisions proceeding from the identification of a particular methodology with the scientific method, as well as those proceeding from the acceptance of particular scientific conclusions. Therefore, *Daubert*'s exclusive focus on method rests on the philosophical premises that the scientific method is itself scientifically uncontroversial and that by appealing to this method, judges can avoid taking a stand on any substantive scientific questions, yet rationally determine whether "an expert's testimony [does or does not] pertain . . . to 'scientific knowledge.'"[23]

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standard of *Frye v. United States* with the validity standard of *Daubert*, the Court discarded a standard that was deferential to external groups with [sic] one that requires judges themselves to make the necessary determination." (footnote omitted)).

21. *Daubert*, 509 U.S. at 595. The *Bonds* court recognized:

> [T]he *Daubert* Court has instructed the courts that they are not to be concerned with the reliability of the conclusions generated by valid methods, principles and reasoning. Rather, they are only to determine whether the principles and methodology underlying the testimony itself are valid. If the principles, methodology and reasoning are scientifically valid then it follows that the inferences, assertions and conclusions derived therefrom are scientifically valid as well.

12 F.3d at 556. *See also* Kenneth J. Chesebro, *Taking Daubert's "Focus" Seriously: The Methodology/Conclusion Distinction*, 15 CARDOZO L. REV. 1745 (1994).

22. *Daubert*, 509 U.S. at 590 (citations omitted).

23. *Id.* Justice Rehnquist fails to grasp the philosophical assumptions underlying *Daubert*'s alternative to *Frye* when he criticizes the majority on the ground that Federal Rule of Evidence 702 does not impose on judges either "the obligation or the authority to become amateur scientists." *Id.* at 601. By assuming that admissibility decisions can be based solely on a scientifically uncontroversial conception of the scientific method, the *Daubert* majority assumed that judges can decide whether proffered expert testimony pertains to genuine science without either deferring to scientists or doing science themselves. Milich, *supra* note 12, at 918-20, and Scheck, *supra* note 11, at 1984, similarly fail to grasp that *Daubert*'s underlying philosophical assumption is that judges need not antecedently take sides on any substantive scientific disputes to rationally decide whether science is being done. *See also infra* note 44.

As a practical matter, however, judges will be called upon to make such admissibility determinations only where some qualified scientists are willing to testify about a theory or technique that other qualified scientists dismiss as pseudo-scientific.[24] Thus, one of *Daubert*'s key assumptions is that, without antecedently adopting the substantive positions of some competing scientists but not others, judges can resolve qualified scientists' disagreements about whether work is or is not genuinely scientific.[25] Moreover, according to *Daubert*, judges can reach a resolution without resorting to their personal biases or hunches; they instead need only consider whether the scientific method has been used.[26] *Daubert*'s alternative to *Frye* thus rests on the assumption that judges, standing apart from all relevant scientific controversies, can rationally decide whether science is being done.

Accordingly, the fundamental questions raised by *Daubert*'s alternative are whether a rational, extra-scientific standpoint exists and if so, whether it is possible for judges to proceed from it to decide if proferred expert testimony pertains to genuine science. Hence, it is necessary to consider whether *Daubert* cogently guides judges to the requisite Archimedean standpoint. *Daubert* does "not presume to set out a definitive checklist or test."[27] However, in the guise of presenting "some general observations,"[28] the majority delineated four factors for judges to consider in reaching "a preliminary assessment of whether the reasoning or methodology underlying the testimony is scientifically

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24. *See, e.g., Daubert*, 509 U.S. at 582 (noting that both the respondent's and petitioners' experts "possessed impressive credentials"); *Daubert II*, 43 F.3d at 1315 ("The question of admissibility only arises if it is first established that the individuals whose testimony is being proffered are experts in a particular scientific field . . . .").

25. *See Daubert II*, 43 F.3d at 1316 ("Our responsibility, then, unless we badly misread the Supreme Court's opinion, is to resolve disputes among respected, well-credentialed scientists about matters squarely within their expertise, in areas where there is no scientific consensus as to what is and what is not 'good science,' and occasionally to reject such expert testimony because it was not 'derived by the scientific method.'" (citation omitted)); *The Supreme Court, 1992 Term — Leading Cases*, *supra* note 11, at 261 ("If the scientific community . . . is undecided about a particular technique or theory, can judges be expected to have the competence necessary to determine whether the technique or theory is scientifically valid?" (footnote omitted)).

26. Recognizing that *Daubert* makes this assumption, Joan E. Bertin & Mary S. Henifin, *Science, Law and the Search for Truth in the Courtroom*, 22 J. LAW, MED. & ETHICS 6, 9 (1994), contend that *Daubert*'s reasoning is called into question by the fact that "[t]he [*Daubert*] plaintiffs claimed that their expert testimony was based on valid and reliable scientific methods. The defendant disputed that claim. The issue thus revolved around the difficulty of determining whether methodologies are in fact valid and reliable, in the face of contradictory claims by respectable scientists."

27. *Daubert*, 509 U.S. at 593.

28. *Id.*

valid."[29] In evaluating *Daubert*'s alternative to *Frye*, one must consider whether the four *Daubert* factors enable judges to resolve contests about the scientific status of proferred testimony by rationally and uncontroversially determining whether the scientific method was used.

A negative answer will not show, however, that *Daubert*'s alternative to *Frye* is unviable. The *Daubert* majority explicitly declined to decide whether its four factors were either necessary or sufficient components of an adequate criterion of the scientific method.[30] Even if *Daubert*'s four factors are inadequate, one still must consider whether judges can in principle attain the requisite Archimedean standpoint.[31]

This Article contends that it is impossible to construct criteria of the scientific method that allow judges both to adhere strictly to an unbiased, extra-scientific standpoint and to make rational admissibility decisions that are not under- or over-inclusive. It follows that *Daubert*'s alternative to *Frye*, which demands that both of these requirements be satisfied, is unsound.

Proposed criteria of the scientific method may allow judges to stray from an unbiased, external standpoint in two ways. First, the criteria may require or allow judges to adopt particular scientists' substantive views in order to determine whether testimony is based on the scientific method. If this occurs, independent judicial gatekeeping will be replaced

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29. *Id.*

30. *Daubert* admonishes: "[T]he inquiry envisioned by Rule 702 is, we emphasize, a flexible one." 509 U.S. at 594. It further states:

> A number of authorities have presented variations on the reliability approach, each with its own slightly different set of factors. To the extent that they focus on the reliability of evidence as ensured by the scientific validity of its underlying principles, all these versions may well have merit, although we express no opinions regarding any of their particular details.

Id. at 595 n.12 (citations omitted). *See also Daubert II*, 43 F.3d at 1316-17 ("We read these [four *Daubert*] factors as illustrative rather than exhaustive"); United States v. Crumby, 895 F. Supp. 1354, 1358 (D. Ariz. 1995) ("[T]he Supreme Court delineated a non-exhaustive list of factors to be considered by a district court. The factors are not talismanic."); *The Supreme Court — Leading Cases*, *supra* note 11, at 257.

31. Black et al. similarly state:

> Justice Blackmun emphasized the need for flexibility in applying Rule 702 and the need to focus on the "overarching subject [of] the scientific validity — and thus the evidentiary relevance and reliability — of the principles that underlie a proposed submission." Heeding this admonition will require far more than the kind of label and checklist approaches that have worked so poorly in the past. If lawyers and judges hope to apply the new *Daubert* test rationally, they will have to learn what distinguishes science from other forms of knowledge — what it is that makes science scientific.

See Black et al., *supra* note 2, at 751. Unlike Black et al., this Article contends that it is in principle impossible to "apply the new *Daubert* test rationally."

by *Frye*-type deference to scientists. Second, proposed criteria of the scientific method may require or allow judges to rely on their own substantive scientific judgments and/or personal biases in order to resolve qualified experts' disagreements about testimony. *Daubert* rightly eschews both as a basis for admissibility decisions.[32]

Alternatively, if the proposed criteria prevent judges from either deferring to scientists or relying on their own substantive scientific judgments or personal biases, irrational classifications of proferred scientific evidence will result. Strictly applied, the proposed criteria may fail to be satisfied by classic examples of fine scientific work (e.g., the theory of relativity). Since the criteria will therefore imply that testimony pertaining to this work is pseudo-scientific and hence inadmissible, determination of which proferred evidence is scientific will be under-inclusive. The only criteria of the scientific method that can enable judges to adhere strictly to an unbiased, extra-scientific standpoint, yet avoid under-inclusive classifications, are toothless criteria that can be satisfied by even wildly unscientific work (e.g., reading tea leaves) and that consequently classify such work as an admissible subject of scientific expert testimony. Thus, judges must either depart from an unbiased, extra-scientific standpoint or adhere to such a standpoint at the cost of making irrationally under- or over-inclusive determinations of which evidence is scientific.

Closer inspection of *Daubert*'s four factors shows how this dilemma vitiates *Daubert*'s — and any other — attempt to delineate criteria of the scientific method that will enable judges to stand outside of science and yet rationally determine whether science is being done. One of the four *Daubert* factors is *Frye*'s "general acceptance" test.[33] A second *Daubert* factor — "whether there has been peer review and publication" — also

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32. *See supra* text accompanying notes 25 & 26. If judges based admissibility rulings on their extra-scientific biases, there would be no reason to expect the admitted, but not the excluded, testimony to be genuinely scientific. Moreover, it would be justifiable to allow judges to rely on their own substantive scientific judgments only if, as a rule, judges were at least as able as qualified experts to make substantive scientific judgments.

33. *See Daubert*, 509 U.S. at 594. *See also* United States v. Bonds, 12 F.3d 540, 553 n.10, 561 & n.18 (6th Cir. 1993) (stating that the effect of *Daubert* is that "general acceptance is no longer the test for admissibility of scientific evidence but now is only one factor to consider"); *Daubert II*, 43 F.3d at 1319 n.11; Giannelli, *supra* note 11, at 2002; Scheck, *supra* note 11, at 1960; Commonwealth v. Lanigan, 641 N.E.2d 1342, 1349 (Mass. 1994) (accepting *Daubert*'s alternative to *Frye* but voicing the suspicion that "general acceptance in the relevant scientific community will continue to be the significant, and often the only, issue").

While this Article contends that *Daubert*'s attempt to develop an alternative to *Frye*'s dictate of judicial deference is philosophically misguided, Larvie views the continued role for *Frye* as the "skeleton in the closet" of *Daubert*'s proposed alternative. Larvie, *supra* note 11, at 287.

measures whether work has been accepted by scientists.[34] Since neither of these factors are proper Archimedean criteria, the soundness of *Daubert*'s alternative to *Frye* rests on its third and fourth factors alone.

The third factor is "the known or potential rate of error" of a technique about which expert testimony is proposed.[35] In advising judges to consider this factor, *Daubert* says nothing about pervasive problems raised by techniques such as forensic DNA analysis and voice printing. Scientists may radically disagree about the potential accuracy of a technique,[36] the accuracy of particular laboratories' application of the technique,[37] and/or how to measure laboratory error rates.[38] When these controversies exist, a judge must resolve them in order to determine "the known or potential rate of error of a technique" and apply this factor to assess the technique's conformity to the scientific method.[39] If, however, a judge measures the error rate by adopting the assessment of

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34. *Daubert*, 509 U.S. at 593. *See also Daubert II*, 43 F.3d at 1318 ("That the research is accepted for publication in a reputable scientific journal after being subjected to the usual rigors of peer review is a significant indication that it is taken seriously by other scientists."); Allen, *supra* note 9, at 1169.

35. *Daubert*, 509 U.S. at 594. Some commentators see the *Daubert* Court's call for inquiry into error rates as a call to consider the known or potential rate of error and the existence of established standards for applying the technique. Accordingly, they see *Daubert* as delineating five, rather than four, factors. *See, e.g.*, Scheck, *supra* note 11, at 1964; Giannelli, *supra* note 11, at 2022. This Article's argument is unaffected by whether *Daubert* is seen as proposing four or five factors.

36. To dispute the F.B.I.'s contention that erroneous determinations of whether forensic DNA samples match are impossible, Scheck advances a detailed argument in support of "the NRC [National Research Council]'s common sense observation that '[l]aboratory errors happen, even in the best laboratories and even when the analyst is certain that every precaution against error was taken.'" Scheck, *supra* note 11, at 1983 & 1983 n.87 (citation omitted).

37. For a detailed discussion of data showing that the F.B.I. and other forensic DNA laboratories in fact had significant error rates, see Scheck, *supra* note 11, at 1982 ("forensic DNA laboratories [falsely] maintained for years that the technology was so powerful and foolproof that erroneous results were impossible"). *See also* People v. Wesley, 633 N.E.2d 451, 465-66 (N.Y. 1994) (Kaye, C.J., concurring); United States v. Yee, 134 F.R.D. 161, 171-72 (N.D. Ohio 1991).

38. Scheck concludes: "In the final analysis, the problem for courts applying *Daubert* will be deciding . . . what kind of proficiency testing . . . is necessary to compute a reasonably reliable [forensic DNA] laboratory error rate." Scheck, *supra* note 11, at 1984 (footnote omitted). *See also* Giannelli, *supra* note 11, at 2022-25 (discussing the controversies over what the error rates were in the use of voiceprint technology to make identifications, and how to measure those error rates). Giannelli argues that controversies about voiceprint evidence cast doubt on judicial competence to apply *Daubert*'s third factor. *Id.*

39. Radical questions about the possibility of using Daubert's third factor to decide on the scientific status of a theory or technique are raised by the National Research Council's recent conclusion that "it is unfeasible to estimate the likelihood of [forensic DNA] laboratory error." *DNA Evidence: When Properly Collected and Analyzed, Should Not Be Called into Question*, National Research Council News, May 2, 1996.

some segment of the scientific community or resorting to personal substantive scientific views and/or biases, the third *Daubert* factor is not Archimedean. It follows that *Daubert*'s third factor can aid judges to be principled, independent gatekeepers of the scientific method only if some other criterion can enable judges to choose among competing scientists' claims about the error rates of a technique.[40] *Daubert*'s only suggestion for such an Archimedean criterion comes from its fourth factor: "whether a theory or technique . . . can be (and has been) tested."[41]

Daubert's statement that testability is a factor in evaluating the scientific status of a theory or technique[42] is followed by a quotation from Sir Karl Popper: "The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability."[43] As interpreted by lower courts post-*Daubert*, the criterion of falsifiability has not enabled judges to escape from the dilemma of either departing from an unbiased, extra-scientific standpoint or adhering to such a standpoint but making irrationally under- or over-inclusive determinations of admissibility of scientific expert testimony.

Some lower courts have defined falsifiable theories or techniques as those that can be disputed.[44] The undesirable consequence of this

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40. An additional problem with *Daubert*'s third factor is that even when the error rate of a technique is known, there may be disagreements about what an acceptable error rate is. *See, e.g., Daubert II*, 43 F.3d at 1316 n.3 ("These [*Daubert*] factors raise many questions, such as how do we determine whether the rate of error is acceptable, and by what standard?"); State v. Foret, 628 So.2d 1116, 1126 (La. 1993) (stating that although a study found that a 32% error rate made certain clinical criteria "valid predictors of whether children have been sexually abused . . . we are not so comfortable");United States v. Crumby, 895 F. Supp. 1354, 1359-60 (D. Ariz. 1995) ("The known error rates for the science of polygraphy are remarkably low," but "[t]he prejudice which might result from one polygraph test which wrongly suggests that an innocent person committed [a] crime is perhaps intolerable."); *New Challenges of Scientific Evidence, supra* note 9, at 1556.

As the above quotations suggest, in a legal context, there is no strictly scientific resolution of the issue of what error rate is acceptable. Rather, courts need to make normative determinations of the positive and negative error rates that can and should be tolerated, given the values served by particular uses of a technique (e.g., what rates of false positives and false negatives in establishing forensic DNA matches are respectively tolerable when that technology is used for purposes of convicting and exonerating criminal suspects?, also, to what extent are different error rates tolerable for purposes of establishing liability in paternity proceedings and for purposes of establishing criminal liability?). It follows that even if it exists, a scientifically uncontroversial assessment of the error rate of a technique cannot by itself enable judges to determine whether evidence is admissible.

41. *Daubert*, 509 U.S. at 593.

42. *See id.*

43. *Id.* at 593 (citation omitted).

44. *Bonds* held that because the defense experts questioned both whether the F.B.I. obtained accurate results through forensic DNA analysis and whether the F.B.I.'s methods for testing its forensic DNA procedures were valid, "it seems clear that this first *Daubert* factor [of falsifiability] is not really in dispute." United States v. Bonds, 12 F.3d 540, 559 (6th Cir. 1993). The *Bonds* court reasoned:

definition, not recognized by these courts, is that all purported scientific theories or techniques satisfy this formulation of falsifiability.

Other courts' formulations are so vague as to imply that the falsifiability criterion is satisfied either: so long as a theory or technique has been used or tested in any study, or only if the theory or technique has been applied or tested by established scientists.[45] By implying that clearly unscientific but widely used techniques, such as prediction through tea leaves, are falsifiable, the first position makes the falsifiability criterion an overly-inclusive criterion of scientific status. In contrast, the second stance may avoid overbreadth, but only because by making *Frye*-type deference to scientists a definitional component of falsifiability, it makes falsifiability a non-Archimedean standpoint of scientific status.[46]

The irony is that Sir Karl Popper developed the criterion of falsifiability in order to distinguish between science and pseudo-science without appealing to the views of those claiming to do science. For Popper, the task of defining and applying a criterion of scientific status was not a task for scientists, but rather a task for the preeminent rational

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> Defendants vociferously dispute the accuracy of the match results and the adequacy of the testing done, and in refutation have presented evidence about deficiencies in both the results and the testing of the results. Thus it appears that by attempting to refute the FBI's theory and methods . . . the defendants have conceded that the theory and methods can be tested.

Id. at 559 (citations omitted). *See also* State v. Anderson, 881 P.2d 29, 42 (N.M. 1994). This reasoning implies that any attempt to contest a theory or technique renders it falsifiable. *See* Scheck, *supra* note 11, at 1994 (arguing that the *Bonds* court's "reasoning is plainly superficial and circular").

45. *See* Government of Virgin Islands v. Penn, 838 F. Supp. 1054, 1065 (D.V.I. 1993) (suggesting either that the mere fact of "widespread repetition and use" shows that the F.B.I.'s forensic DNA procedures are falsifiable, or that the falsifiability criterion is satisfied because "processes similar to the FBI's DNA profiling process are widely used . . . by the medical community as well as numerous laboratories"). *See also* Arnold v. Riddell, Inc., 882 F. Supp. 979, 990 (D. Kan. 1995) (stating without any description or analysis of the tests conducted that the testability criterion was satisfied because "Dr. Stalnaker testified to the testing he conducted and how the testing supports his theories"); *Crumby*, 895 F. Supp. at 1358-59 (reasoning that the falsifiability criterion was satisfied because a witness, whom the court characterized as "an eminently qualified expert in the field of polygraphy . . . testified that polygraphy has been tested by [the] scientific method").

46. *See* Paul C. Giannelli, *The Admissibility of Novel Scientific Evidence*, 80 COLUM. L. REV. 1197, 1213 (1980) (discussing controversies over what constitutes empirical validation of various forensic techniques).

Scheck mistakenly assumes that some, but not other, scientists' criteria for appropriate testing procedures can be relied on to determine whether forensic DNA profiling satisfies *Daubert*'s testability factor. *See* Scheck, *supra* note 11, at 1968-80, 1984-85. Therefore, Scheck fails to realize that the aim of *Daubert*'s factors is to enable judges to decide rationally whether the scientific method was used without first adopting the positions of some competing scientists, but not those of others.

outsiders: epistemologists or philosophers of science.[47] Thus, courts have clearly misunderstood Popper when they have made deference to scientists a definitional component of falsifiability. In addition, courts have clearly failed to realize Popper's goal of rationally distinguishing between science and pseudo-science, because their interpretations have allowed even wildly unscientific work to satisfy the criterion of falsifiability.[48]

However, even the most perfect understanding of falsifiability would not enable courts to make rational, unbiased admissibility decisions. The history and philosophy of science reveal the intellectual bankruptcy of Popper's — and *Daubert*'s — quest for an Archimedean standpoint from which to judge whether science is being done.

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47. *See, e.g.*, KARL R. POPPER, THE LOGIC OF SCIENTIFIC DISCOVERY 19 (1968) [hereinafter LOGIC] (claiming that "the theory of knowledge was inspired by the hope that it would enable us . . . to contribute to the advance . . . of scientific knowledge" and criticizing linguistic philosophers for "defin[ing] the word 'philosophy' in a way that may well prevent a student of philosophy from trying to contribute, qua philosopher, to the advancement of our knowledge of the world" (citation omitted)); *id.* at 38-39 ("I . . . take it to be the first task of the logic of knowledge to put forward a concept of empirical science . . . in order to draw a clear line of demarcation between science and metaphysical ideas."). *See also* Sandra G. Harding, *Introduction*, *in* CAN THEORIES BE REFUTED? xiii (S.G. Harding ed., 1976). Thus, Popper's quest, like *Daubert*'s, was for a conception of the scientific method that people standing outside of science could apply to determine whether purported scientists are in fact doing science.

A failure to appreciate that Popperian falsifiability is a version of the quest for an Archimedean standpoint underlies Farrell's misguided conclusion that *Daubert* inconsistently combines Popper's "positivism" and the "constructivist" view that she identifies with Kuhn. See Farrell, *supra* note 13. Moreover, *Bonds* failed to appreciate that the point of *Daubert*'s appeal to falsifiability is to avoid the deference to scientists implicit in the *Frye* general acceptance test. *See* United States v. Bonds, 12 F.3d 540, 558 (6th Cir. 1993).

The *Daubert II* court saw that *Daubert*'s viability depended on the possibility of delineating a scientifically uncontroversial testability criterion, but did not realize that Popperian falsifiability was intended as such a criterion. *See Daubert II*, 43 F.3d at 1316 n.3. *But see New Challenges of Scientific Evidence*, *supra* note 9, at 1515 (noting that "[e]ven if a particular theory can be tested, the assessment of its validity may differ according to who is doing the testing," but failing to see that this raises doubts about whether "testability or falsifiability [is] a theoretically appealing criterion").

48. Chief Justice Rehnquist, dissenting from the *Daubert* majority's delineation of an alternative to the *Frye* approach to admissibility, correctly anticipated that some judges would be unable to understand Popper's work or apply it to make viable admissibility decisions: "I defer to no one in my confidence in federal judges; but I am at a loss to know what is meant when it is said that the scientific status of a theory depends on its 'falsifiability,' and I suspect some of them will be, too." *Daubert*, 509 U.S. at 600 (Rehnquist, C.J., dissenting) (citation omitted). *But see* Poulter, *supra* note 11, at 1319 n.72 (suggesting that Chief Justice Rehnquist failed to realize that the concept of falsifiability is easy to apply).

III. A PHILOSOPHICAL CRITIQUE OF FALSIFIABILITY

A. Popper's Project

Marxist and Freudian theories and the psychological theories of Alfred Adler were the immediate targets of Popper's philosophy of science.[49] After World War I, many of Popper's fellow Viennese intellectuals claimed that these theories were genuine sciences. A disillusioned Popper concluded that each was more like a religion held by true believers and contrasted these pseudo-sciences with what he took to be the exemplary science: physics — in particular, the theory of relativity. Seeking to justify this position, Popper argued that the primary task of epistemology is to solve what he termed "the problem of demarcation."[50] In other words, the task of the theory of knowledge is to answer: "'When should a theory be ranked as scientific?' or 'Is there a criterion for the scientific character or status of a theory?'"[51] Thus, one of Popper's primary philosophical concerns was "to define the concepts 'empirical science' and 'metaphysics' in such a way that we shall be able to say of a given system of statements whether or not its closer study is the concern of empirical science."[52]

The criterion of falsifiability was Popper's solution to the problem of demarcation. In contrast to other philosophers, Popper believed that science cannot be distinguished from metaphysics or pseudo-science by its confirmation by experience or observations. Rather, Popper claimed that the hallmark of a metaphysical theory — such as Marxism, psychoanalysis, or Adlerian psychology — is that it leaves its adherents free to transmute any seemingly refuting observation into a confirming instance.[53] Popper argued, "not the verifiability but the falsifiability of a system is to be taken as a criterion of demarcation . . . it must be possible for an empirical scientific system to be refuted by experience."[54] For Popper, "falsifiability" was synonymous with "refutability" or "testability." Thus, Popperian falsifiability is the view that:

> [A] system is to be considered as scientific only if it makes assertions which may clash with observations;

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49. *See* KARL R. POPPER, *Science: Conjectures and Refutations*, *in* CONJECTURES AND REFUTATIONS 33-37 (1965) [hereinafter CONJECTURES] (Popper's autobiographical account of the genesis of his falsifiability criterion).

50. *Id.* at 33.

51. *Id.* (emphasis omitted).

52. LOGIC, *supra* note 47, at 37.

53. CONJECTURES, *supra* note 49, at 34-35.

54. LOGIC, *supra* note 47, at 40-41 (emphasis omitted).

> and a system is, in fact, tested by attempts to produce such clashes, that is to say by attempts to refute it. Thus testability is the same as refutability, and can therefore likewise be taken as a criterion of demarcation.[55]

Two basic interpretations of the intent of the criterion of falsifiability are possible. By one interpretation, the criterion establishes a logical distinction between groups of sentences.[56] Popper's project, by this interpretation, was to use mathematical logic to formulate a precise definition of falsifiability that classifies groups of sentences as science or pseudo-science. As shown in Part III.B, the logical version of Popperian falsifiability met with a formidable barrage of philosophical criticism.

In response, Popper insisted that the falsifiability criterion was not intended to establish a logical distinction between groups of sentences, but rather a methodological distinction between empirical science and pseudo-science. Popper contended that by distinguishing between those using and eschewing the falsificationist method of science, a rational outsider can distinguish scientists from practitioners of pseudo-science.[57]

Parts III.B-D delineate widespread and cogent criticisms by philosophers and historians of science showing that both the logical and methodological versions of falsifiability are fundamentally untenable. These criticisms show, first, that Popper failed to construct a rational and scientifically uncontroversial standpoint for distinguishing science from pseudo-science. Second, any attempt to delineate such a standpoint fails.[58]

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55. KARL R. POPPER, *The Demarcation Between Science and Metaphysics*, *in* CONJECTURES, *supra* note 49, at 256, *cited in Daubert*, 509 U.S. at 593.

56. *See, e.g.*, LOGIC, *supra* note 47, at 37 (stating that the aim of the falsifiability criterion is to "be able to say of a given system of statements whether or not its closer study is the concern of empirical science"). *See also* CARL G. HEMPEL, *Postscript (1964) on Cognitive Significance*, *in* ASPECTS OF SCIENTIFIC EXPLANATION AND OTHER ESSAYS IN THE PHILOSOPHY OF SCIENCE 121 (1970) [hereinafter ASPECTS].

57. *See* Part III.C. Some of Popper's statements suggest that the falsifiability criterion is intended to establish both a methodological distinction between science and pseudo-science, and a distinction in logical form between scientific and pseudo-scientific sentences. *See, e.g.*, LOGIC, *supra* note 47, at 39 (stating that "empirical science seems to be characterized not only by its logical form but, in addition, by its distinctive method" (emphasis omitted)).

58. For a similar argumentative strategy, see Adina Schwartz, *Against Universality*, 78 J. PHIL. 127-36 (1981) (exhibiting the failure of various attempts to construct normatively uncontroversial standpoints for rationally resolving controversial social and political questions and arguing, as a consequence, that any attempt to delineate an Archimedean standpoint for resolving social and political disputes will necessarily fail); *see also* Adina Schwartz, *Towards a Jurisprudence of Labor Law: Methodological Preliminaries*, 19

Before presenting this critique, it is important to note that Popper and his intellectual allies and critics were all concerned with testability in principle. Science was supposed to be distinguished from pseudo-science by whether it was testable in theory, not by whether testing was technically possible or practically feasible, much less by whether actual testing had been done.[59] In contrast, *Daubert*'s discussion of whether a theory or technique "can be (and has been) tested"[60] is consistent both with a testability criterion and with the notion that actual testing must have occurred in order for a theory or technique to count as scientific. *Daubert*'s ambiguity notwithstanding, a critique of testability suffices to establish the philosophical untenableness of *Daubert*'s alternative to *Frye*. By establishing that some of the world's finest scientific work cannot satisfy any reasonably rigorous testability requirement, this critique implies that at least as much peerless scientific work cannot satisfy any reasonably rigorous requirement of actual testing.[61]

The arguments in Parts III.B-D against the possibility of an adequate, clean distinction between science and pseudo-science[62] might seem inapplicable to *Daubert* on the ground that judges do not need a sharp line; they can rely on their own judgments in borderline cases. However, the absence of a clean distinction is critical precisely because judges need only apply the *Daubert* standard in borderline cases. The issue of whether the subject matter of expert testimony is genuinely scientific is contested only if "the individuals whose testimony is being proferred [are first established as] experts in a particular [relevant] scientific field."[63] In addition, other experts in the field must be willing to question whether the proffered expert testimony pertains to genuine science. If judges are to apply *Daubert*'s testability factor to acceptably resolve admissibility disputes from an unbiased, extra-scientific standpoint, this factor cannot leave them to their own judgment or the judgment of others in borderline cases. For Daubert's purposes, a testability criterion must enable judges to draw a clear line between science and pseudo-science.

VAL. U. L. REV. 71, 81-94 (1984).

59. *See, e.g.*, ASPECTS, *supra* note 56, at 104 n.3; CARL G. HEMPEL, PHILOSOPHY OF NATURAL SCIENCE 22 (1966) [hereinafter NATURAL SCIENCE].

60. *Daubert*, 509 U.S. at 593.

61. Many have adopted the view that the *Daubert* factor is "whether the scientific theory or technique has been tested" and the mistaken assumption that this is an adequate criterion of scientific status. *See, e.g.*, Jonakait, *supra* note 11, at 2107 & n.24 ("[t]he defining touchstone of science is a testable proposition that is tested"); Black et al., *supra* note 2, at 750-51 (The *Daubert* court "recognized that a theory or technique constitutes valid scientific knowledge only if it is testable and has in fact been tested."); Giannelli, *supra* note 11, at 2002.

62. Popper sought a criterion that would "draw a clear line" between science and pseudo-science. *See* CONJECTURES, *supra* note 49, at 33.

63. *Daubert II*, 43 F.3d at 1315.

B. Testability Is an Inadequate Logical Criterion

1. Hempel's Critique

One of the most formidable criticisms of Popperian falsifiability was advanced by the philosopher Carl G. Hempel in the early 1950s. Justice Blackmun's opinion for the *Daubert* majority misleadingly cites Hempel's statement that "[t]he statements constituting a scientific explanation must be capable of empirical test."[64] But Hempel argued rigorously that despite its intuitive plausibility, the idea of testability can never be developed into an adequate criterion for distinguishing between science and pseudo-science.[65] Hempel specifically directed this argument against both Popper and his Viennese contemporaries, the logical positivists or Vienna Circle.[66]

Hempel argued that it is in principle impossible to achieve either the logical positivist goal of cleanly distinguishing between empirically meaningful and meaningless sentences or the Popperian goal of clearly demarcating science from metaphysics. To logical positivists, testability

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64. NATURAL SCIENCE, *supra* note 59, at 49, *cited in Daubert*, 509 U.S. at 593.

65. Hempel's arguments are summarized in the textbook from which Blackmun culled his misleading quotation. *See* NATURAL SCIENCE, *supra* note 59, at 22-32. *See also* CARL G. HEMPEL, *Empiricist Criteria of Cognitive Significance: Problems and Changes*, *in* ASPECTS, *supra* note 56, at 101-22 (presenting Hempel's fuller, technical argument against a testability criterion). Blackmun's mistaken interpretation may have been caused by Hempel's own ambivalence about the notion of testability. Although he concluded that "it is not possible to draw a sharp dividing line between hypotheses and theories that are testable in principle and those that are not," Hempel nonetheless insisted that "even though it is somewhat vague, the [testability] distinction . . . is important and illuminating." NATURAL SCIENCE, *supra* note 59, at 32. *See also* ASPECTS, *supra* note 56, at 102.

Commentators have repeated Blackmun's mistake of seeing Hempel as an ally of Popper, rather than as one of his most formidable philosophical opponents. *See, e.g.*, Giannelli, *supra* note 11, at 2002 & n.23; Farrell, *supra* note 13, at 2190, 2199, 2205; Black et al., *supra* note 2, at 754 n.261.

66. Popper repeatedly and strenuously insisted on the differences between his views and those of the logical positivists. *See, e.g.*, LOGIC, *supra* note 47, at 34-37, 40-41. *See also* Thomas Kuhn, *Logic of Discovery or Psychology of Research?*, *in* 2 THE PHILOSOPHY OF KARL POPPER 798-99 (P.A. Schilpp ed., 1974) (agreeing with some of Popper's claims about his differences with the logical positivists). The argument in this Article does not depend on an assessment of the kinship (or lack thereof) between logical positivism and Popper's views.

Michael Friedman has recently challenged the widespread view that "[t]he positivists . . . were concerned . . . to provide a philosophical justification of scientific knowledge from some privileged, Archimedean vantage point situated somehow outside of, above, or beyond the actual (historical) sciences." Michael Friedman, *The Re-Evaluation of Logical Positivism*, 88 J. PHIL. 505, 506 (1991). Even if correct, Friedman's revisionist interpretation of logical positivism does nothing to undermine this Article's critique of *Daubert*, because *Daubert* does presuppose the existence of some Archimedean standpoint for determining whether science is being done.

means either confirmation or falsification, whereby empirically meaningful sentences can be distinguished from meaningless, metaphysical sentences.[67] Hempel argued, in contrast, that neither confirmation nor falsification by experience can be developed to yield the requisite sharp divide between sentences.[68]

To reach this conclusion, Hempel first advanced an analysis of what it means for a sentence to be testable in principle, whether by satisfying a criterion of verifiability or one of falsifiability. According to Hempel, the verifiability criterion defines a sentence S as empirically meaningful or scientific if and only if it is capable in principle of being completely proven. Similarly, Hempel stated that the falsifiability criterion defines a sentence S as empirically meaningful or scientific if and only if it is capable in principle of being completely[69] disproven.[70] Hempel argued

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67. This is only part of the logical positivists' distinction between meaningful and meaningless sentences. The full logical positivist position is that "a sentence makes a cognitively significant assertion . . . if and only if either (i) it is analytic or contradictory — in which case it is said to have purely logical meaning or significance — or else (ii) it is capable, at least potentially, of test by experiential evidence — in which case it is said to have empirical meaning or significance." ASPECTS, *supra* note 56, at 101. Similarly, the full demarcation Popper sought was not simply between science and metaphysics, but rather "between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other." LOGIC, *supra* note 47, at 34 (citation omitted).

68. *See* ASPECTS, *supra* note 56, at 102. Willard Van Orman Quine similarly argued against the possibility of sharply distinguishing between either (i) empirical and metaphysical statements or (ii) empirical statements and statements that are true or false by virtue of logic alone. *See* WILLARD V.O. QUINE, *Two Dogmas of Empiricism*, *in* FROM A LOGICAL POINT OF VIEW 43 (1961). However, while Hempel focused on the problems with the first distinction, Quine focused on the problems with the second distinction. Because the main, philosophical premise underlying *Daubert*'s proposed alternative to *Frye* is that the first distinction can be sharply drawn, this Article is primarily concerned with that distinction.

69. The quest for a sharp distinction makes it necessary to define the demarcation criteria in terms of complete verifiability and complete falsifiability. On the one hand, a sharp distinction cannot possibly be drawn if scientific or empirically meaningful sentences are required to be falsifiable or verifiable to an imprecisely defined extent. On the other hand, if demarcation criteria are instead defined to require some precise degree, short of complete verifiability or falsifiability, there appears to be no principled basis for preferring those criteria to criteria requiring some slightly higher or lower degree of verifiability or falsifiability. *See supra* text accompanying note 62 for a discussion of why the viability of *Daubert*'s alternative to *Frye* rests on the possibility of drawing a bright line distinction between science and pseudo-science.

70. Sentence S can be completely verified if and only if it is possible to specify a finite set of observation sentences — sentences reporting direct observations — such that the truth of sentence S follows logically from the truth of the observation sentences. Sentence S can be completely falsified if and only if it is possible to specify a finite set of observation sentences such that the falsehood of sentence S (or, equivalently, the truth of the negation of sentence S) follows logically from the truth of the observation sentences. *See* ASPECTS, *supra* note 56, at 102-04, 106. If a logical contradiction is assumed to be true, it follows logically that any and all sentences are true. Accordingly, in order to ensure that only some sentences satisfy the criteria of verifiability and falsifiability, Hempel qualifies the above

that both criteria are vitiated by under-inclusiveness; if either criterion is consistently applied, some of the most important sentences of empirical science count as empirically meaningless or metaphysical.

No universal statement or general law (e.g., "all storks are red-legged"[71]) can ever be completely verified or logically entailed by any finite set of observation sentences. Further observations of non-conforming events always remain logically possible, no matter how many conforming observations are made of what is claimed to be a general law (e.g., no matter how many times we observe the sun rising, it remains possible that the sun may not rise tomorrow). The verifiability criterion thus excludes all general laws from the class of empirically meaningful or scientific statements, even though general laws "constitute an integral part of scientific theories."[72]

Hempel recognized that this criticism of the verifiability criterion does not apply to Popper's solution to the problem of demarcation.[73] Indeed, one of Popper's arguments for making falsifiability the criterion of scientific status is that a falsifiability criterion does not relegate all general laws to the category of metaphysics.[74] A sentence is falsifiable if some finite set of observation sentences can logically entail its negation. For example, the negation of a general law ("all storks are red-

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definitions by requiring that the sets of observation sentences be logically consistent. *See id.* at 104, 106. Hempel further qualifies the definitions to take account of the fact that an analytic sentence (or, roughly, a sentence true by virtue of logic or meaning alone, such as "2+2=4" or "all bachelors are unmarried males") is always true and thus follows logically from the truth of any set of observation sentences. To ensure that the truth of a testable sentence depends on the truth of some set of observation sentences, Hempel further requires a completely verifiable sentence to be non-analytic. *See id.* at 104. He correspondingly requires that the negation of a completely falsifiable sentence be non-analytic. *See id.* at 106; *see also supra* note 68.

The need for these qualifications shows that the argument against the possibility of sharply distinguishing between empirical and metaphysical statements is intimately connected to the argument against the possibility of sharply distinguishing between empirical statements and statements true or false by virtue of logic alone. The notions of logical contradiction and analyticity can be clearly defined only if the second distinction can be sharply drawn. Since these notions are in turn needed to define a testability criterion, the arguments against a sharp second distinction also oppose a sharp first distinction.

71. As Hempel indicates, this sentence is capable of satisfying the criterion of verifiability only if it is not analytic or, in other words, only if all storks are not red-legged by definition. *See* ASPECTS, *supra* note 56, at 104-05.

72. *Id.* at 105.

73. *See id.* at 105-06.

74. *See, e.g.*, LOGIC, *supra* note 47, at 40. See *supra* text accompanying notes 53 & 54 for Popper's argument for falsifiability, over verifiability, on the ground that a metaphysical system allows every observation sentence to count as a confirmation. *See also* Allen, *supra* note 9, at 1169-71 (failing to see that Popper advanced this second argument); Harding, *supra* note 47, at xiii-xiv (discussing the relation between Popper's two arguments for a falsifiability criterion).

legged") is a singular or existential statement ("there is at least one stork that is not red-legged"). The truth of this singular statement can follow logically from the truth of an observation sentence ("here is a yellow-legged stork"). Hence, a sentence of general or universal logical form can satisfy the falsifiability criterion, but not the verifiability criterion.[75]

According to Hempel, however, the falsifiability criterion has a problem similar to those of the verifiability criterion. The negation of any singular or existential statement (e.g., "there is at least one unicorn") is a universal statement ("there are no unicorns"). No matter how many conforming observations are made of this universal statement, non-conforming events remain logically possible. Hence, no singular statement can be completely falsified. The falsifiability criterion is under-inclusive because it excludes all singular statements from the class of empirically meaningful or scientific statements.[76]

Thus, according to Hempel, it is in principle impossible to use either a verifiability or a falsifiability criterion to achieve the rational and clean demarcations that Popper and the logical positivists sought between the sentences of science and pseudo-science.

2. Facts Are Theory-Laden

Even if some testability criterion could escape Hempel's problem of under-inclusiveness, it would be afflicted by a further, fundamental problem. Regardless of whether testability is seen as a matter of

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75. *See* LOGIC, *supra* note 47, at 41.

76. *See* ASPECTS, *supra* note 56, at 106. Black et al. correctly recognized that universal statements are excluded from scientific knowledge by a verifiability criterion *See* Black et al., *supra* note 2, at 755-56. But they did not recognize the opposing problem of the exclusion of singular statements by a falsifiability criterion. For a criticism of Popper's attempt to establish the insignificance of this problem with falsifiability, see *infra* Part III.C.1.

Hempel argued that the criterion of falsifiability is also overly inclusive if it is taken as a solution to Popper's problem of demarcation. *See* ASPECTS, *supra* note 56, at 106. Suppose that the negation of sentence S is entailed by a finite set of observation sentences and that S (e.g., "all storks are red-legged") is, accordingly, falsifiable. According to standard logic, the conjunction of any set of sentences is false if and only if at least one of the sentences forming the conjunction is false. Hence, the observation sentences will also entail the negation, and consequent falsifiability, of the conjunction of S and any other sentence N (i.e., "S and N"), even if N (e.g., "the absolute is perfect") clearly does not belong to empirical science. Since it thus serves to include even obviously metaphysical sentences (e.g., "all storks are red-legged and the absolute is perfect") in the class of scientific sentences, the criterion of falsifiability does not achieve Popper's goal of demarcating science from metaphysics. Hempel explained why an analogous over-inclusiveness does not result if the criterion of falsifiability is intended to achieve the logical positivist goal of distinguishing between meaningful and meaningless sentences. *See id.* at 120-21.

verifiability or falsifiability, testability presupposes a distinction between sentences that need to be tested and sentences that serve as the test. Hempel, along with many other philosophers and historians of science, argued that statements about the facts or observations or experience are themselves inevitably theory-laden. Thus, the sentences that are supposed to serve as the test are themselves in need of testing.[77]

The distinction between sentences that serve as the test and sentences that need to be tested is problematic because statements about observations or experience can confirm or disconfirm a theory or hypothesis only if the statements at least implicitly claim to report veridical observations or experiences. Confirming or disconfirming statements must be intended to convey facts about the observed or experienced event; they cannot simply be intended as reports of how a particular individual experienced or observed the event at a particular time.

In everyday life, people frequently make claims about the veridicality or accuracy of observations or experiences. The claims implicitly assume that veridicality is increased or decreased by some, but not other, characteristics of the observing individual or observational situation. As disputes about eyewitness testimony show, these assumptions can be controversial. For example, is a victim's ability to identify an assailant likely to be affected and, if so, in what direction, by the violence of the assault? To what extent, if at all, do racial differences between a witness and a suspect decrease the likelihood of accurate identification?[78] These examples undermine the notion of testability by showing that even the hypotheses or theories that people put forth in everyday life are not directly tested by observation sentences.

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77. *See, e.g.*, ASPECTS, *supra* note 56, at 107-118; NATURAL SCIENCE, *supra* note 59, at 22-28; Imre Lakatos, *Falsification and the Methodology of Scientific Research Programmes*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 205-59; QUINE, *supra* note 68, at 37-43; HILARY PUTNAM, *The 'Corroboration' of Theories*, *in* 1 MATHEMATICS, MATTER AND METHOD 255-58 (2d ed. 1979); KUHN, *supra* note 13, at 111-35, 192-98. The version presented here is most similar to that in Lakatos.

As part of the revisionist interpretation of logical positivism, Friedman argues that the logical positivists came "very close indeed to [developing] the supposedly antipositivist doctrine of the theory ladenness of observation." *See* Friedman, *supra* note 66, at 513-14, 519. Regardless of its truth, Friedman's interpretation of logical positivism has no bearing on this Article's critique of *Daubert*.

Black et al., *supra* note 2, fail to acknowledge that their endorsement of Popperian falsifiability is inconsistent with the discussion of the theory-ladenness of facts and other problems with a testability criterion in Bert Black, *A Unified Theory of Scientific Evidence*, 56 FORDHAM L. REV. 595, 618-21 (1988).

78. *See* People v. McDonald, 690 P.2d 709, 720-21 (Cal. 1984) (en banc) (psychologist's expert testimony on eyewitness identification was needed because of laypersons' tendency not to appreciate such problems as "the pitfalls of cross-racial identification").

Instead, there is a tripartite relationship between any common sense hypothesis or theory proposed for testing (e.g., suspect Z was the assailant), the observation sentence that is supposed to confirm or disconfirm it (e.g., witness X observed suspect Z or someone other than suspect Z performing the assault at time Y), and the common sense assumptions or theory underlying the observation sentence's implicit claim to report a veridical observation (e.g., witness X's observation was accurate because people of his race, personality type, etc. are likely to observe accurately when witnessing an assault at the distance, under the lighting conditions, etc. that prevailed). The existence of this tripartite relationship implies that no observation sentence can ever conclusively confirm or disconfirm a common sense hypothesis or theory. The observation sentence may itself be called into question by examining the assumptions or theories that justify its implicit claim to describe a veridical observation.[79]

Observation sentences are even more dramatically theory-laden in science than in common sense discourse. A correlate of scientific progress is the development and use of instruments to increase the accuracy and range of observation and to manipulate the materials and events observed. Concomitantly, theories have been developed to interpret observations made with scientific instruments. These theories must specify what various instruments can measure, with what degrees of accuracy, and the conditions for proper use of the instruments. The interpretative theories must also explain why some measurements can be made, while others cannot. According to philosopher and historian of science Imre Lakatos, "the problem is not what to do when 'theories' clash with 'facts'. . . . [T]he clash is not 'between theories and facts' but between . . . an interpretative theory to provide the facts and an explanatory theory to explain them."[80]

Scientists sometimes maintain interpretative theories by rejecting the explanatory theories that clash with observations whose accuracy follows from the interpretative theories. The opposite also occurs. For example, the Aristotelian (explanatory) theory that celestial bodies are faultless crystal balls was rejected in light of Galileo's observations of mountains on the moon and spots on the sun. Galileo's observations were accurate, however, only if both his telescope and the optical theory on which it was based were reliable. Both the interpretative optical theory and Galileo's derivative claims about the telescope's reliability were hotly

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79. In a similar argument, Imre Lakatos concluded that "there are and can be no sensations unimpregnated by expectations and therefore there is no natural (i.e. psychological) demarcation between observational and theoretical propositions." Lakatos, *supra* note 77, at 210-12.

80. *Id.* at 238 (emphasis omitted).

contested by scientists at that time.[81] However, explanatory "theory" triumphed over "facts" in the conflict between Newton's lunar theory and the observations of Royal Astronomer Flamsteed. Newton made no observations of his own, but instead repeatedly persuaded Flamsteed to reinterpret his data — and, concomitantly, revise his interpretative theories — to remove the conflict with Newton's explanatory theory.[82]

The tripartite relationship among scientific observation sentences and explanatory and interpretative theories implies that in science, as in common sense discourse, which sentences need to be tested and which provide tests themselves is open to dispute. Whenever an observation sentence conflicts with an explanatory theory, scientists face a choice of replacing the explanatory theory or replacing the interpretative theory that holds the observation sentence as veridical. Scientists may appeal to further theories or observations to justify a decision to replace either the explanatory or the interpretative theory. The tripartite relationship among observation sentences and explanatory and interpretative theories implies, however, that any "fact" or "theory" to which scientists appeal can itself be tested. Thus, there is no incorrigible foundation from which testing can proceed.[83]

Similarly, Hempel concluded that "even the most careful and extensive test can neither disprove one of two hypotheses nor prove the other; thus strictly construed, a crucial experiment is impossible in science."[84] In his classic essay, *Two Dogmas of Empiricism*, Willard Van Orman Quine reached the kindred conclusion that "it is misleading

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81. *See id.* at 211.

82. Lakatos's account of the relationship between Newton and Flamsteed shows both that great scientists can — and do — revise facts in light of explanatory theories and that scientific and interpersonal conflicts may be intertwined. *See* Lakatos, *supra* note 77, at 254 n.129.

83. *See* Lakatos, *supra* note 77, at 239-40. Similarly, Kuhn concluded that: "All experiments can be challenged, either as to their relevance or their accuracy. All theories can be modified by a variety of *ad hoc* adjustments without ceasing to be, in their main lines, the same theories . . . it is often by challenging observations or adjusting theories that scientific knowledge grows." Kuhn, *supra* note 66, at 808.

84. NATURAL SCIENCE, *supra* note 59, at 28. Hempel based this conclusion on a more general argument than that outlined above. Hempel's argument is that observation sentences are not logical consequences of single explanatory theories, but only of explanatory theories supplemented by "auxiliary hypotheses." *Id.* at 22-28. The auxiliary hypotheses include both interpretative theories and, possibly, simplifying assumptions and further explanatory theories. The testing relationship is thus not between an observation sentence and a single explanatory theory, but rather between an observation sentence and a network of interpretative and explanatory theories. Accordingly, Hempel concluded that a conflict never disproves a single theory, but rather leaves scientists with a choice of which theory to replace. Philosopher Hilary Putnam also concluded that "falsification in science is no more conclusive than verification." *See* PUTNAM, *supra* note 77, at 255-58.

to speak of the empirical content of an individual statement . . . . Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system."[85] It follows that it is impossible to formulate a testability criterion that achieves either the Popperian or logical positivist goal of sharply distinguishing between scientific or empirically meaningful sentences and pseudo-scientific or empirically meaningless sentences. If scientific or empirically meaningful sentences are identified with those that can be definitively tested, no sentences fall into that class.[86]

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85. QUINE, *supra* note 68, at 43. Quine's argument for this conclusion is related to, but importantly different from, that advanced here. Quine based the conclusion on a criticism of the attempt by logical positivist Rudolf Carnap to "reduce" all empirically meaningful sentences to sentences about immediate sense experience. *Id.* at 37-42. An argument against the possibility of such reductionism is similarly one of the bases for Hempel's conclusion that it is impossible to formulate a viable testability criterion. See ASPECTS, *supra* note 56, at 107-18.

For agreement with this Article's assessment of the significance of Quine's argument, see L. M. Antony, *Quine as Feminist: The Radical Import of Naturalized Epistemology*, *in* A MIND OF ONE'S OWN: FEMINIST ESSAYS ON REASON AND OBJECTIVITY 202 (L.M. Antony & C. Witt eds., 1993) [hereinafter A MIND OF ONE'S OWN] ("With the demonstration that any belief, no matter how apparently self-evident, could in principle be rejected on the basis of experience, Quine effectively destroyed the prospects for any 'first philosophy' — any Archimedean fixed point from which we could inspect our own epistemic practice and pronounce it sound." (citation omitted)).

86. Philosopher Adolph Grunbaum argued, against Quine, that some scientific theories are capable of being conclusively falsified. *See* Grunbaum, *The Duhemian Argument*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 116-31. Grunbaum's arguments were highly controversial; he later retreated, claiming that it was possible to establish "a strong presumption of the falsity" of some theories. *See* Grunbaum, *Can We Falsify a Hypothesis Irrevocably?*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 283. This retreat is important because it destroys the possibility of drawing the sharp distinction that *Daubert* demands. For criticisms of Grunbaum's first, pre-retreat paper, *see* Laudan, *Grunbaum on the Duhemian Argument*, 155-61; Giannoni, *Quine, Grunbaum, and the Duhemian Thesis*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 162-75; Wedeking, *Duhem, Quine and Grunbaum on Falsification*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 176-83; Hesse, *Duhem, Quine and a New Empiricism*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 190-91.

Even in his first paper, Grunbaum implicitly admitted that some scientific theories cannot be conclusively falsified and used substantive scientific claims to dispute Albert Einstein's substantive scientific arguments against the possibility of establishing the falsity of the geometry of space. Therefore, Grunbaum's argument for a falsifiability criterion ironically supports this Article's contention that it is impossible to draw a clean and rational line between science and pseudo-science from an uncontroversial standpoint.

C. Testability Is an Inadequate Methodological Criterion

In response to these criticisms, Popper admitted that conclusive falsification is impossible:

> [N]o conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding.[87]

Popper accordingly conceded that neither the criterion of falsifiability — nor any other criterion — can distinguish science from pseudo-science on the basis of formal logic alone.[88] Rather than renouncing his theory, however, Popper insisted that the criterion of falsifiability was meant to distinguish not between "the formal or logical structure" of scientific and pseudo-scientific statements,[89] but instead between the scientific method and the methods of other enterprises:

> [T]he empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which . . . are logically possible. . . . [W]hat characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but . . . to select the one

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87. LOGIC, *supra* note 47, at 42, 50.

88. Popper stated: "[M]y criterion of demarcation cannot be applied immediately to a system of statements . . . . Only with reference to the methods applied to a theoretical system is it at all possible to ask whether we are dealing with . . . an empirical theory." *Id.* at 82. *See also id.* at 42 (admitting that "the logical value of my proposed criterion of demarcation [is] dubious, to say the least"); *id.* at 50 ("If . . . we characterize empirical science merely by the formal or logical structure of its statements, we shall not be able to exclude from it . . . metaphysics.").

Without either recognizing that Popper made this concession or attempting to argue that Popper should not have conceded, Black et al. state that, "[s]cientific explanations must be capable of falsification; that is, the logical form of a hypothesis must make it amenable to empirical testing." Black et al., *supra* note 2, at 783.

89. LOGIC, *supra* note 47, at 50.

> which is by comparison the fittest, by exposing them all to the fiercest struggle for survival.[90]

This methodological version of Popperian falsifiability relies on claims about basic statements, ad hocness, and the distinction between scientific discovery and criticism. A critical analysis of these three components reveals that the methodological version of falsifiability engenders admissibility decisions that are scientifically under- or over-inclusive. Popper's own amendments generate a conception of the scientific method that is either too vague to imply that some, but not other, work is scientific or determinate at the cost of concluding that some of the world's finest scientific work is pseudo-scientific. As Part III.D shows, philosophers and historians of science have built on these criticisms to argue cogently that practicing scientists are not — and should not be — falsificationists. These arguments undermine the possibility of applying a scientifically uncontroversial conception of the scientific method to reach rational, contemporaneous judgments of whether science is being done.

1. Basic Statements

As part of his methodological retreat, Popper conceded that observation sentences (what he called "basic statements") are themselves corrigible: "Any basic statement can again in its turn be subjected to tests, using as a touchstone any of the basic statements which can be deduced from it with the help of some theory, either the one under test, or another. This procedure has no natural end."[91] Accordingly, he admitted that the distinction between sentences that serve as a test and sentences that need to be tested is to some extent arbitrary: "Every test of a theory . . . must stop at some basic statement or other which we decide to accept. . . . Thus if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being."[92] One of Popper's central contentions is that, despite the lack of any foundation for testing that is itself beyond the need for

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90. *Id.* at 42. Popper similarly stated that, "the scientist consciously and cautiously tries . . . to refute his theories with searching arguments, including appeals to the most severe experimental tests which his theories and ingenuity permit him to design." CONJECTURES, *supra* note 49, at 52; *see also* LOGIC, *supra* note 47, at 82.

91. LOGIC, *supra* note 47, at 104.

92. *Id.*

testing, the "critical" method of science can still be distinguished from the "dogmatic" method of pseudo-science:[93]

> [W]e are stopping at statements about whose acceptance or rejection the various investigators are likely to reach agreement. And if they do not agree, they will simply continue with the tests, or else start them all over again. . . . If some day it should no longer be possible for scientific observers to reach agreement about basic statements this would amount to a failure of language as a means of universal communication.[94]

Even if scientists agree on which sentences by which to test, however, this fact does not show, without further argument, that their agreement is justified.[95]

Popper overestimated the significance of scientists' agreement on observation or basic statements in another way. According to Popper, "[b]asic statements [assert] that an observable event is occurring in a certain individual region of space and time."[96] The development of instrumentation means, however, that scientists can agree on such statements and yet disagree as to their factual import. For example, Galileo's disagreement with the Aristotelian astronomers of his time did not turn on what lines and figures were observable through his telescope. The dispute turned, instead, on whether those lines and figures actually depicted heavenly bodies. Similarly, current disputes about forensic DNA analysis seldom turn on how much the observed lengths of two bands on a gel differ. Instead, the crucial disagreement concerns how

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93. Popper wrote: "[T]he dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them — to test them; to refute them; to falsify them, if possible . . . we may identify the critical attitude with the scientific attitude, and the dogmatic attitude with the one which we have described as pseudo-scientific." CONJECTURES, *supra* note 49, at 50 (emphasis omitted).

94. LOGIC, *supra* note 47, at 104. Popper further argued: "The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of dogmas, but only in so far as we may desist from justifying them by further arguments (or by further tests). But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further." *Id.* at 105. In criticizing Popper, Allen fails to recognize that Popper thus attempted to account for the corrigibility of basic statements. *See* Allen, *supra* note 9, at 1171.

95. In other words, Popper fallaciously inferred an 'ought' from an 'is.' For the classic argument against such inferences, see DAVID HUME, A TREATISE OF HUMAN NATURE, bk.2, pt.3, § 3 & bk.3, pt.1, § 1 (1739). For a related consideration of how Popper fallaciously went from "is" to "ought" in replying to criticisms of the falsifiability criterion, see Harding, *supra* note 47, at xv.

96. LOGIC, *supra* note 47, at 103.

close the observed lengths of two bands must be in order to constitute a match.[97]

By ignoring such disputes about the interpretation of basic statements, Popper underestimated the extent to which practicing scientists actually differ about the "facts" that can properly be used to test theories.[98] Such disagreement indicates that to apply a testability criterion to determine whether someone is employing the scientific method, it is necessary to choose between scientists' competing substantive claims. Accordingly, disputes about testability, and thus falsifiability, often collapse into substantive scientific disputes. This conclusion casts doubt on Popper's central contention that, by developing a conception of the scientific method, philosophers, as rational outsiders, can "contribute to the advance . . . of scientific knowledge."[99] It also undermines *Daubert*'s fundamental assumption that a testability criterion can enable judges to avoid relying on scientifically controversial assumptions and yet rationally decide when the scientific method is being used.

2. The Prohibition of Ad Hocness

Popper also argued that, when taken as a methodological criterion, falsifiability is not called into question by the fact that scientists sometimes preserve theories in the face of conflicting observation sentences. In particular, Popper admitted that scientists sometimes make the "facts" fit an explanatory theory by conjoining the theory with new auxiliary hypotheses (i.e., interpretative theories, simplifying assumptions, or related explanatory theories).[100] Popper nonetheless contended

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97. *See, e.g.*, United States v. Chischilly, 30 F.3d 1144, 1154 (9th Cir. 1994), *cert. denied*, 115 S. Ct. 946 (1995); United States v. Yee, 134 F.R.D. 161, 171-72 (N.D. Ohio 1991).

98. In other words, Popper failed to recognize the full implications of his own acknowledgment that "there is no uninterpreted empirical basis; and the test statements which form the empirical basis cannot be statements expressing uninterpreted 'data' (since no such data exist) . . . . They are, of course, facts interpreted in the light of theories; they are soaked in theory, as it were." CONJECTURES, *supra* note 49, at 387 (citation omitted). For implicit recognition of the implications of the theory-ladenness of facts, see *Daubert II*, 43 F.3d at 1316 ("As the record in this case illustrates, scientists often have vigorous and sincere disagreements as to what . . . should be accepted as sufficient proof for the existence of a 'fact.'" (citation omitted)).

This Article's criticism of Popper's contentions about the significance of scientists' agreement on basic statements is related to a criticism advanced by Imre Lakatos. *See* Lakatos, *supra* note 77, at 236-37.

99. LOGIC, *supra* note 47, at 19.

100. For an explanation of auxiliary hypotheses, see Putnam, *supra* note 77, at 255; NATURAL SCIENCE, *supra* note 59, at 22-25.

that science can be distinguished from pseudo-science by its method of saving theories from falsification. The hallmark of science, according to Popper, is a refusal to "evad[e] falsification . . . by introducing ad hoc an auxiliary hypothesis, or by changing ad hoc a definition."[101] Thus, one of the main tasks for the philosophy of science is "to formulate methodological rules which prevent the adoption of conventionalist [or ad hoc] stratagems" for saving theories.[102]

Criticisms of Popper show that the notion of ad hocness cannot enable judges justifiably to determine when the scientific method is being used. By definition, an adjustment is ad hoc if it is "introduced . . . for the sole purpose of saving a hypothesis seriously threatened by adverse evidence."[103] Although Popper's absolute prohibition of such adjustments may seem intuitively plausible, noted philosopher Hilary Putnam has argued both that scientists do make ad hoc adjustments and that it is reasonable for them to do so.[104] For example, Putnam contended that scientists were justified in repeatedly using ad hoc measures, over a period of two hundred years, to rescue Newton's theory of universal gravitation from observational anomalies.[105]

It follows that if *Daubert*'s testability criterion is interpreted to include Popper's absolute prohibition of ad hoc adjustments, the criterion will, at best, be philosophically controversial. Moreover, the criterion will be under-inclusive in the sense that it will classify some of the world's finest scientific work as pseudo-scientific. However, *Daubert*'s testability criterion is not rescued by interpreting it to adopt Putnam's position that preserving a theory by means of ad hoc adjustments is sometimes good science. By this interpretation, it is impossible to determine whether the testability criterion is satisfied when particular scientists take particular ad hoc measures to rescue a theory. Thus, Putnam's argument implies that a testability criterion can avoid under-inclusiveness only at the cost of indeterminacy.

In contrast to Putnam, Hempel agreed with Popper that it is always scientifically undesirable to revise auxiliary hypotheses for the sole purpose of rescuing a "theory" from recalcitrant "facts."[106] Even if

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101. LOGIC, *supra* note 47, at 42.

102. *Id.* at 82.

103. NATURAL SCIENCE, *supra* note 59, at 29.

104. *See* Putnam, *supra* note 77, at 266.

105. *Id.* at 256-57, 266.

106. *See* NATURAL SCIENCE, *supra* note 59, at 28 (stating that "science is not interested in . . . protecting its hypotheses or theories at all costs — and for good reasons"); *id.* at 29 (explaining that even though an assumption that might have been used to rescue the historically discredited notion that nature abhors a vacuum "is not logically absurd or patently false, it is objectionable from the point of view of science. For it would be introduced ad hoc").

Hempel and Popper's position is philosophically superior to Putnam's, Hempel's own criticism of Popper implies that the notion of ad hocness cannot rescue the testability criterion from the dilemma of under- or over-inclusiveness.[107] According to Hempel, the notion that an ad hoc hypothesis is introduced for the sole purpose of saving a theory is not sufficiently precise to enable one to determine whether a particular hypothesis is ad hoc. Since "[t]here is, in fact, no precise criterion" for determining when a hypothesis is ad hoc,[108] the only adjustments to scientific theories that can confidently and rationally be dismissed as ad hoc are those that history has already discredited. "[W]ith the benefit of hindsight, it seems easy to dismiss certain scientific suggestions of the past as ad hoc hypotheses, whereas it may be quite difficult to pass judgment on a hypothesis proposed in a contemporary context."[109] This is relevant to *Daubert* because judges are not called on to decide the admissibility of scientific work that history has already judged.[110] The legal issue of admissibility arises only in regard to "hypothes[es] proposed in a contemporary context."[111]

The relevance of Putnam's and Hempel's criticisms might be disputed on the ground that Popper did not simply identify ad hoc hypotheses with ones introduced for the sole purpose of saving theories. In accord with his project of "lay[ing] down rules" to enable scientists to identify and avoid ad hoc strategies,[112] Popper claimed that in order not to be ad hoc, a hypothesis must "predict . . . some novel, hitherto

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107. Although Hempel did not specifically refer to Popper in his discussion of ad hoc hypotheses, a criticism of Popper's views is implicit in that discussion. *See* NATURAL SCIENCE, *supra* note 59, at 28-30.

108. *Id.* at 30. Similarly, Grunbaum acknowledged "[t]he unavailability of a formal sufficient condition for non-triviality," where a trivial hypothesis is one that "no scientist would accept as admissible." *The Duhemian Argument*, *supra* note 86, at 130 n.2.

109. NATURAL SCIENCE, *supra* note 59, at 30. Instead of recognizing and dealing with Hempel's criticism, Black et al. assume without argument that a sufficiently precise notion of ad hocness is available. *See* Black et al., *supra* note 2, at 759, 783, 785.

110. *See Daubert*, 509 U.S. at 597.

111. NATURAL SCIENCE, *supra* note 59, at 30.

112. LOGIC, *supra* note 47, at 82.

unexpected fact."[113] However, this further definition also gives rise to the dilemma of under- or over-inclusiveness.

In particular, Imre Lakatos and Paul Feyerabend both noted that Galileo's theory of the natural, circular motion of terrestrial objects was an ad hoc hypothesis, because it did not increase empirical content at the time it was introduced.[114] Notwithstanding Galileo's scientific stature, Lakatos concluded that because the scientific method precludes all ad hoc hypotheses, Galileo's theory of terrestrial motion was not good science at the time it was introduced.[115] However, after a detailed analysis of contemporary science, Feyerabend contended that "Galileo did use ad hoc hypotheses. [From the point of view of science,] [i]t was good that he used them."[116] From this historical example, Feyerabend deduced that it sometimes is good science to rely on hypotheses that are ad hoc by their failure to increase empirical content.[117]

Daubert's testability criterion must either be interpreted to include the philosophically controversial position of Popper and Lakatos, or that of Feyerabend. If, following Popper and Lakatos, a theory is untestable and pseudo-scientific whenever it is rescued from observational anomalies by ad hoc hypotheses that do not increase empirical content, the likely consequence is under-inclusiveness. The opposite vice of indeterminacy arises if *Daubert*'s criterion is interpreted to include Feyerabend's view that it sometimes is good science to rely on hypotheses that fail to increase empirical content. Only substantive scientific positions or personal biases will then enable judges to determine whether particular uses of ad hoc hypotheses comport with testability and the scientific method.

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113. Lakatos, *supra* note 77, at 228. *See also* LOGIC, *supra* note 47, at 82-83 ("[W]e decide to lay down the rule that only those [auxiliary hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it." (emphasis omitted)).

Strictly speaking, Hempel's doubts about the possibility of arriving at a sufficiently precise criterion of ad hocness apply to the notion that ad hoc hypotheses do not increase empirical content as well as to the notion that ad hoc hypotheses are introduced for the sole purpose of rescuing theories. Hempel wrote:

> There is, in fact, no precise criterion for ad hoc hypotheses, though the[se] questions . . . provide some guidance: is the hypothesis proposed just for the purpose of saving some current conception against adverse evidence, or does it also account for other phenomena, does it yield further significant test implications?

NATURAL SCIENCE, *supra* note 59, at 30 (emphasis omitted).

114. *See* Lakatos, *supra* note 77, at 233; PAUL FEYERABEND, AGAINST METHOD 93-98 (1978).

115. *See* Lakatos, *supra* note 77, at 233.

116. FEYERABEND, *supra* note 114, at 98.

117. *Id.* at 93-98.

3. The Distinction Between Scientific Discovery and Criticism

The dilemma of under- or over-inclusiveness is also generated by Popper's attempt to distinguish between testing and discovery of scientific ideas. Popper argued that since the justification for an idea has nothing to do with how it was discovered, the discovery process is of no philosophical interest. The task of the philosophy of science is to define the method of scientific testing or criticism necessary to justify discoveries, however they occur.[118] In Popper's words:

> The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man . . . is irrelevant to the logical analysis of scientific knowledge. . . .
>
> Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. . . . [T]he task of the logic of knowledge . . . consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.[119]

Thus, Popper sought to abstract the critical scientific work of testing from the creative scientific work of discovery. He aimed to delineate a method for scientific criticism alone.[120]

The problem is that the very possibility of scientific discovery depends on the types of criticism or testing pursued. To qualify as scientific, criticism must be consonant with discovery. By abstracting from the discovery process, Popper arrived at standards of criticism, but not at standards of scientific criticism.[121]

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118. *See id.* at 31.

119. *Id.*

120. Popper's distinction is similar to the logical positivists' distinction between the contexts of discovery and justification. *See* ISRAEL SCHEFFLER, SCIENCE & SUBJECTIVITY 2 (1967). Scheffler accepted this distinction, stating: "The process of critical appraisal is, then, integral to science, its operative canons reflecting, not the course of theory generation, but rather the practice of theory assessment." *Id.* at 72.

121. Similarly, Feyerabend argued that Popper "distinguished between the practice of science and standards of scientific excellence and asserted that epistemology dealt only with the latter: the world (of science, and of knowledge in general) must be adapted to the map, not the other way around." PAUL FEYERABEND, KILLING TIME 90 (1995). *See also id.* at 91 ("Popper's rules can produce a Byzantine science; they are not entirely without results. But these results are a far cry from the science of Newton, Faraday, Maxwell, Darwin, Einstein, and Bohr.").

Popper himself presented striking examples of the distortion involved in abstracting from the process of scientific discovery in order to delineate canons of scientific criticism. He recognized that "metaphysical ideas" are crucial to scientific progress:

> [A]long with metaphysical ideas which have obstructed the advance of science there have been others — such as speculative atomism — which have aided it. And looking at the matter from the psychological angle, . . . scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy; a faith which is . . . "metaphysical."[122]

Notwithstanding this acknowledgment of their importance, Popper nonetheless maintained that metaphysical ideas are excluded by the scientific method: "[T]he first task of the logic of knowledge [is] to . . . draw a clear line of demarcation between science and metaphysical ideas — even though these ideas may have furthered the advance of science throughout its history."[123]

The bizarre consequence is that, by Popper's own admission, scientific discovery and progress would be impossible if scientists consistently adhered to his falsificationist method of criticism and banned all metaphysical ideas.[124] Nor is falsificationism viable if it precludes scientists from entertaining metaphysical ideas when they act as critics, but allows them to develop such ideas when they act as creators. Falsificationism will fail to distinguish between scientific and pseudo-scientific work if any purported scientist entertaining any metaphysical idea at any time counts as a creator entertaining ideas that may contribute to scientific progress. However, Popper's aim to delineate a method for criticism, but not discovery, precludes any attempt to filter out metaphysical ideas that contribute to scientific progress from those that do not. Similarly, Popper's position that "[t]he question how it happens that a new idea occurs to a man . . . is irrelevant to the logical analysis of scientific knowledge"[125] renders impossible any effort to

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122. LOGIC, *supra* note 47, at 38.

123. *Id.*

124. This accords with Feyerabend's contention that "[p]racticed with determination and without subterfuge, the doctrine of falsifiability would wipe out science as we know it." FEYERABEND, *supra* note 121, at 90.

125. *Id.* at 31. Hilary Putnam's more general criticism of the logical positivist (and Popperian) distinction between contexts of discovery and justification is related to the criticism of Popper advanced above: "There are maxims [as opposed to algorithms] for discovery and maxims for testing: the idea that correct ideas just come from the sky, while the methods for testing them are highly rigid and predetermined, is one of the worst legacies

develop standards for when scientists should engage in criticism, and when they should engage in discovery.

Popper's attempt to distinguish the critical attitude of science from the dogmatic attitude of pseudo-science leads to analogous problems. According to Popper:

> [T]he dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them — to test them; to refute them; to falsify them, if possible. . . . [W]e may identify the critical attitude with the scientific attitude, and the dogmatic attitude with the one which we have described as pseudo-scientific.[126]

Notwithstanding this distinction, Popper claimed that the dogmatic or pseudo-scientific attitude is crucial to scientific progress: "this dogmatism allows us to approach a good theory in stages, by way of approximations: if we accept defeat too easily, we may prevent ourselves from finding that we were very nearly right."[127]

Here, as with his treatment of metaphysical ideas, Popper himself admitted that if scientists always adhered to the critical attitude of falsificationism, scientific progress would be impossible. However, falsificationism cannot viably be interpreted to require scientists to adopt the critical attitude when engaged in criticism, yet permit them to adopt the dogmatic attitude when engaged in discovery. Science cannot be distinguished from pseudo-science if any purported scientist, clinging to any idea, counts as a scientist engaged in discovery. However, so long as the scientific method is identified as a method of testing or criticism alone, it cannot support determinations of whether a given scientist should be engaged in criticism or discovery at a given time. Nor can

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of the Vienna Circle." Putnam, *supra* note 77, at 268.

126. CONJECTURES, *supra* note 49, at 50. Popper further described the dogmatic attitude:

> [W]e expect regularities everywhere and attempt to find them even where there are none; events which do not yield to these attempts we are inclined to treat as a kind of "background noise;" and we stick to our expectations even when they are inadequate and we ought to accept defeat.

Id. at 49.

127. *Id.* at 49. Similarly, Feyerabend argued: "[M]assive dogmatism . . . has . . . a most important function. Science would be impossible without it." Paul Feyerabend, *The Rationality of Science*, *in* CAN THEORIES BE REFUTED?, *supra* note 47, at 298.

scientific adherents of promising ideas be methodologically distinguished from dogmatic adherents of outmoded faiths.

D. Practicing Scientists Are Not — and Should Not Be — Falsificationists

Many philosophers and historians of science have concluded that since practicing scientists can practice falsificationism consistently only at the cost of scientific progress, the notion of falsifiability cannot be used to distinguish between science and pseudo-science. While Thomas Kuhn's *The Structure of Scientific Revolutions* is the most widely known exposition of this view, a devastating critique of falsifiability can be based solely on ideas that Kuhn shares with many other scholars, including Lakatos, Putnam, and Feyerabend.[128] The broad, anti-Popperian scholarly consensus concludes that falsifiability rests on a myth about science and therefore cannot validly guide judgments of whether science is being done. Moreover, widely-accepted arguments cast doubt not only on *Daubert*'s attempt to resurrect Popperian philosophy of science but also on *Daubert*'s attempt to delineate an alternative to *Frye*. While *Daubert*'s attempt rests on the assumption that an Archimedean standpoint exists,[129] the widely-accepted critique of methodological falsifiability challenges the existence of such a standpoint.

It is widely agreed that practicing scientists are not and should not be falsificationists because theories do not come with testable implications. Observation sentences follow only when theories are conjoined with auxiliary hypotheses, including interpretative theories, simplifying assumptions, statements of initial conditions, and supplementary

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128. In particular, a cogent criticism of falsifiability can be constructed without relying on three of Kuhn's most frequently cited and controversial positions: that the activities of "normal" science differ sharply from those of "revolutionary" science; that scientific work centers around "paradigms" rather than theories; and that "revolutionary" scientific disputes are marked by "incommensurability," or, in other words, by inability on the part of opposing scientists to understand each others' claims and to resolve disagreements rationally.

Farrell mistakenly assumes that Kuhn's controversial conceptions of "normal" versus "revolutionary" science, paradigms, and incommensurability are crucial to the philosophical critique of falsifiability. *See* Farrell, *supra* note 13, at 2195-96. This misconception is consistent with her mistaken view of Hempel as an ally of Popper, rather than as one of Popper's most formidable critics. *See id.* at 2199. *See also* Allen, *supra* note 9, at 1171-72; Note, *supra* note 12, at 1202-03 (presenting the mistaken view that Kuhn's distinctive positions, as opposed to positions that he shares with a broad spectrum of philosophers and historians of science, are crucial to a philosophical critique of *Daubert*'s alternative to *Frye*).

129. *See supra* Part II.

explanatory theories.[130] As established previously, the possibility of using a testability criterion to draw a logical distinction between scientific and pseudo-scientific sentences is undermined by the need "for an interpretative theory to prove the facts."[131] In turn, the methodological version of Popperian falsifiability is undermined by a consideration of the real life processes by which scientists frame auxiliary hypotheses and use them to derive observation sentences.

From a historical point of view, a primary vice of falsificationism is that it underestimates the difficulty of developing auxiliary hypotheses that can be used to test a theory. Thus, for example, the major difficulty in establishing Newton's theory of universal gravitation was formulating the auxiliary hypotheses needed for the theory to have observable implications. According to Putnam:

> Popper is right in thinking that a theory runs a risk during the period of its establishment. In the case of UG [the theory of universal gravitation], the risk was not a risk of definite falsification; it was the risk that Newton would not find reasonable AS [auxiliary hypotheses] with the aid of which he could obtain real (non-ad hoc) explanatory successes for UG.[132]

Arriving at useful and reasonable auxiliary hypotheses is difficult; concomitantly, scientists tend to have better grounds for believing in a theory than in the auxiliary hypotheses used to derive testable implications from the theory.[133] Thus, Popper is wrong to claim that the

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130. For the claim that interpretative theories are needed to derive observation sentences from any theory, see *supra* Part III.B.2. This Article defines "auxiliary hypotheses" broadly to include both (i) simplifying assumptions, statements of initial conditions, and supplementary explanatory theories and (ii) the interpretative theories that underlie observation sentences' claims to veridicality. However, the term auxiliary hypotheses is sometimes more narrowly defined to include only simplifying assumptions, statements of initial conditions, and supplementary explanatory theories. Using the narrow definition, Putnam and Hempel both argued that some of the most notable scientific theories — such as the theory of universal gravitation — must be combined with auxiliary hypotheses to yield observation sentences. *See* PUTNAM, *supra* note 77, at 255-58; NATURAL SCIENCE, *supra* note 59, at 22-28.

131. Lakatos, *supra* note 77, at 238.

132. PUTNAM, *supra* note 77, at 268.

133. *See id.* at 258, 266 ("The point that is often missed is that, in cases such as the one discussed [developing and using auxiliary hypotheses to derive observable implications from the theory of universal gravitation], the auxiliary statements are much less certain than the theory under test; without this remark, the criticism that one might preserve a theory by revising the AS [auxiliary hypotheses] looks like a bit of formal logic, without real relation to scientific practice."); KUHN, *supra* note 13, at 146 ("If any and every failure to fit [observations] were ground for theory rejection, all theories ought to be rejected at all times.").

hallmark of the scientific method is "an eagerness to revise the theory if we succeed in designing a test which it cannot pass."[134] To the contrary, scientists frequently justifiably retain their theories by replacing the auxiliary hypotheses that create conflicts with observation sentences. Historical examples discredit even the weaker Popperian position that the scientific method precludes introducing new auxiliary hypotheses or definitions for the sole purpose of rescuing a theory from observational anomalies.[135] Thus, Newton's theory was preserved by introducing hypotheses for the sole purpose of preserving the theory (for example, the hypothesis that certain stars have "dark companions") and by dismissing the conflict with the observed orbit of Mercury as an anomaly that would surely be explained away in time. Lakatos and Putnam both argued that these ad hoc measures were "good science" at the time.[136]

The Popperian opposition between critical science and dogmatic metaphysics is undermined by an additional difficulty in deriving observational consequences from theories. Even where auxiliary hypotheses are formulated, scientists may lack the requisite mathematical knowledge for determining what observational consequences follow from the hypotheses and a given theory. For example, extremely time-consuming and creative mathematical work was needed to derive the observational implications of Newtonian dynamics.[137] Similarly, "little is known to this day concerning just what the physical consequences of Einstein's 'unified field theory' are, precisely because the mathematical problem of deriving those consequences is too difficult."[138] The development of the requisite mathematics for testing theories would be severely hindered if, in accord with Popper, scientists concentrated on testing previously articulated, falsifiable ideas.

Moreover, long periods of time may pass before scientists develop the requisite technology for determining whether a prediction is confirmed.[139] For example, it took until 1995 for scientists to discover how to cool a dilute gas of atoms to a sufficiently low temperature to test and confirm Einstein's prediction, seventy years earlier, that Bose-Einstein condensates ("BEC"s) would exist as a result of the quantum behavior of assemblies of identical particles. "As nature kept foiling evermore-ingenious schemes for attaining the temperatures and densities

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134. CONJECTURES, *supra* note 49, at 51.

135. *See supra* notes 100-08 and accompanying text.

136. *See* PUTNAM, *supra* note 77, at 256-57, 266; Lakatos *supra* note 77, at 241-45. *See supra* notes 114-17 and accompanying text (discussing Feyerabend's and Lakatos's conflicting views on whether it was "good science" for Galileo to develop a theory of terrestrial motion that was ad hoc in the sense of not increasing empirical content).

137. *See* KUHN, *supra* note 13, at 34.

138. PUTNAM, *supra* note 77, at 262.

139. *See* KUHN, *supra* note 13, at 26-27.

of BEC, says Cornell [a discoverer of the requisite technology], 'people wondered if there was just some reason that Bose condensates were just not meant to be.'"[140] Popper's notion that the scientific method consists of "exposing all [systems] to the fiercest struggle for survival"[141] is questionable because Einstein's name, rather than the intrinsic merit of his prediction, appears to explain scientists' persistence in developing the requisite technology. "If anyone but Einstein had come up with the idea it would have been consigned to obscurity, physicists agree."[142]

E. No Archimedean Standpoint Exists

This criticism alone is not sufficient to justify rejecting *Daubert*'s alternative to *Frye*. Notwithstanding the *Daubert* majority's citation of Popper, *Daubert*'s alternative does not depend on the validity of Popper's conception of the scientific method. *Daubert*'s alternative is viable so long as some scientifically uncontroversial conception of the scientific method enables judges to rationally distinguish between science and pseudo-science. While the methodological version of Popperian falsifiability is vitiated by its inability to account for scientists' persistence in developing theories,[143] it is a mistake to conclude that scientists always adhere to theories, regardless of the conflict with observation sentences or the lack of observational consequences. As implicit in the title of Kuhn's book,[144] even long-standing theories are sometimes replaced. Building on the critique of Popperian falsifiability, a broad consensus of philosophers and historians of science has argued that no scientifically uncontroversial conception of the scientific method can be rationally applied to determine whether scientists are engaging in science or pseudo-science when a given theory is replaced by an alternative at a particular point in time. Thus, the Archimedean standpoint presupposed by *Daubert* does not exist.

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140. Gary Taubes, *Physicists Create New State of Matter*, SCI., July 14, 1995, at 152 (quoting Eric Cornell). *See also* M.H. Anderson et al., *Observation of Bose-Einstein Condensation in a Dilute Atom Vapor*, SCI., July 14, 1995, at 198; Keith Burnett, *An Intimate Gathering of Bosons*, SCI., July 14, 1995, at 182 ("The effort to observe Bose condensation in systems where the quantum nature of the transition is not obscured by the complications of strong interactions has been long and hard. It has involved heroic efforts by people in several communities." (citation omitted)); Malcolm W. Browne, *Atoms Without Identities*, N.Y. TIMES , July 16, 1995, § 4 (Week in Review), at 2; Malcolm W. Browne, *Two Groups of Physicists Produce Matter that Einstein Postulated*, N.Y. TIMES, July 14, 1995, at A1.

141. LOGIC, *supra* note 47, at 42.

142. Browne, *supra* note 140, at 2.

143. *See supra* Part III.C.2-3, III.D.

144. *See* KUHN, THE STRUCTURE OF SCIENTIFIC REVOLUTIONS, *supra* note 13.

There are no hard and fast rules for determining when theories should be changed. While scientific revolutions have sometimes been precipitated by observational anomalies, changes in theory are not always justified by conflicts with observations. For example, although the conflict between Newton's gravitational theory and the observed orbit of Mercury was one of the motivations for the Einsteinian revolution,[145] Newton's theory was considered successful because "Newtonians turned, with brilliant tenacity and ingenuity, one counter-instance after another into corroborating instances."[146] Long periods of time were required to explain away some of the anomalies.[147]

It is also implausible to claim that a change in theory is justified or considered scientific only when the new theory is falsifiable but unfalsified by any observation statements.[148] For example, there were many known anomalies to both Newton's and Einstein's theories at the time they were advanced.[149] Moreover, auxiliary hypotheses needed to be framed and mathematical and technological knowledge advanced before many of the observational consequences of these theories could be known. In short, the history of science shows that any test or method

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145. *See, e.g.*, Lakatos, *supra* note 77, at 210, 225, 233; Putnam, *supra* note 77, at 257.

146. Lakatos, *supra* note 77, at 241-42.

147. *See id.* at 77, at 213, 241-45; KUHN, *supra* note 13, at 31-32; PUTNAM, *supra* note 77, at 256-57.

148. Feyerabend argued against a criterion under which "good theories are theories which can be refuted, but which are not yet contradicted by any fact" on the ground that "[a] principle of falsification that removes theories because they do not fit the facts would have to remove the whole of science (or it would have to admit that large parts of science are irrefutable)." FEYERABEND, *supra* note 114, at 303. Likewise, Kuhn argued:

> Sir Karl [Popper]'s notion of falsification . . . presupposes . . . that a theory is cast, or can without distortion be recast, in a form which permits scientists to classify each conceivable event as either a confirming instance, a falsifying instance, or irrelevant to the theory . . . . In practice, however, no scientific theory satisfies these rigorous demands, and many people have argued that a theory would cease to be useful in research if it did so.

KUHN, *supra* note 66, at 810. For the mistaken assumption that scientists reject theories only for proposed alternatives that have survived testing, see Black et al., *supra* note 2, at 760-61.

149. Lakatos wrote: "Einstein's theory is not better than Newton's because Newton's theory was 'refuted' but Einstein's was not: there are many known 'anomalies' to Einsteinian theory." Lakatos, *supra* note 77, at 233. *See also id.* at 244 ("Most, if not all, Newtonian 'puzzles,' leading to a series of new variants superseding each other, were foreseeable at the time of Newton's first naive model and no doubt Newton and his colleagues did foresee them: Newton must have been fully aware of the blatant falsity of his first variants." (citation omitted)). Similarly, Kuhn stated: "When a new candidate for [a] paradigm is first proposed, it has seldom solved more than a few of the problems that confront it, and most of these solutions are still far from perfect." KUHN, *supra* note 13, at 156. *See also id.* at 154 ("Copernicus' theory was not more accurate than Ptolemy's and did not lead directly to any improvement in the calendar.").

for determining whether it is scientific to work within a given theory or to replace it with an alternative at a given time falls into the dilemma of under- or over-inclusiveness. The test or method may be too indeterminate to decide what is scientific at a given time. However, determinacy is achieved at the cost of defining some of the finest historical examples of scientific work as pseudo-scientific.

In recognition of these difficulties, Lakatos argued that "[n]ot an isolated theory, but only a series of theories can be said to be scientific or unscientific."[150] The weakness of *Daubert*'s proposal that judges become arbiters of the scientific method is revealed by considering Lakatos's criteria for assessing whether series of theories are scientific or pseudo-scientific. According to Lakatos, it is possible to judge the scientific character of a series of theories because some individual theories are connected to certain others in research programs.[151] A research program is scientific if "each step . . . is consistently content-increasing," and if "at least every now and then the increase in content should be seen to be retrospectively corroborated. . . . We do not demand that each step produce immediately an observed fact."[152]

The bare statement of these criteria shows how Lakatos's proposal for determining whether series of theories are scientific or pseudo-scientific falls into the dilemma of under- or over-inclusiveness into which all proposals for evaluating single theories have fallen. Lakatos's explicit requirement that new content in a series of theories be empirically confirmed "at least every now and then" is too vague to support determinations of whether a series has been rendered pseudo-scientific by the absence of empirical confirmation over a given period of time. However, any rigid time limit may imply that particularly promising research programs are pseudo-scientific. The history of science shows that "in a research programme we may be frustrated by a long series of 'refutations' before ingenious and lucky content-increasing auxiliary hypotheses turn a chain of defeats — with hindsight — into a resounding success story, either by revising some false 'facts' or by adding novel auxiliary hypotheses."[153]

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150. Lakatos, *supra* note 77, at 229.
151. *See id.* at 240.
152. *Id.* at 242.
153. *Id.* See also Feyerabend, *supra* note 127, at 296-97, noting:

> [S]tandards of this kind have practical force only if they are combined with a time limit. What looks like a degenerating problem shift may be the beginning of a much longer period of advance, so — how long are we supposed to wait? But if a time limit is introduced, then the argument . . . . [is] if you can wait, why not wait a little longer? . . . [T]here are theories which for centuries were accompanied by degenerating problem shifts until they found the right defenders and

Even if Lakatos's proposal solved the dilemma of under- or over-inclusiveness, it would nonetheless reveal that *Daubert*'s alternative to *Frye* is based on a fundamental misunderstanding of the history and philosophy of science. Unlike Kuhn, Feyerabend, and Putnam, Lakatos viewed himself as an intellectual heir of Popper, rather than an opponent.[154] Nonetheless, on the basis of the history and philosophy of science, Lakatos concluded that the distinction between science and pseudo-science can be coherently applied only in retrospect to research programs propounded over scores, if not hundreds, of years.[155] Judicial decisions on the admissibility of particular scientific research cannot be delayed, however, until enough years pass for it to be retrospectively possible to assess the entire series of theories to which the research is connected.[156] Therefore, the work of one of Popper's foremost defenders shows that the *Daubert* majority was misguided in attempting to resurrect the Popperian assumption that by adopting a scientifically uncontroversial perspective, outsiders can rationally resolve contemporary scientific disputes.

> returned to the stage in full bloom. The heliocentric theory is one example. The atomic theory is another .... [T]he new standards which Lakatos wants to defend either are vacuous — one does not know when and how to apply them — or else can be criticized on grounds very similar to those that led to them in the first place.

154. *See* Lakatos, *supra* note 77. Feyerabend's description of how people were recruited to be "faithful Popperians" might explain why Lakatos continued to identify himself as a disciple of Popper, despite the many fundamental doubts that his work raised about Popperian philosophy of science. *See* FEYERABEND, *supra* note 121, at 97. Describing his own refusal to become a "faithful Popperian," Feyerabend wrote:

> Falsificationism, I seemed to say to myself, may be OK; but why should I act as if it were a sacrament? Why, for example, should I put Popper on every page and into every footnote of everything I wrote? ... I seemed to be entering the domain of religious PR, group dynamics, or intellectual greed ....

Id.

155. *See* Lakatos, *supra* note 77, at 230 ("[C]rucial counterevidence — or 'crucial experiments'— can be recognized as such among the scores of anomalies only with hindsight, in the light of some superseding theory." (footnote omitted)).

156. The *Daubert* majority recognized that a standard for the admissibility of scientific evidence must be tailored to the short time frame of litigation: "Scientific conclusions are subject to perpetual revision. Law, on the other hand, must resolve disputes finally and quickly .... Rules of Evidence [are] designed not for the exhaustive search for cosmic understanding but for the particularized resolution of legal disputes." 509 U.S. at 597 (footnote omitted).

IV. IMPLICATIONS OF THE CRITIQUE OF *DAUBERT*: THE NEED TO RESURRECT AND REVAMP *FRYE*

A. Frye*'s Underlying Philosophical Insight*

The foregoing critique does more than show that *Daubert*'s alternative to *Frye* is philosophically unsound. It also implies that the much-maligned *Frye* standard is grounded in a major philosophical insight.[157] To appreciate *Frye*, it is necessary to see that criticisms like those above do not show that science is irrational.[158] Rather, the criticisms imply that certain widespread assumptions about rationality must be rejected.

One assumption is that science is rational only if non-scientists, without committing themselves to any controversial scientific claims, can apply rules to determine which competing scientists are actually employing the scientific method at any given time.[159] The existence of such algorithms seems inconsistent with scientists' need to undergo lengthy, specialized training. If non-scientists could grasp and apply the rules of the scientific method, there would be no need for research to be conducted within communities of specialists. There would be either no significant disputes among scientists or impartial lay people could resolve such disputes as they arose. It is absurd to claim that science is rational only if non-specialists can resolve disputes on which world renowned scientists take diametrically opposed positions. Since this claim follows from assuming that science is rational only if there are algorithms for determining how science should be done, that algorithmic conception of rationality must itself be questioned.

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157. For disagreement with this Article's contention that *Frye* is grounded in a fundamental philosophical insight, see, for example, Black et al., *supra* note 2, at 724-25, who observed: "*Frye*'s only salutory effect has been to institutionalize hesitation to embrace scientific claims too quickly, which is hardly a substitute for thoughtful consideration of scientific method." (footnote omitted).

158. *See, e.g.*, Lakatos, *supra* note 77, at 205-08, 226; Scheffler, *supra* note 120, at 72; LOGIC, *supra* note 47, at 34-37, 104; KUHN, *supra* note 13, at 205-07 (rejecting the claim that his work depicts science as an irrational enterprise).

159. Expressing the view that science is rational only if rational outsiders can apply its norms, Scheffler argues that there is no sharp dividing line between science and philosophy:

> There is, in general . . . no sharp line between the concerns of science and the concerns of epistemology. Scientists themselves are continuously engaged in rational reconstruction, criticism, and evaluation of ideas within their respective domains of investigation . . . . Epistemology may, perhaps, be conceived as striving for a greater generality and explicitness in its formulations, but it builds upon modes of criticism and evaluation internal to the workings of science itself.

SCHEFFLER, *supra* note 120, at 72; *see also id.* at 1-19.

The conception of rationality rests on the individualistic epistemology which has dominated Western philosophy since Descartes. The "knower" is equated with the isolated individual, skeptical of — and without allegiance to — any particular intellectual tradition. Claims to knowledge are legitimate only if they can be justified to this asocial individual.[160] This epistemological individualism implies that scientific claims can be justified — and science itself can be a rational enterprise — only if rules for the proper conduct of science can be applied by individuals who are not committed to any particular scientific tradition.[161]

A major problem with conceiving of the knower as an isolated individual is that human knowledge is not created from scratch by individuals. Instead, communities of people, building on substantive claims and methodological maxims or rules of thumb[162] passed on to them by earlier communities, have developed the human intellectual

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160. In contrast, Louise Antony has argued that since Descartes believed that a route to knowledge was travel and concomitant experience of human diversity, he did not view the knower as asocial. *See* Antony, *supra* note 85, at 196-97. The point, however, is that Descartes believed that the individual should experience human diversity in order to free himself from any particular, historically-bounded social or intellectual commitments. Thereby, the individual would attain the ideal epistemological state of assenting only to claims that can be justified apart from any particular social perspective. Descartes described the benefits of his travels:

> The greatest profit to me was . . . that I became acquainted with customs generally approved and accepted by other great peoples that would appear extravagant and ridiculous among ourselves, and so I learned not to believe too firmly what I learned only from example and custom. Also I gradually freed myself from many errors which would obscure the light of nature and make us less capable of correct reasoning. But after spending several years in thus studying the book of nature and acquiring experience, I eventually reached the decision to study my own self.

RENÉ DESCARTES, DISCOURSE ON METHOD 9 (L.J. LaFleur trans., 1960).

161. In accord with this reasoning, Popper and other prominent philosophers erroneously assumed that scientists' research decisions are either explainable by metholodogical rules that any rational person can apply or explainable only by the individual psychology of particular scientists. *See, e.g.*, LOGIC, *supra* note 47, at 31-32; SCHEFFLER, *supra* note 120, at 1-19, 72; Lakatos, *supra* note 77, at 205-07. This Article's critique of epistemological individualism implies that this dichotomy is false: research decisions can be explained by the fact that scientists train and work with others within particular research traditions. *See* KUHN, *supra* note 13, at 176 ("If this book were being rewritten, it would . . . open with a discussion of the community structure of science."); *id.* at 178 (arguing that "the producers and validators of scientific knowledge" are scientific communities).

162. The foregoing argument against the existence of determinate rules — or algorithms — of the scientific method does not deny the existence of maxims or rules of thumb for both scientific discovery and criticism. *See supra* note 125 (Putnam explains how the distinction between rules and maxims bears on the philosophy of science).

heritage.[163] This development has gone hand in hand with intellectual specialization.[164] One example of specialization is the study of population genetics within the general field of biology.[165] Peer review and promotion decisions in universities and other institutional practices rest on the assumption that scholars within a discipline are the proper judges of work within that discipline.[166] This goes against assuming that an ahistorical individual, unsullied by intellectual specialization or allegiances, can properly judge all knowledge claims. Since knowledge has been developed by communities of people bound together by particular methodological maxims and interconnected beliefs, commitment to some, rather than other, historically-developed methods and beliefs would seem necessary for judging the legitimacy of knowledge claims.[167] Thus, there is no Archimedean standpoint for judging whether

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163. *See* Naomi Scheman, *Though This Be Method, Yet There Is Madness in It: Paranoia and Liberal Epistemology*, *in* A MIND OF ONE'S OWN, *supra* note 85, at 147 ("Knowledge rests not on universally recognizable and unassailable premises but on the social labor of historically embodied communities of knowers.").

Quine partially recognizes the social character of human knowledge: "Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic." QUINE, *supra* note 68, at 46. While Quine's statement correctly recognizes that each person develops beliefs on the basis of an intellectual heritage developed by others, the statement wrongly suggests that people individually decide how to develop their intellectual legacies. In contrast, this Article argues that communities of people jointly build on the intellectual foundations that they have jointly inherited.

164. The process of intellectual specialization is not simply a matter of the division of fields that were the concern of broad communities into subfields of concern to particular subcommunities. To the contrary, Kuhn explained: "There was, for example, no physics community before the mid-nineteenth century, and it was then formed by the merger of parts of two previously separate communities, mathematics and natural philosophy . . . . What is today the subject matter for a single broad community has been variously distributed among diverse communities in the past." KUHN, *supra* note 13, at 179.

165. As Kuhn recognized, the critique of falsificationism implies that we can understand the development of scientific methods only if we understand the development of specialized scientific research communities. *See id.* at 176-81.

166. *See id.* at 177-78.

167. In contrast, epistemological individualism implies that proper knowledge claims must be universally justifiable (i.e., justifiable to all rational human beings), so long as they are free from particularistic perspectives. Naomi Scheman similarly recognized the connection between epistemological individualism and universalism, noting:

> The individualism of Cartesian epistemology is yoked to its universalism: Though we are each to pursue knowledge on our own, freed from the influence of any other people, what we come up with is not supposed to be our own view of the world — it is supposed to be the truth, unique and invariable . . . . Individualism is the route not to the idiosyncracies of individuality but to the universality of reason.

See Scheman, *supra* note 163 at 152; *see also* Schwartz, *Towards a Jurisprudence of Labor Law: Methodological Preliminaries*, *supra* note 58, at 91-94 (discussing the interconnec-

intellectual work — including science — is being properly performed. The act of judging, like any other intellectual work, must proceed from a commitment to a particular intellectual tradition.

In contrast to *Daubert*, *Frye* recognizes the social character of human knowledge in general and science in particular. By making acceptance by the relevant scientific community the criterion for the admissibility of scientific evidence, *Frye* implicitly recognizes that there is no extra-scientific standpoint from which judges — or any one else — can rationally assess the scientific merits of proposed scientific evidence. Admissibility can be based on scientific merit only if judges defer to practicing scientists' assessments of scientific merit.[168] Therefore, acceptance by the current scientific community is the sole rational basis for contemporaneous judicial determinations of whether proferred scientific testimony is in fact scientific.

B. The Need for a Special Standard for the Admission of Scientific Evidence

Resurrection of the *Frye* standard is warranted only if there is a justification for making judges gatekeepers with regard to scientific evidence. *Daubert* and *Frye* both make testimony by a qualified scientific expert admissible only if a judge determines beforehand that its subject matter is genuinely scientific. A qualified expert's testimony might be required only to meet the criteria generally set for the admission of evidence. The critique of *Daubert* calls for a return to *Frye* only if there is a sound reason for *Daubert* and *Frye*'s shared assumption that scientific evidence should be subject to a special admissibility standard.

The requisite justification is provided by the special prerogatives that American courts accord to expert witnesses. Under both the Federal

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tions between the view that proper political principles are universally justifiable and a view termed "metaethical individualism").

168. Rather than criticizing *Daubert* for assuming that judges have access to a non-existent Archimedean standpoint, Milich erroneously criticizes *Daubert* for assuming that judges are better at making substantive scientific judgments than the scientists themselves. *See* Milich, *supra* note 12, at 918-20, 924-25. In agreement with this Article, however, Milich concludes that deference to scientists is the only rational basis for judicial determinations of the scientific merit of proffered evidence; he notes:

> Science is the only source of its own reliability. Anything less than complete deference to the weight of credible scientific opinion concerning the reliability of scientific evidence means going outside science — to the judge or jury . . . — to resolve a scientific dispute. The resulting judgment cannot be scientific and therefore we cannot honestly speak of the evidence as having "scientific" reliability.

Id. at 923-24. *See also id.* at 926 (warning that "the *Daubert* Court invites the rather Orwellian prospect of judges 'deeming' what is and is not good science").

Rules of Evidence and most state laws, an expert, unlike an ordinary witness, "is permitted wide latitude to offer opinions, including those that are not based on first-hand knowledge or observation."[169] However, because of the generally recognized danger that juries will be awed by expert evidence (especially scientific evidence),[170] admissibility of scientific evidence in the form of expert testimony makes sense only if the science is judged to be of special value in the courtroom. Science must, more generally, be judged to be of special intellectual and social worth.[171] Although such assessments of the worth of science are controversial,[172] so long as the legal treatment of scientific evidence

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169. *Daubert* claims that Federal Rules of Evidence 701, 702, and 703 accord experts these distinctive prerogatives and argues "this relaxation of the usual requirement of first-hand knowledge . . . is premised on an assumption that the expert's opinion will have a reliable basis in the knowledge and experience of his discipline." 509 U.S. at 592. Most states track Rule 701 in requiring lay, but not expert, witnesses to base their opinions on first-hand knowledge, and also track Rule 703 in permitting expert, but not lay, witnesses to rely on hearsay. *See* CHRISTOPHER B. MUELLER & LAIRD C. KIRKPATRICK, 3 FEDERAL EVIDENCE § 342, at 579 & § 345, at 672 (2d ed. 1994); Simard & Young, *supra* note 4, at 1459.

170. *See, e.g.*, *Daubert*, 509 U.S. at 595 ("Judge Weinstein has explained: 'Expert evidence can be both powerful and quite misleading because of the difficulty in evaluating it.'"(citation omitted)); Joseph Saunders, *Scientific Validity, Admissibility, and Mass Torts After Daubert*, 78 MINN. L. REV. 1387, 1435 (1994) ("A mounting body of evidence supports the position that jurors do have a difficult time understanding and assessing expert scientific testimony."); Ake v. Oklahoma, 470 U.S. 68, 81 n.7 (1985); Black et. al, *supra* note 2, at 789 ("[M]ost commentators believe ostensibly scientific testimony may sway a jury even when as science it is palpably wrong. Science can be greatly distorted by the pressures of litigation, but once admitted into evidence, it has an imprimatur of legitimacy and validity, and cross-examination often will not expose its flaws." (footnotes omitted)); *The Supreme Court, 1986 Term — Leading Cases*, 101 HARV. L. REV. 119, 127 (1987) [hereinafter *Leading Cases*]; Giannelli, *supra* note 46, at 1237; Simard & Young, *supra* note 4, at 1459-62. *But see* Barefoot v. Estelle, 463 U.S. 880, 899 (1983) (assuming that "cross-examination and contrary evidence by the opposing party" suffice to prevent jurors from being overawed by psychiatric testimony); Michael S. Jacobs, *Testing the Assumptions Underlying the Debate about Scientific Evidence*, 25 CONN. L. REV. 1083, 1090, 1094-98 (1993).

171. *See, e.g.*, Milich, *supra* note 12, at 914-15 ("We accept science in the courtroom because we have confidence in the methods and respect for the achievements of science. The law gives science a special status in the courtroom, one denied to astrologers, mystics, and others who practice alternative modes of knowledge."); Giannelli, *supra* note 46, at 1200 ("[T]he use of scientific knowledge to solve legal problems has long been recognized, and it is not surprising that a society so dependent on science and technology should turn to such knowledge as a method of proof." (footnote omitted)).

172. *See, e.g.*, MAX WEBER, *Science as a Vocation*, *in* FROM MAX WEBER: ESSAYS IN SOCIOLOGY 139 (Gerth & Mills, trans. and eds. 1946) ("The increasing intellectualization and rationalization [created by science and by scientifically oriented technology] do not . . . indicate an increased and general knowledge of the conditions under which one lives."); Adina Schwartz, Book Review, 92 PHIL. REV. 258, 259 (1983) (reviewing A. WOOD, KARL MARX, and discussing Weber's doubts about whether scientific progress leads to an enlightened society); MILLS, *supra* note 16, at 168 ("Science, it turns out, is not a

implies that science is especially valuable, judges can justifiably admit some evidence as scientific only if they have reason to believe that it is scientific. Since judges must defer to scientists if they are rationally to decide on the scientific status of expert testimony, this critique of *Daubert* argues for a return to *Frye*.

C. The Need for a Revamped Frye *Standard*

Though a return to *Frye* is needed, this standard as it developed in the case law requires modification. Around *Frye*'s dictate of deference to scientists, varying *Frye* tests have developed.[173] Courts have differed over the specification of the scientific community whose judgment must determine whether a principle or technique is genuinely scientific.[174] Courts have also differed over how much agreement within the relevant scientific community constitutes the general acceptance required for admissibility.[175] Judicial definitions of the relevant scientific community

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technological Second Coming. That its techniques and its rationality are given a central place in a society does not mean that men live reasonably and without myth, fraud and superstition."); FEYERABEND, *supra* note 121, at 146 ("Science is anything but the 'free' and 'open' enterprise philosophers are dreaming about. Business considerations play a large role, the race for Nobel Prizes drastically reduces communications between scientists."); FEYERABEND, *supra* note 114, at 295-309; Boaventura de Sousa Santos, *Three Metaphors for a New Concept of Law*, 29 L. & SOC'Y REV. 569, 570-72 (1995).

173. *See* Giannelli, *supra* note 46, at 1228. *See also* Black, *supra* note 77, at 643-44 ("Courts have shown a remarkable ability to manipulate *Frye* to reach desired results . . . ."); United States v. Downing, 753 F.2d 1224, 1237 (3d Cir. 1985) (noting that the *Frye* standard is "too malleable").

174. *See, e.g.*, Giannelli, *supra* note 46, at 1208-10; Friedman, *supra* note 11, at 142.

175. For example, the New York Court of Appeals recently stated that "the particular procedure need not be 'unanimously endorsed' by the scientific community but must be 'generally acceptable as reliable.'" People v. Wesley, 633 N.E.2d 451, 454 (N.Y. 1994) (quoting People v. Middleton, 429 N.E.2d 100, 103 (N.Y. 1981)). Also, the California Supreme Court stated that "'[g]eneral acceptance' . . . means a consensus drawn from a typical cross-section of the relevant, qualified scientific community." People v. Leahy, 882 P.2d 321, 337 (Cal. 1994). *See also id.* at 336 (arguing that this formulation was consistent both with the statement that a "technique or theory is not 'generally accepted' if there is public opposition to it by scientists significant either in number or expertise," and the statement that the *Frye* test does not require "absolute unanimity of views in the scientific community . . . . Rather, the test is met if use of the technique is supported by a clear majority of the members of that community.") (explaining People v. Shirley 723 P.2d 1354, 1375 (Cal. 1982); People v. Guerra, 690 P.2d 635, 656 (Cal. 1984)).

In *U.S. v. Bonds*, 12 F.3d 540 (6th Cir. 1993), the Sixth Circuit adumbrated a liberal interpretation of the general acceptance test, holding that general acceptance does not require acceptance by a majority of scientists in the relevant scientific community, stating: "Only when a theory or procedure does not have the acceptance of most of the pertinent scientific community and in fact a substantial part of the scientific community disfavors the principle or procedure, will it not be generally accepted." *Id.* at 562.

For discussions of differing courts' specifications of the requisite amount of agreement

and of the requisite amount of acceptance have been dispositive of whether a particular theory or technique has "gained general acceptance in the particular field in which it belongs."[176] However, courts have tended to ignore the controversiality of their definitions, rather than attempting to justify them.[177] As a result, "the process of choosing an appropriate 'community' and determining the necessary degree of support within that community [has] allow[ed] a judge's subjective beliefs as to the 'true value' of the evidence to influence the admissibility decision [under *Frye*]."[178]

These problems in defining the relevant scientific community or requisite degree of acceptance reflect genuine difficulties in applying *Frye*'s principle of judicial deference to scientific opinion. The history and philosophy of science show that practicing scientists' opinions

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under *Frye*, see, e.g., Giannelli, *supra* note 46, at 1210-11; Friedman, *supra* note 11, at 142-43; Melissa M. Heine, Note, *Novel Scientific Evidence: Does Frye Require that General Acceptance Within the Scientific Community Be Established by Disinterested Scientists?*, 65 U. DET. L. REV. 147, 155 (1987) ("Although it is generally agreed that *Frye* does not require unanimous acceptance, a consensus on whether a certain percentage of those in the field must accept the technique has never been achieved. Most courts define the standard generally, rather than quantitatively, or ignore the issue altogether." (footnotes omitted)); Hao-Whien Q. Yu & Richard A. Tamor, *Of Daubert, Elvis and Precedential Relevance*, 41 UCLA L. REV. 487, 493 (1993) ("The term 'general acceptance' in *Frye* can mean anything from 51% to unanimity"). *See also* People v. Young, 391 N.W.2d 270, 288 (Mich. 1986) (Boyle, J., dissenting) ("[A] criticism of the *Frye* test is that there are no definite criteria to use to decide if there has been general acceptance. Because it is impossible to find unanimous agreement in any field, the courts have been hard pressed to find the appropriate number of experts who must have accepted the technique as reliable."); *infra* Part VI.B (discussing courts' additional disagreement over whether general acceptance should be determined by simple headcounting or by according relative weights to the opinions of different members of the relevant scientific community).

176. *Frye*, 293 F. at 1014.

For the dispositiveness of definitions of the relevant scientific community and of the requisite degree of acceptance, see, e.g., *Wesley*, 633 N.E.2d at 463-64 (Kaye, C.J., concurring) ("In defining the relevant scientific field, the court must seek to comply with the *Frye* objective of obtaining a consensus of the scientific community. If the field is too narrowly defined, the judgment of the scientific community will devolve into the opinion of a few experts." (citation omitted)); United States v. Williams, 583 F.2d 1194, 1198 (2d Cir. 1978) ("Selection of the 'relevant scientific community,' appears to influence the result.") (quotation omitted); Giannelli, *supra* note 46, at 1208-11; Kaushal B. Majmudar, Note, *Daubert v. Merrell Dow: A Flexible Approach to the Admissibility of Novel Scientific Evidence*, 7 HARV. J.L. & TECH. 187, 197 (1993) ("The vagueness in the *Frye* standard [in regard to the appropriate scientific community and the requisite level of consensus in that community] was reflected in the varying applications of the technique in lower courts."); *The Supreme Court — Leading Cases*, *supra* note 11, at 258.

177. *See* Giannelli, *supra* note 46, at 1210-11.

178. Majmudar, *supra* note 176, at 198. *See also Downing*, 753 F.2d at 1236 ("[T]he vague terms included in the [*Frye*] standard have allowed courts to manipulate the parameters of the relevant scientific community and the level of agreement needed for general acceptance.").

provide the only rational basis for contemporaneous judicial assessments of the scientific status of work, but that science proceeds only when there are unresolved questions about how to apply a given research tradition. The existence of such questions creates the possibility that scientists will even question the tradition and its well-established theories. Thus, unanimity cannot be the requisite degree of acceptance for according scientific status to contemporary work.[179]

The rule that for work to be deemed scientific, it must merely have attained some degree of acceptance within the relevant scientific community (albeit not unanimous acceptance), is not an adequate criterion of admissibility. By allowing widely different specifications of the amount of agreement required for general acceptance, this rule permits inconsistent decisions on whether the *Frye* standard is satisfied.[180] However, determinacy is not the sole requisite for an adequate revision of the *Frye* standard. The revised definition of the requisite degree of acceptance must also be compatible with a realistic conception of the roles of agreement and debate within scientific research communities.

To apply *Frye*'s dictate of judicial deference to scientists, a principled response to the rise of specialized scientific research communities is also needed. Since the domains of inquiry of these communities may overlap, selecting the proper academic discipline or professional field for determining whether a technique or theory is

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179. Absolute unanimity is not necessary under *Frye*'s general acceptance standard. *See, e.g.*, Commonwealth v. Lykus, 327 N.E.2d 671, 678 n.6 (Mass. 1975) ("[The] *Frye* standard does not require unanimity of view, only general acceptance; a degree of scientific divergence of view is inevitable."); People v. Soto, 35 Cal. Rptr. 2d 846, 853 (Ct. App. 1994), *review granted*, 890 P.2d 1115 (Cal. 1995) (reasoning that unanimity is not requisite under the California version of the *Frye* test because "the very nature of science encompasses constant and continuous refinement, improvement and clarification"); *see also* United States v. Bonds, 12 F.3d 540, 556-57, 561-62 (6th Cir. 1993); People v. Leahy, 882 P.2d 321, 336 (Cal. 1994); State v. Vandebogart, 652 A.2d 671, 678 (N.H. 1994); People v. Wesley, 633 N.E.2d 451, 454 (N.Y. 1994); Giannelli, *supra* note 46, at 1211.

180. Giannelli argues that, due to courts' failure to define how much disagreement is compatible with general acceptance, *Frye* has become too malleable a standard. He observed:

> Most courts applying *Frye* have not addressed the issue adequately .... For example, one court has defined general acceptance as 'widespread; prevalent; extensive though not universal.' Another court has conceded that 'a degree of scientific divergence of view is inevitable,' without elaborating on how much divergence would be dispositive .... [T]he latitude allowable to a court under the malleable *Frye* standard could yield the admission of evidence that a large segment of the scientific community would find unacceptable.

Giannelli, *supra* note 46, at 1211 (footnotes omitted).

generally accepted may prove "troublesome."[181] In particular, the use of science in the courtroom has been correlated with the rise of separate communities of forensic scientists and of theoretical scientists whose work provides the underpinnings for particular forensic techniques.[182] There is a danger of bias if general acceptance is determined by those specializing in the application and development of a forensic technique. Since these persons' professional reputations and commercial interests may depend on validation of the technique, general acceptance may be a foregone conclusion if they are identified as the relevant scientific community.[183] However, there is a danger in identifying theoreticians as the relevant community — deference may be paid to those who do not understand the distinctive problems involved in extending a theory or applying a technique to a forensic setting.[184]

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181. *Id.* at 1208 (footnotes omitted). *See also* Black et al., *supra* note 2, at 728-29 ("Because *Frye* requires that a scientific principle be generally accepted 'in the particular field in which it belongs,' a court applying the test has to define the boundaries of the field . . . . This step can be problematic because the complex reasoning involved in developing methods or techniques may involve several different fields." (footnotes omitted)).

182. *See* Giannelli, *supra* note 46, at 1211, 1213-14.

183. Thus, Giannelli states that "general acceptance of the polygraph is almost assured if the opinions of [polygraph] examiners are considered." *Id.* at 1214. *See also id.* at 1210, 1213-14 (discussing the danger of bias arising from the identification of forensic specialists as the relevant scientific community); Friedman, *supra* note 11, at 142, n.39 (illustrating the importance of the question of whether *Frye* allows a court to define a field consisting of those who practice and believe in a given technique by citing conflicting cases on whether the field of forensic chemistry is the appropriate community for purposes of determining the general acceptance of electrophoretic techniques for identifying body fluids); Jones v. United States, 548 A.2d 35, 42 (D.C. 1988) (noting that "a proffered expert's bias or incompetence" may stem from the fact that "[t]he expert . . . may be the principal proponent of a controversial technique"); People v. Brown, 709 P.2d 440, 449 (Cal. 1985) (en banc) (questioning whether testimony by a forensic chemist employed by a police department would suffice "to establish acceptance by impartial scientists in the field of forensic chemistry"); *Downing*, 753 F.2d at 1236 ("[S]ome courts, when they wish to admit evidence, are able to limit the impact of *Frye* by narrowing the relevant scientific community to those experts who customarily employ the technique at issue."); *see also infra* Part V.B.2.

Black similarly argued that by allowing use of a technique by forensic scientists to be the sole basis for a finding of general acceptance, courts have "allow[ed] a group that advocates a technique or method to self-validate it simply by declaring acceptance." Black, *supra* note 77, at 633. However, Black et al. later drew contradictory conclusions. *See supra* note 2, at 728-30. Although Black et al. noted that under the *Frye* standard, "a 'field' may be so narrowly defined that it encompasses only advocates and no real critics," they did not conclude that the *Frye* standard should be modified to eliminate this danger of bias. *Id.* at 729. In contrast to Black's earlier stance and to that of this Article, Black et al. concluded that this danger sufficed to show that the *Frye* standard is fundamentally flawed.

184. *See* Giannelli, *supra* note 46, at 1209-10 (discussing cases and arguing for the proposition that although theoretical scientists may legitimately be excluded from the relevant scientific community on the ground that they are unfamiliar with a forensic

New York Court of Appeals Chief Judge Kaye's concurrence in *People v. Wesley*[185] recognized these opposing dangers, challenging the majority's holding that forensic DNA analysis, as of 1988, passed the *Frye* test. Chief Judge Kaye refused to identify the forensic scientists who pioneered the development of forensic DNA analysis as the relevant scientific community: "The opinions of two scientists, both with commercial interests in the work under consideration and both the primary developers and proponents of the technique [of forensic DNA analysis], were insufficient to establish 'general acceptance' in the scientific field."[186] However, Chief Judge Kaye also concluded that general acceptance of the restriction fragment length polymorphism ("RFLP") procedure used in forensic DNA analysis for identifying and typing DNA fragments whose sizes vary from person to person[187] could not be based on the opinions of three theoretical scientists. According to Chief Judge Kaye, these theoreticians did not belong to the relevant scientific community because they were familiar with the use of RFLP procedure for research and diagnostic purposes but not familiar with its

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technique, exclusion on the ground that their knowledge of the technique is merely theoretical is improper).

This Article's requirement that those in a relevant scientific community understand the problems involved in a forensic application serves the same purpose as Black's proposal that "an acceptance test . . . [not] ignore . . . the purpose or application for which a method or device has been accepted." Black, *supra* note 77, at 633. Thus, this Article's proposals for distinguishing between relevant and irrelevant scientific communities solve Black's problem of identifying what must be accepted. *See infra* Part V.B.

185. 633 N.E.2d 451, 461 (N.Y. 1994). Chief Judge Kaye's decision to concur, rather than dissent, was based on harmless error analysis: "[I]t was error to admit the DNA bloodstain analysis in this case. We nevertheless agree that defendant's conviction should be affirmed, because that evidence comprised only a minor part of the People's case." *Id.*

Both the majority opinion and concurrence in *Wesley* proceed from the holding that notwithstanding *Daubert*, *Frye* continues to govern in New York state courts. *Id.* (Kaye, C.J., concurring) ("The Court agrees unanimously that where the scientific evidence sought to be presented is novel, the test is that articulated in Frye.").

186. *Wesley*, 633 N.E.2d at 465 (citation omitted). *Accord* United States v. Yee, 134 F.R.D. 161, 195 (N.D. Ohio 1991) (noting that "the F.B.I.'s DNA principles and procedures must be shown to be generally acceptable to scientists beyond the forensic users of such techniques").

In contrast, the New York State Appellate Division, Third Department failed to criticize the trial court for "determin[ing] that the People's witnesses were more credible than defendant's in that they were more closely associated with the field of forensic DNA identification while defendant's witnesses were associated with academic research." People v. White, 621 N.Y.S.2d 728, 731 (N.Y. App. Div. 1995).

187. For descriptions of RFLP analysis, see NATIONAL RESEARCH COUNCIL, DNA TECHNOLOGY IN FORENSIC SCIENCE 1-50 (1993) [hereinafter 1992 NRC REPORT]; *Wesley*, 633 N.E.2d at 459-61; People v. Soto, 35 Cal. Rptr. 2d 846, 849-50 (Ct. App. 1994), *review granted*, 890 P.2d 1115 (Cal. 1995); *Yee*, 134 F.R.D. at 169-71.

forensic application:[188] "Focussing on DNA profiling in the forensic setting is crucial because 'DNA fingerprinting is far more technically demanding than DNA diagnostics,' particularly in the art of declaring a 'match' between samples."[189]

The existence of separate communities of theoretical and forensic scientists is not the only cause of difficulty in determining which scientists' opinions should be dispositive under Frye. The domains of inquiry for various communities of theoretical scientists may also overlap. Thus, whether RFLP analysis or other techniques can validly establish a match between a suspect's DNA and a forensic sample is only part of the controversy about forensic DNA analysis. In her *People v. Wesley* concurrence, Chief Judge Kaye stated: "evidence of a 'match' is virtually meaningless without resort to the statistical interpretation; population genetics is arguably the most crucial step of the analysis. It is the area of greatest controversy among the experts."[190] However, in

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188. Chief Judge Kaye's concern about whether these theoreticians belonged to the relevant community for determining whether there was general acceptance of RFLP procedure in a forensic setting is similar to Black's concern that the scientific community's acceptance of the use of a technique for some purposes not be taken to imply acceptance of that technique for other purposes. *See* Black, *supra* note 77, at 633-34. In particular, Black criticizes People v. Haggart, 370 N.W.2d 345 (Mich. Ct. App. 1985), for finding general acceptance of electrophoresis for purposes of analyzing blood on the basis of the scientific acceptance of electrophoresis for other purposes. *See id.* at 633 n.193.

189. *Wesley*, 633 N.E.2d at 468 (citing Eric S. Lander, *DNA Fingerprinting on Trial*, 339 NATURE 501 (1989)). *See also* Scheck, *supra* note 11, at 1964-65 (describing differences between forensic and diagnostic applications of DNA profiling); 1992 NRC REPORT, *supra* note 187, at 52-53; *Yee*, 134 F.R.D. at 164-65 (stating that for purposes of evaluating the FBI's forensic DNA procedures, the "pertinent scientific community is made up of 'scientists from the fields of molecular biology and population genetics who have expertise in either or both of these fields and a reasonably comprehensive understanding about the F.B.I.'s DNA testing protocol and procedures'" (citation omitted)).

Interestingly, during the *Yee* court's "*Frye* hearing," a defense expert agreed while a government expert disagreed that "forensic applications were more demanding to interpret [than diagnostic applications]." *Yee*, 134 F.R.D. at 175, 178. This dispute about the relative difficulty of interpreting forensic and diagnostic applications of DNA profiling alone provides an argument that those in the relevant scientific community be familiar with forensic applications.

Judge Kaye's argument is similar to the Michigan Supreme Court's argument in *People v. Young* for selecting the relevant scientific community so as to measure acceptance of electrophoresis of evidentiary bloodstains, rather than for acceptance of other uses of electrophoresis. *See* People v. Young, 391 N.W.2d 270, 274-75 (Mich. 1986); *see also* People v. Brown, 709 P.2d 440, 448 (Cal. 1985) (en banc) (stating that the "scientific validity of genetic typing tests in general" does not suffice to show that aged-stain typing passes the *Frye* test).

190. 633 N.E.2d at 466 n.9. *See also* Nelson v. State, 628 A.2d 69, 76 (Del. 1993); *New Challenges of Scientific Evidence*, *supra* note 9, at 1542-43 ("Although naked evidence of a match means that the DNA sample is consistent with the defendant's DNA, it may also be consistent with the DNA of others. Without some estimate of the frequency

People v. Soto, a California appellate court recently held that there was general acceptance of the "product rule," a particular statistical method for calculating the likelihood that a match between a suspect's DNA and a forensic sample was merely random.[191] This holding was based, in part, on a decision to ignore the concerns about the product rule that three theoretical scientists ("a professor of ecology whose genetic research [concerned] fruit flies, . . . a biostatistician whose work [concerned] the mathematical use of statistics and . . . an environmental science professor whose genetic work principally dealt with nonhuman populations concerns") had raised on behalf of the defense.[192] The *Soto* court reasoned that, in contrast to the prosecution experts, the defense experts were not human population geneticists and thus did not belong to "the relevant, qualified scientific community."[193] The *Soto* court's restriction of the relevant scientific community to human population geneticists was both controversial among scientists and in conflict with decisions by other courts.[194]

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with which the match may have occurred randomly, the occurrence of the match is of little assistance to the trier of fact . . . ." (footnote omitted)). The issue is: what are the odds that the next random bystander tested could do as good a job of matching the evidence as the defendant?

United States v. Bonds, 12 F.3d 540 (6th Cir. 1993), dramatically illustrates the variations that occur in estimates of the statistical significance of a match. *See id.* at 540. In April 1989, the FBI's DNA laboratory estimated that there was only a 1 in 270,000 likelihood of a random match between the suspect's DNA and DNA recovered from a bloodstain at the crime scene. In May 1990, the same laboratory claimed that there was a 1 in 35,000 chance of a merely random match, thus finding the statistical significance of the match almost ten times lower than originally estimated. *See id.* at 551. The defendants claimed, however, that even the lower, revised estimate was indefensible because it was arrived at by using the product rule that was called into question by the 1992 NRC Report. While the defendants argued that the interim ceiling method proposed by the NRC showed that there was a 1 in 17 likelihood of a merely random match, the government contended that the probability was 1 in 6,200 even under the interim ceiling method. *See id.* at 552. *See also* Scheck, *supra* note 11, at 1991-92.

Wesley did not involve the issue of whether the *Frye* standard was satisfied by a particular technique for calculating the statistical significance of a match. Rather, Chief Judge Kaye criticized the hearing court for holding that concerns about statistical significance bore solely on the weight of the evidence and thus were not relevant to admissibility under *Frye*. *Wesley*, 83 N.Y.2d at 444. For an argument in support of Chief Judge Kaye's position that procedures for calculating the statistical significance of forensic DNA matches bear on the admissibility of testimony about forensic DNA and hence must pass the *Frye* test, see *infra* note 263.

191. *Soto*, 35 Cal. Rptr. 2d at 858. *But see infra* Part VI.A.2.

192. *Soto*, 35 Cal. Rptr. 2d at 855.

193. *Id.* at 855-56.

194. *See, e.g.*, Eric S. Lander & Bruce Budowle, *DNA Fingerprinting Dispute Laid to Rest*, 371 NATURE 735, 737 (1994). ("The NRC . . . has concluded that the best solution [to arriving at a method for calculating the statistical significance of forensic DNA matches] is to constitute another ad hoc committee on DNA fingerprinting, composed primarily of

Accordingly, new definitions of the requisite degree of acceptance and of relevant scientific communities are needed if *Frye* is to provide an adequate test for the admissibility of scientific evidence.[195] Prominent commentators have intimated that all proposed definitions must either be indeterminate, or achieve determinacy by departing from Frye's dictate of judicial deference to scientists.[196] According to the commentators, some theories and techniques can qualify as generally accepted only if judges incorporate their own substantive scientific judgments or personal biases in the definitions of the requisite degree of acceptance or the relevant scientific community.[197]

Notwithstanding these commentators' claims, it is possible to develop rational, scientifically uncontroversial criteria for requisite levels of acceptance and relevant scientific communities and to apply these criteria to reach determinate and justifiable admissibility decisions. To this end, Part V delineates necessary conditions for a reasonable judicial determination that a theory or technique is scientific and, hence, an admissible subject of testimony by scientific experts. Parts VI and VII build on these necessary conditions to define a family of revamped *Frye* standards whose application depends on whether the prosecution, a civil litigant or a criminal defendant seeks the admission of scientific evidence.

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statisticians and population geneticists."); United States v. Bonds, 12 F.3d 540, 556, 565 (6th Cir. 1993) (assuming that the pertinent scientific community for the purposes of evaluating statistical significance of forensic DNA matches is population geneticists); State v. Anderson, 881 P.2d 29, 41 (N.M. 1994) (holding that proffered experts, including the professor of ecology whom *Soto* disqualified, were all qualified because "[t]hey are all prominent in the field of either molecular biology, population genetics, statistics, or forensic DNA typing"); State v. Vandebogart, 652 A.2d 671, 677 (N.H. 1994) (assuming professor of ecology whom *Soto* disqualified was qualified to testify at *Frye* hearing).

195. *See also* People v. Leahy, 882 P.2d 321, 328-29 (Cal. 1994) (reasoning that vagueness in the definitions of the requisite degree of acceptance and of the relevant scientific community created a need for "clarifying or modifying our *Kelly* standard [California's label for the *Frye* standard] for use in future cases," but did not point to "fundamental defects in our approach in *Kelly*"). The *Leahy* court proceeded to hold that, *Daubert* notwithstanding, the *Frye* (labeled *Kelly*) standard continued to govern in California courts. *Id.* at 323, 331.

196. *See* Black et al., *supra* note 2, at 726-27.

197. Thus, Black et al. argue that the widespread criticisms of *Frye* show that: "[D]eferring to the scientific community does not really eliminate choices about science. A court applying *Frye* has to decide what must be accepted, what constitutes the relevant field of science, and what demonstrates acceptance, and making these decisions requires the very understanding of science that *Frye* ostensibly avoids." Black et al., *supra* note 2, at 726-27 (footnotes omitted). These authors' definition of what must be accepted is the same as Black's earlier definition of this issue. *See* Black, *supra* note 77, at 629-30. This Article's proposals for distinguishing between relevant and irrelevant scientific communities also solve this problem.

V. NECESSARY CONDITIONS FOR A REASONABLE JUDICIAL DETERMINATION THAT A THEORY OR TECHNIQUE IS SCIENTIFIC

A. Dual Iterated Disinterested Acceptance

An adequate standard must both distinguish between admissible and inadmissible expert testimony and measure acceptance of the theory or technique in question (not merely acceptance by its proponents). Scientists in the same laboratory depend on the reception of each other's work for their reputations, even if they have not collaborated on particular work.[198] At a minimum, then, the requisite acceptance must come from a non-collaborator outside the laboratory where the theory or technique was developed.[199]

However, acceptance by a disinterested person is not sufficient, because a judge may reasonably conclude that a theory or technique is

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198. Others argue that because the developer of a particular forensic procedure knows most about it, he or she is particularly well qualified to establish that the procedure is "generally accepted." *See, e.g.*, People v. Young, 391 N.W.2d 270, 289-90 (Mich. 1986) (Boyle, J., dissenting); Heine, *supra* note 175, at 157-62.

This Article's grounds for rejecting this argument are similar to the majority's grounds in *Young*, that:

> If this Court were to adopt the view that the testimony of persons who have developed and whose reputation and livelihood depends on the use of a new technique alone supports admissibility, then the views of the developer and his disciples would be substituted for the scrutiny of the marketplace of general scientific opinion and the substance of the *Frye* test would be eliminated.

Young, 391 N.W.2d at 276 n.24. *See also id.* at 283 ("It is scientists not responsible for the original research that confirm its validity."); Jones v. United States, 548 A.2d 35, 42 (D.C. 1988) (stating that "a proffered expert's bias or incompetence" may stem from the fact that "[t]he expert . . . may be the principal proponent of a controversial technique").

199. In its amicus brief in *Leahy*, the California Attorney General similarly "argue[d] that general acceptance should mean that the technique 'is accepted by other well credentialed scientists outside the testing laboratory.'" *Leahy*, 882 P.2d at 329 (citation omitted).

The hearing and appellate courts in the *Yee* case also reasoned that it was crucial that the F.B.I.'s forensic DNA procedures be accepted by non-F.B.I. scientists. The Sixth Circuit stated that, "[t]he Government's experts, some of whom were from outside the FBI lab, clearly indicated that the FBI's DNA procedures were generally accepted." *Bonds*, 12 F.3d at 562. According to the *Yee* hearing court:

> [T]he crucial consideration . . . [is that] testimony solely by the developer of the novel technique almost never has been held to have shown that a procedure enjoys general acceptance. In this case, there is extensive testimony by experts other than F.B.I. employees . . . . This distinction is crucial, because the government's evidence does not simply stand alone and unsupported.

United States v. Yee, 134 F.R.D. 161, 200 (N.D. Ohio 1991).

scientific only if the disinterested person who accepts it is a member of the relevant scientific community. Judicial assessments of scientific merit can be avoided only if members of a proposed, relevant scientific community determine whether the putative disinterested acceptor of the theory or technique in fact belongs to their community. The separate danger of bias can be avoided only if, at a minimum, the disinterested acceptor is accepted as a member of the scientific community by a fellow member of the community who is neither a collaborator nor in the laboratory of the acceptor or the person(s) whose theory or technique is under consideration. This iterated disinterested acceptance requirement does not have the absurd consequence of requiring an infinite regression of scientists to testify as to others' membership in a research community. Rather, membership in the requisite community of mutually disinterested scholars can be established through "paper credentials," such as publications, receipt of competitively awarded funding for research, academic positions, or positions in scholarly associations or on panels reviewing grants.[200]

The requirement of iterated disinterested acceptance only partially ensures that a scientific community, rather than a mutual admiration society or cult, determines whether a theory or technique is admitted as scientific. The requirement would be satisfied if, for idiosyncratic reasons having nothing to do with science, one respected member of a relevant scientific community gave her imprimatur to a theory or technique. To prevent judicial determinations of scientific status from being based on such wholly idiosyncratic reasons, it seems reasonable to require acceptance from two non-collaborators in different laboratories, each of whom is neither a collaborator nor in the laboratory that developed the theory or technique at issue. Each of these disinterested acceptors must be acknowledged as a member of a relevant scientific community by a fellow member who is not affiliated with either of the acceptors nor with the person who developed the theory or technique.[201] Thus, a minimal condition for a reasonable judicial determination that a

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200. This implies that at hearings under this Article's revamped *Frye* standard, judges would be permitted to take judicial notice of publications, as well as considering the testimony of live witnesses. For convenience, however, this Article is written as if, in determining whether the revamped standard is satisfied, judges will be considering live testimony alone. *See, e.g.*, Scheck, *supra* note 11, at 1959 & 1959 n.3 (arguing for consideration of scientific literature, as well as live witnesses, in determining whether there is general acceptance of a theory or technique); *Leahy*, 882 P.2d at 336.

201. The California Supreme Court has recognized the danger of allowing a single person to establish that there is scientific acceptance of a theory or technique. See *Leahy*, 882 P.2d at 336 ("[I]t [is] questionable whether the testimony of a single witness alone is ever sufficient to represent, or attest to, the views of an entire scientific community regarding the reliability of a new technique." (citing People v. Kelly, 549 P.2d 1240, 1248 (Cal. 1976))).

theory or technique is scientific is *dual* iterated disinterested acceptance.[202]

B. Minimal Conditions for Relevant Scientific Communities

We have seen that due to intellectual specialization, the scientific status of a theory or technique depends on acceptance by a specialized research community, not on acceptance by scientists per se. To find the requisite dual iterated disinterested acceptance, a judge must find that the scientific community to which the disinterested acceptors of the theory or technique and the disinterested acceptor of the acceptors belong is in fact a relevant research community.

To solve the problem of judges relying on their subjective judgment when defining the relevant research community, the *Frye* standard can be modified to include three scientifically noncontroversial requirements that distinguish between relevant and irrelevant scientific communities. These scientific community requirements and the requirement of dual iterated disinterested acceptance together form the core of this Article's revamped *Frye* standard.

1. "Scientists, Not Technicians"

Frye's dictate of judicial deference to scientists implies, first, that a relevant scientific community must consist of "scientists, not technicians."[203] Scientists do not decide on the scientific status of a technique if judges consider acceptance by law enforcement officers or others whose professional concern is applying a technique (not explaining or

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202. Under this Article's revamped *Frye* standard, an implication of the requirement of dual iterated disinterested acceptance is that a relevant scientific community must be broader than a group of scientists tied together through collaborations or membership in the same laboratories. This breadth requirement nonetheless fails to eliminate the possibility that even in the absence of direct affiliations, all or most members of a research community will share biases produced by similar training, and social and academic class positions and interests. The preceding critique of falsifiability implies, however, that neither judges nor anyone else has access to a rational, impartial standpoint from which to decide how much of science reflects the operation of non-scientific biases. In addition to implying that acceptance by the current scientific community is the sole rational basis for judicial determinations of the scientific status of work, the critique of falsifiability implies that it is beyond judicial competence to second-guess science by uncovering its biases. In other words, the philosophical demand for deference to practicing scientists demands deference to science as practiced, biases and all. For discussion of the biases of science, see FEYERABEND, *supra* note 121, at 146, and FEYERABEND, *supra* note 114, at 303-04.

203. *Young*, 391 N.W.2d at 275.

considering why the technique does or does not work) to be sufficient.[204] The Michigan Supreme Court correctly recognized in *People v. Young* that "a theoretical understanding is essential" for membership in a relevant scientific community.[205] The iterated acceptance criterion advanced above expresses an additional requirement stemming from the philosophical need to avoid judicial assessments of scientific merit. Scientists, not judges, must determine whether a person belongs to a research community concerned with the theoretical underpinnings of given work. To defer to these determinations, judges must rely on the credentials and other signs of recognition that scientists use to award achievement. The basic principle was articulated by the California Supreme Court in *People v. Brown*: "The witness [establishing that the *Frye* standard is satisfied] must have academic and professional credentials which equip him to understand both the scientific principles involved and any differences of view on their reliability."[206] The *Brown* court correctly applied this principle to rule that "competent and well-credentialed forensic technicians . . . [who had an] identification with law enforcement . . . and [a] lack of formal training and background in the applicable scientific disciplines" could not establish that aged stain typing tests were admissible under *Frye*.[207]

2. Scientists' Livelihoods Cannot Be "Intimately Connected" with the Proffered Technique

As the example of forensic DNA analysis shows, competing laboratories may develop and apply alternate versions of the same technique.[208] Absent collaboration, scientists in all the laboratories may

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204. *See also Kelly*, 549 P.2d at 1250 ("In considering the position of the scientific community, a court is found [sic] to let scientists speak for themselves. [The law enforcement officer's] undoubted qualifications as a [voiceprint analysis] technician . . . do not necessarily qualify him as a scientist to express an opinion on the question of general scientific acceptance.")

205. Reasoning that "[b]ecause a theoretical understanding is essential, the relevant scientific community is scientists, not technicians," the *Young* court ruled that the prosecution could not use the testimony of "three . . . technicians, two of whom were full-time employees of law enforcement agencies" to establish that electrophoresis of evidentiary bloodstains passed the *Frye* test. *Young*, 391 N.W.2d at 275.

206. 709 P.2d 440, 448 (1985), *rev'd on other grounds sub nom.* California v. Brown, 479 U.S. 538 (1987).

207. *Id.* at 450. *See also* People v. Leahy, 882 P.2d 321, 334 (Cal. 1994) (reasoning that because they were not "qualified to relate the scientific bases underlying the nystagmus test," police officers who administered the test were not "competent to establish general acceptance of HGN testing in the scientific community").

208. *See* People v. Wesley, 633 N.E.2d 451, 463-64 (N.Y. 1994) (explaining that three commercial laboratories — Lifecodes, Cetus, and Cellmark — were conducting forensic DNA analysis in the United States as of 1988 and that the F.B.I. established its own

be financially and professionally affected by whether one laboratory's procedures are judged to have attained sufficient scientific stature to merit admissibility.

To avoid basing admissibility on the personal interests of scientists, courts must prevent interested proponents of a technique from establishing its scientific acceptability. The exclusion does not simply repeat the disinterested acceptance requirement advanced above, since the requisite exclusion extends even to forensic scientists who are neither collaborators nor members of the laboratory whose version of a technique is at issue. In her concurrence in *People v. Wesley*, Chief Judge Kaye implicitly recognized that interested proponents of a technique cannot belong to a relevant scientific community, even when such proponents are not collaborators or members of the same laboratory. She noted that both experts had "commercial interests in the work under consideration and both [were] the primary developers and proponents of the technique."[209] She thus concluded that neither the opinion of the Director of Forensics at Lifecodes's laboratory nor the opinion of the founder of Cellmark, a rival commercial forensic DNA laboratory, could be used to establish the admissibility of Lifecodes's procedures for declaring a forensic DNA match.[210]

It is unreasonable, however, to require that relevant scientific communities exclude all forensic scientists involved in developing and applying the technique at issue. Hands-on experience may make forensic scientists distinctively aware of the issues involved in extending a theory or technique to a forensic setting.[211] Accordingly, the danger of personal interests affecting judgments of admissibility must be balanced against the danger of ignoring the opinions of those especially equipped to understand the scientific issues in question. In *People v. Young*, the Michigan Supreme Court appropriately balanced these dangers. The *Young* court reasoned that "a certain degree of 'interest' must be tolerated if scientists familiar with the theory and practice of a new

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forensic DNA laboratory in 1989); United States v. Yee, 134 F.R.D. 161, 202-03 (N.D. Ohio 1991) (discussing the forensic DNA laboratory operated by one of the prosecution witnesses testifying in support of the F.B.I.'s procedures for forensic DNA analysis); People v. Soto, 35 Cal. Rptr. 2d 846, 849 (Ct. App. 1994) (noting that the Orange County Sheriff's Department laboratory analyzed forensic DNA evidence in the case), *review granted*, 890 P.2d 1115 (Cal. 1995).

209. *Wesley*, 633 N.E.2d at 465.

210. *See id.* at 464-65.

211. Similarly, the Michigan Supreme Court reasoned that even though "the relevant scientific community is scientists not technicians[,] . . . [p]ractical experience with the process . . . is also necessary. Ideally the community would be scientists with direct empirical experience with the procedure in question." People v. Young, 391 N.W.2d 270, 274 (Mich. 1986).

technique are to testify at all."[212] Therefore, a scientist belongs to a relevant scientific community only if his or her "livelihood [is] not intimately connected with the new technique."[213]

The *Young* court applied this criterion to distinguish between forensic scientists whose professional and economic interests could not be significantly divorced from the success of a technique and those with significant outside sources of professional status and livelihood. Thus, in deciding whether electrophoresis of evidentiary bloodstains passed the *Frye* test, the *Young* court excluded the opinions of the director of the F.B.I. serology laboratory and the police detective who performed the electrophoresis in the case.[214] However, it ruled that the relevant scientific community included a professor of public health at the University of California at Berkeley who had conducted electrophoresis studies of dried bloodstains, even though this prosecution witness had been a paid consultant with the Oakland Crime Laboratory and thus could not be deemed to offer absolute neutrality with regard to the technique.[215] The court also ruled that the relevant scientific community included a retired academic biochemist who was a recognized leader in developing electrophoresis to test body-fluid enzymes for forensic purposes.[216] Even though he was an unpaid consultant to a crime laboratory at the time of the *Frye* hearing[217] and was "[a]rguably . . . still seeking to vindicate his original position [that a particular bloodstain

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212. *Id.* at 275.

213. *Id.* at 276-77 (citing People v. Barbara, 25 N.W.2d 171, 180 (Mich. 1977) and People v. Tobey, 257 N.W.2d 537, 539 (Mich. 1977)). Similarly, the court in *Daubert II* reasoned that:

> One very significant fact to be considered [in determining whether proposed expert testimony concerns good science] is whether the experts are proposing to testify about matters growing naturally and directly out of research they have conducted independent of the litigation, or whether they have developed their opinions expressly for purposes of testifying. That an expert testifies for money does not necessarily cast doubt on the reliability of his testimony, as few experts appear in court merely as an eleemosynary gesture. But in determining whether proposed expert testimony amounts to good science, we may not ignore the fact that a scientist's normal work-place is the lab or the field, not the courtroom or the lawyer's office.

Daubert II, 43 F.3d at 1317.

For disagreement with this Article's endorsement of the *Young* court's position, see Heine, *supra* note 175, at 157-62, endorsing Judge Boyle's dissenting position in *Young*, 391 N.W.2d at 290 n.11, that *Frye* may be satisfied even "where those who have developed the technique and whose reputation and livelihood depend on use of the new technique alone certify the validity of the technique."

214. *See Young*, 391 N.W.2d at 275-76.

215. *See id.*

216. *See id.*

217. *See id.* at 275.

analysis system should be discontinued],"[218] he was allowed to testify on behalf of the defense.[219]

Similarly, the hearing court in *United States v. Yee* recognized that a government witness who operated a forensic DNA laboratory "no doubt hopes that the ruling in this case will favor the F.B.I., as it will provide a judicial imprimatur to his own program."[220] Nonetheless, the *Yee* court accepted the testimony of the witness, stating:

> Though Dr. Caskey may currently be within the community of forensic DNA scientists, he remains, as he was at the time that he was making his decision to adopt the F.B.I. protocol, a pre-eminent academic and clinician. His views, accordingly, reflect those of someone who may be viewed as being both 'inside' and 'outside' the forensic community.[221]

The *Yee* court reasoned that as both an insider and an outsider, Dr. Caskey belonged to the relevant scientific community for determining whether the F.B.I.'s forensic DNA procedures passed the *Frye* test.

3. Scientists Must Have a "Reasonably Comprehensive Understanding" of the Forensic Issues.

Besides including forensic scientists whose "livelihood [is] not intimately connected with the new technique,"[222] relevant scientific communities may also include scientists whose work concerns only related nonforensic techniques and/or the theoretical foundations of the forensic technique at issue. However, these theoretical scientists must have "a reasonably comprehensive understanding" of the forensic science issues.[223] This accords with the decision in *Young* to consider the opinions of "nonforensic scientists using electrophoresis who are capable of evaluating the reliability of electrophoresis of evidentiary

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218. *Id.*
219. *See id.* at 277.
220. 134 F.R.D. 161, 202-03 (N.D. Ohio 1991).
221. *Id. But see Daubert II*, 43 F.3d at 1317 n.5:
> Fingerprint analysis, voice recognition, DNA fingerprinting and a variety of other scientific endeavors closely tied to law enforcement may indeed have the courtroom as a principal theatre of operations. As to such disciplines, the fact that the expert has developed an expertise principally for purposes of litigation will obviously not be a substantial consideration.

222. *Young*, 391 N.W.2d at 276.
223. *Yee*, 134 F.R.D. at 195.

bloodstains if presented with the information they need to fill the gaps in their own knowledge and experience."[224]

The requirement that theoretical scientists have a reasonably comprehensive understanding of the forensic science issues is also implicit in Chief Judge Kaye's concurrence in *Wesley*[225] and the hearing court's opinion in *Yee*.[226] As previously noted, Judge Kaye excluded three theoreticians from the relevant scientific community on the ground that they were unfamiliar with issues peculiar to forensic, as opposed to diagnostic, DNA analysis.[227] Consistent with this, the *Yee* court reasoned:

> To the extent that the government seriously intended to contend that scientists from the broader fields of molecular biology and population genetics, including theorists in those fields, were not credible if they had not had experience with the forensic applications of DNA and genetic theories, I reject that contention. . . . [T]he scientific community to which we must turn in order to assess whether general acceptance has been attained is composed of scientists from the fields of molecular biology and population genetics who have expertise in either or both of those fields and a reasonably comprehensive understanding about the F.B.I.'s DNA testing protocol and procedures.[228]

The *Yee* hearing court thus held that the relevant scientific community included theoretical scientists who had a reasonably comprehensive understanding of the F.B.I.'s procedures for conducting forensic DNA analysis.[229]

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224. *Young*, 391 N.W.2d at 271.
225. People v. Wesley, 633 N.E.2d 451, 461 (N.Y. 1994) (Kaye, C.J., concurring).
226. *Yee*, 134 F.R.D. at 161.
227. *See supra* note 190 and accompanying text.
228. *Yee*, 134 F.R.D. at 195.
229. *Id.*

VI. FRYE'S CORE PHILOSOPHICAL DICTATE PRECLUDES FURTHER JUDICIAL ASSESSMENTS OF SCIENTISTS' QUALIFICATIONS

A. Judges Should Not Attempt to Identify the Relevant Scientific Community

Although Part V borrowed from cases interpreting the *Frye* standard to delineate minimal acceptance and relevance conditions, a major departure from prevailing interpretations is needed. Literally interpreting *Frye*'s call for "general acceptance in the particular field in which it belongs,"[230] virtually all judges and scholars assume that *Frye* assigns judges the task of defining the relevant scientific community.[231] To the contrary, judges should only distinguish between relevant and irrelevant scientific communities and not identify any one group of scientists as the relevant community. A judicial distinction between relevant and irrelevant communities is required because intellectual specialization renders some theories and techniques outside the purview of some witnesses willing to testify at admissibility hearings. Yet, the domains of inquiry of particular scientific fields or subspecialties overlap. Judges seeking to determine whether the relevant scientific community consists of scientists in some or all of the subspecialties concerned with a particular theory or technique will be tempted, if not forced, to take controversial scientific positions or to resort to personal biases. Accordingly, to adhere to *Frye*'s dictate of judicial deference to scientific opinion, courts should not attempt to identify any one group of scientists as the relevant scientific community.

In assessing scientists' qualifications for evaluating a theory or technique, judges should only consider whether a proposed witness belongs to a scientific community that satisfies the minimal relevance conditions delineated above. Therefore, the court initially determines whether a proposed witness is accepted by insiders as a member of a field concerned with scientific, rather than purely technical, questions about the theory or technique at issue. Next, the court must consider

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230. Frye v. United States, 293 F. 1013, 1014 (D.C. Cir. 1923).

231. *See, e.g.*, People v. Wesley, 633 N.E.2d 451, 463 (N.Y. 1994) (Kaye, C.J., concurring) ("In defining the relevant scientific field, the court must seek to comply with the *Frye* objective of obtaining a consensus of the scientific community." (citation omitted)); United States v. Williams, 583 F.2d 1194, 1198 (2d Cir. 1978) ("Selection of the 'relevant scientific community,' appears to influence the result."); *Yee*, 134 F.R.D. at 195; People v. Leahy, 882 P.2d 321, 336 (Cal. 1994); People v. Young, 391 N.W.2d 270, 274-77 (Mich. 1986); United States v. Downing, 753 F.2d 1224, 1236 (3d Cir. 1985); Majmudar, *supra* note 176, at 198; Giannelli, *supra* note 46, at 1209.

whether the witness' "livelihood [is or is] not intimately connected with the new technique."[232] Finally, when the proferred witness is a theoretical scientist, the court considers whether he or she has "a reasonably comprehensive understanding" of the forensic science issues.[233] A proferred witness counts as a member of a relevant scientific community if and only if he or she passes these three tests. At this point, a judge's inquiry into the relevance of scientific communities is properly concluded.

1. A Critical Analysis of *People v. Young*

A critical analysis of the Michigan Supreme Court's contrasting position in *People v. Young*[234] reveals the rationale of this position. The *Young* court mistakenly assumed the task of defining the relevant scientific community,[235] holding that the relevant community could not consist of forensic scientists alone, but must also include theoreticians.[236]

However, *Young* should have held that any group of forensic and/or theoretical scientists constituted a relevant scientific community so long as its members could identify electrophoresis as a subject of their research and could satisfy our three minimal relevance conditions. Even if several distinct groups of scientists had been deemed relevant scientific communities under these tests, a decision on the admissibility of electrophoresis of evidentiary bloodstains could and should have been reached without assessing the relative qualifications of these distinct scientific groups.

The problems with the *Young* court's approach lie with its criterion for identifying the relevant scientific community. The court reasoned that because the relevant scientific community must be "large enough to obtain an adequate sampling of scientific opinion,"[237] the community determining acceptability could not consist solely of relatively disinter-

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232. *Young*, 391 N.W.2d at 276.
233. *Yee*, 134 F.R.D. at 195.
234. 391 N.W.2d 270 (Mich. 1986).
235. *See id.* at 274-77.
236. *See id.* at 276-77.
237. *Id.* The *Young* court's requirement of a community "large enough to obtain an adequate sampling of scientific opinion" was intended to echo Giannelli's position that a relevant scientific community must be "sufficiently large so that the *Frye* objective of receiving a consensus judgment of the scientific community can be satisfied." *Id.* (citing Giannelli, *supra* note 46, at 1209). Both formulations are, in turn, closely related to the California requirement that courts consider "the views of a typical cross-section of the scientific community . . . ." People v. Leahy, 882 P.2d 321, 336 (Cal. 1994) (quoting People v. Kelly, 549 P.2d 1240 (Cal. 1976)). This Article's criticism of the Young court's requirement accordingly also argues against the requirements advanced by Giannelli and the California courts.

ested forensic scientists. It must also include "nonforensic scientists using electrophoresis . . . [who] have sufficient theoretical understanding and practical experience to be able to evaluate the evidence."[238] Thus, *Young* held that neither the relatively disinterested forensic scientists nor the forensically informed theoreticians could provide, by themselves, "an adequate sampling of scientific opinion."[239]

Young's holding would be justified if neither group was by itself qualified to resolve the underlying scientific dispute about electrophoresis. However, a position on the merits of this scientific dispute appears necessary for any rational judicial assessment of the qualifications of these groups. Therefore, *Young*'s "adequate sampling" criterion contravenes *Frye*'s philosophical core.

Young might be interpreted to require judges to examine only the size of a group. However, *Young*'s criterion is not viable even with this interpretation. While relevant scientific communities can justifiably be required to satisfy a size criterion, a justifiable size criterion will not necessarily select only one group of scientists as the relevant community for determining the scientific status of a theory or technique.

A size criterion is implicit in our dual iterated disinterested acceptance test. Under this requirement, a judge can reasonably determine that a theory or technique is scientific only if it is accepted by a community that extends beyond a network of collaborators or members of the same laboratories. Hence, once the *Young* court determined that groups of both forensic and nonforensic scientists could render disinterested, scientifically informed opinions about electrophoresis of evidentiary bloodstains, the next proper judicial inquiry was whether either of these groups, or only the combined group of forensic and nonforensic scientists, had a sufficient number of mutually independent scholars to satisfy the requirement of dual iterated disinterested acceptance.

If more than one of these groups had been found to have the requisite breadth, the *Young* court should have resolved the admissibility question without deciding which group was the relevant scientific community.[240] Since time may uphold the judgments of scientific majorities but sometimes vindicates those of scientific minorities,[241] the sheer number of scientists in a community cannot justify a judicial

238. *Young*, 391 N.W.2d at 276-77.

239. *Id.*

240. This Article's proposed criteria will enable judges to determine whether particular evidence is admissible without identifying any one community as the relevant scientific community. *See infra* Part VII.

241. *See supra* Parts III.D-E.

decision to defer to that community.[242] Therefore, *Young*'s adequate sampling criterion faces a dilemma. On one hand, the criterion may fail to identify one single group of scientists as the relevant community. On the other hand, misidentification may occur because the adequate sampling criterion is filled in with misconceptions about the practice of science (e.g., the notion that a majority of scientists always accept new truths) or with judges' substantive scientific understanding or personal biases. Thus, *Frye*'s philosophical core is likely to be subverted if judges attempt to identify a single relevant scientific community.

2. A Critique of *People v. Soto*

The decision of a California appellate court in *People v. Soto*[243] provides an additional caution against judicial attempts to identify a single relevant scientific community. As indicated previously,[244] the *Soto* court held that the *Frye* standard was satisfied by the "product rule," a statistical method for calculating the likelihood that a match between a suspect's DNA and a forensic sample was merely random.[245] This holding was based on the court's determination that only human population geneticists belonged to the relevant scientific community.[246] Therefore, the *Soto* court found that a biostatistician and two geneticists whose research concerned nonhuman populations were not members of the relevant scientific community, thereby excluding these defense witnesses' doubts about the product rule's ability to account for the possibility of substructuring within the human population.[247]

One of the court's justifications for ignoring the concerns raised by the defense researchers of nonhuman populations was that "[m]ost importantly, the scientists themselves now proclaim, 'the DNA finger-printing wars are now over.'"[248] However, *Soto* based its claim about the end of the scientific controversy on an article by Lander and Budowle[249]

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242. A further complication that weighs against judicial definition of the relevant scientific community is that scientific communities of varying sizes exist for different purposes (e.g., biology departments, virology conferences, the group that exchanges manuscripts about retroviruses). *See* KUHN, *supra* note 13, at 177-78.

243. 35 Cal. Rptr. 2d 846 (Ct. App. 1994), *review granted*, 890 P.2d 1115 (Cal. 1995).

244. *See supra* notes 191-94 and accompanying text.

245. For further description of the product rule, see United States v. Yee, 134 F.R.D. 161, 173 (N.D. Ohio 1991); Lander & Budowle, *supra* note 194.

246. *See Soto*, 35 Cal. Rptr. 2d at 855-56.

247. *See id.* For explanations of substructuring, see, e.g., State v. Anderson, 881 P.2d 29, 39-40 (N.M. 1994). *See also* Scheck, *supra* note 11, at 1970-71, 73.

248. *Soto*, 35 Cal. Rptr. 2d at 857.

249. *See* Lander & Budowle, *supra* note 194.

that other scientists criticized[250] and that was discredited by the National Research Council's ("NRC") 1996 report on forensic DNA analysis.[251]

Even if this criticism is discounted, the Lander and Budowle article shows that by restricting the relevant scientific community to human population geneticists, the *Soto* court parted from mainstream scientific opinion. In reporting that the NRC had recommended the formation of a committee to arrive at an agreed method for calculating the statistical significance of matches, Lander and Budowle did not quarrel with the NRC's proposal that the committee be composed primarily of statisticians and population geneticists.[252] Similarly, Lander and Budowle did not argue with the NRC's recommendations, subsequently enacted in the DNA Identification Act of 1994,[253] for a permanent advisory board on forensic DNA.[254] The Act specifies that scientists on the board be molecular biologists and population geneticists unaffiliated with forensic laboratories,[255] but does not incorporate the *Soto* court's requirement that the population geneticists' work concern human populations.[256]

The *Soto* court's exclusion of the defense witnesses from the relevant scientific community also effectively disallowed any testimony based on the work of Richard C. Lewontin.[257] In 1990, during the *Frye*

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250. *See, e.g.*, Letters from R.C. Lewontin and Daniel L. Hartl, in *Correspondence: Forensic DNA Typing Dispute*, 372 NATURE 398 (1994); Letters from J.H. Edwards et al., in *Correspondence: More on DNA Typing Dispute*, 373 NATURE 98 (1995).

251. *See* NATIONAL RESEARCH COUNCIL, THE EVALUATION OF FORENSIC DNA EVIDENCE (1996) [hereinafter 1996 NRC REPORT].

While Lander and Budowle argued that the NRC's 1992 report had ended all controversy about the statistical and population genetics issues, in 1996 the NRC severely criticized and urged the abandonment of the ceiling and interim ceiling principles that the 1992 report proposed for calculating the statistical significance of forensic DNA matches: "We share the view of those who criticize [the ceiling principles] on practical and statistical grounds and who see no scientific justification for its use. . . . Our view is that sufficient data have been gathered to establish that neither [the ceiling nor interim ceiling] principle is needed." *Id.* at 0-27, 5-31, 5-32. *See also* Scheck, *supra* note 11, at 1963 (noting that the 1992 NRC Report "did not resolve the debate among population geneticists about what methodology should be used to calculate DNA profile frequencies").

For further information on the continuing controversy over this subject, see, e.g., Bruce S. Weir, *Invited Editorial: The Second National Research Council Report on Forensic DNA Evidence*, 59 AM. J. HUM. GENET. 497 (1996).

252. *See* Lander & Budowle, *supra* note 194, at 738.

253. 42 U.S.C. §§ 14131-34 (1994).

254. *See* Lander & Budowle, *supra* note 194, at 738.

255. *See* 42 U.S.C. § 14131(a)(1)(B) (1994). The recommendation that the board's scientists not be affiliated with forensic laboratories accords with this Article's recognition that all forensic DNA scientists have professional and economic stakes in acceptance of the technique.

256. *See id.*

257. *See* People v. Soto, 35 Cal. Rptr. 2d 846 at 851, 857 (Ct. App. 1994), *review granted*, 890 P.2d 1115 (Cal. 1995); *see also* United States v. Yee, 134 F.R.D. 161, 174 (N.D. Ohio 1991).

hearing in *United States v. Yee*,[258] court-appointed expert Eric Lander, who subsequently co-authored the paper proclaiming the death of the forensic DNA wars, testified that Dr. Lewontin was "probably regarded as the most important intellectual force in population genetics alive."[259] A government witness similarly testified that it was "fair to say that Dr. Lewontin of Harvard was one of the pre-eminent theoreticians in the area of molecular population as early as the 1960s."[260] Neither these two witnesses nor the *Yee* court had any doubt that Dr. Lewontin's expertise extended to the statistical issues in forensic DNA research.

The *Soto* court's decision to disregard testimony based on Dr. Lewontin's work was justified only if the court was more qualified to assess the statistical issues than one of the world's pre-eminent experts. Moreover, the decision was justified only if the court was more qualified than scientists themselves (including Dr. Lander, paradoxically) to assess Dr. Lewontin's scientific qualifications. These assumptions about judicial competence violate *Frye*'s insight that scientists' opinions provide the sole rational basis for deciding on the scientific status of work. Since the *Soto* court made these assumptions in an attempt to identify a uniquely relevant community, and led the *Young* court to make comparable assumptions, a revamped *Frye* standard should not assign judges the task of defining a single relevant scientific community. Instead, judges should only apply the minimal relevance conditions of Part V.B.

B. Judges Should Not Assign Relative Weights to Opinions Within a Relevant Scientific Community

Frye's philosophical core is also likely to be subverted if judges determine admissibility by assigning relative weights to opinions within a relevant scientific community. If scientists' assessments of a theory or technique are to determine admissibility, the judicial task must be restricted to "counting scientists' votes"[261] within a relevant community. The hearing court's decision in *Yee*[262] illustrates the pitfalls of judicial

258. *See Yee*, 134 F.R.D. at 164.
259. *Id.* at 181.
260. *Id.*
261. People v. Wesley, 633 N.E.2d 451, 464 (N.Y. 1994) (Kaye, C.J., concurring) (citations omitted). *See also* Scheck, *supra* note 11, at 1959 (stating that properly interpreted, *Frye* restricts judges to "counting noses").
262. United States v. Yee, 134 F.R.D. 161 (N.D. Ohio 1991).

assessment of "the quality, as well as quantity"[263] of scientific acceptance and rejection.

Unlike the *Soto* court,[264] the *Yee* hearing court included defense expert Dr. Lewontin in the relevant scientific community for determining whether the *Frye* standard was satisfied by the F.B.I.'s procedures for calculating the statistical significance of forensic DNA matches. The *Yee* court also held that the relevant community included court-appointed expert Dr. Lander. Although he later contended that the scientific controversy about forensic DNA had been "laid to rest"[265] at the time of the *Yee* hearing, Dr. Lander agreed with Dr. Lewontin that "any estimate of probability that might be generated on the basis of the [F.B.I.'s] Caucasian database was too speculative to be accepted scientifically."[266] However, *Yee* held that the F.B.I.'s procedures for calculating statistical significance satisfied the *Frye* test.[267] This holding was explicitly based

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263. People v. Leahy, 882 P.2d 321, 336 (Cal. 1994). The *Yee* hearing court's procedure accords with the California Supreme Court's recent definition, in *Leahy*, of the proper judicial inquiries under the *Frye* (relabelled *Kelly*) test. The *Leahy* court explained:

> Of course, the trial courts, in determining the general acceptance issue, must consider the quality, as well as the quantity, of the evidence supporting or opposing a new scientific technique. Mere numerical majority support or opposition by persons minimally qualified to state an authoritative opinion is of little value.

Id. at 336-37.

Our critique of *Yee* thus argues against the California Supreme Court's interpretation of *Frye* and supports the interpretation of Chief Judge Kaye of the New York Court of Appeals. *See Wesley*, 633 N.E.2d at 461 (Kaye, C.J., concurring).

264. *See supra* text accompanying notes 255-58.

265. Lander & Budowle, *supra* note 194, at 738.

266. *Yee*, 134 F.R.D. at 174-75.

267. *See id.* at 202, 204-06. The *Yee* hearing court's determination that the F.B.I.'s procedures for calculating the statistical significance of forensic DNA matches were admissible under *Frye* rested on a prior determination that concerns about statistical significance bear on the admissibility of testimony about forensic DNA matches. *See Yee*, 134 F.R.D. at 180-81, 197. The *Yee* court reasoned that "[w]ithout the probability assessment, the jury does not know what to make of the fact that the patterns match; the jury does not know whether the patterns are as common as pictures with two eyes, or as unique as the Mona Lisa." *Id.* at 181.

In contrast, the Sixth Circuit held in *Bonds* that the F.B.I.'s procedures for calculating the statistical significance of forensic DNA matches need not pass *Frye*'s general acceptance test because concerns about the statistical significance of forensic DNA matches bear only on the weight of the evidence. *See* United States v. Bonds, 12 F.3d 540, 564-65 (6th Cir. 1993). The *Bonds* court reasoned that these concerns deal with "the accuracy of the probability results," not with "whether the testimony was based on generally accepted (and scientifically valid) theories and procedures." *Id.* at 564-65. *See also* State v. Anderson, 881 P.2d 29, 44-46 (N.M. 1994).

The *Bonds* court's reasoning fails to take account of the major danger that juries will assume that a match between a suspect's characteristics and the characteristics of evidence recovered from a crime scene necessarily places the suspect at the crime scene. One source of this danger is that jurors may overlook the logical point that the extent to which

on a decision to afford overwhelming weight to the testimony of government expert Dr. C. Thomas Caskey.[268]

The problems with this decision are revealed by the *Yee* court's acknowledgment that Dr. Caskey's scientific stature did not rest on work in the relevant field of population genetics. "Dr. Caskey is principally a molecular biologist and geneticist, who, though familiar with the applicable theories and principles of population genetics, is not as expert in that area as Drs. Lewontin [and] Lander."[269] In contrast, the *Yee* court was aware of the enormous stature that other population geneticists accorded to Drs. Lander and Lewontin. Government witnesses testified that Dr. Lander was "a genius with whom it would be hard to argue"[270] and a "very prestigious and respected population geneticist in the field of human genetics."[271] In view of this testimony and the similar testimony about Dr. Lewontin discussed above,[272] the *Yee* court's decision to dismiss the doubts of Drs. Lewontin and Lander on the basis of Dr. Caskey's contrary opinions contravened the relevant scientific community's judgment of who was best qualified to issue an opinion, again abandoning *Frye*'s philosophical core.

The *Yee* court's additional holding that the F.B.I.'s procedures for declaring a match had gained general acceptance further violated *Frye*'s dictate of judicial deference.[273] The court rested this holding explicitly on "the relative professional standing of the prosecution witnesses and the defense witnesses,"[274] reasoning:

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identification is established by a match depends on the rareness of the characteristics matched. For criticism of the *Bonds* court for failing to see that jurors are likely to overlook this logical point, see, e.g., Black et al., *supra* note 2, at 728 n.63 & 798 n.540; Scheck, *supra* note 11, at 1992-93; *New Challenges of Scientific Evidence*, *supra* note 9, at 1537.

This Article's discussion of the dispute about the statistical significance of forensic DNA matches, *supra* notes 249-51 and accompanying text, implies that the *Bonds* court also overlooked a further source of danger: giving jurors the question of the extent to which a forensic DNA match establishes identification is asking them to resolve empirical questions, on which world-renowned scientists disagree, about how to construct data bases for estimating statistical significance. Thus, the relevant scientific disagreement does not simply involve the accuracy of given results, but the theoretical underpinnings for assessing the accuracy of results. In the terminology of Part III.B.2, factual disagreements about the statistical significance of forensic DNA matches rest on theoretical disputes about the nature of substructuring within human populations and, consequently, about how to construct adequate population data bases.

268. *See Yee*, 134 F.R.D. at 202-05.

269. *Id.* at 205.

270. *Id.* at 181.

271. *Id.*

272. *See supra* text accompanying notes 257-58.

273. *See Yee*, 134 F.R.D. at 202-04, 206.

274. *Id.* at 202.

> This is not to question for a moment the scientific competence of any of the witnesses, including the defense witnesses .... This finding simply reflects my judgment in the light of the entire record on the question of which of the experts is more likely to have a better general understanding of the level of general acceptance within the scientific community.[275]

By choosing to disregard the testimony of the defense witnesses even though their scientific competence with regard to the F.B.I.'s procedures for declaring a DNA match could not be questioned, the *Yee* court surreptitiously limited the relevant scientific community to prosecution experts. By doing this, the court was able to both acknowledge and avoid being influenced by the cogency of the defense experts' criticisms of the F.B.I.'s procedures for declaring a match.[276]

C. The Need for Head-Counting Principles

It follows that *Frye*'s dictate of deference to scientists requires severe restrictions on the discretion of courts to choose among scientists' opinions. Judges should distinguish between communities that do and do not satisfy the minimal relevance conditions of Part V. However, they should not ask whether the relevant community for determining the scientific status of a theory or technique is one of the groups satisfying these minimal conditions. Instead of evaluating various scientists' opinions, a court is only to count numbers of scientists within a relevant community who do or do not accept a theory or technique.

Principles for counting heads are necessary if these judicial inquiries are to justify the selective admission of evidence. Our proposed modifications can amount to a determinate legal standard only if we specify how much acceptance within a relevant scientific community is necessary for a theory or technique to count as scientific. We have seen, however, that in the guise of applying a single *Frye* test, courts have propounded varying definitions of how much acceptance constitutes general acceptance.[277] The vagueness of the term "general acceptance" has allowed judges to base admissibility decisions on their personal biases and/or opinions on the merits of substantive scientific disputes. This ambiguity and lack of uniformity can be corrected.

As argued in Part V, dual iterated disinterested acceptance within a relevant scientific community is necessary for a reasonable judicial

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275. *Id. Accord* United States v. Bonds, 12 F.3d 540, 557 (6th Cir. 1993).
276. *See Yee*, 134 F.R.D. at 206-07.
277. *See supra* notes 175, 180 and accompanying text.

determination that a theory or technique is scientific. However, work that satisfies these necessary conditions can enjoy widely varying degrees of acceptance among scientists. The conditions are met even if a theory or technique is accepted by only two independent members of a relevant scientific community and is rejected by all other researchers in both their own and all other relevant scientific communities. At the other extreme, the conditions are satisfied when virtually all researchers in a broad spectrum of fields accept a theory or technique. Parts VI.A and B argued that if judges go beyond Part V's minimal relevance conditions and make additional distinctions among scientists' opinions, they will be tempted, if not forced, to violate *Frye*'s requirement of deference to scientists. This danger does not arise, however, if admissibility is conditioned on acceptance from more than two independent members of a relevant scientific community — that is, on more than dual iterated disinterested acceptance. Therefore, in order to arrive at a determinate and justifiable legal standard, it is necessary to specify whether dual iterated disinterested acceptance within some relevant scientific community, or some additional amount of acceptance, is requisite for a theory or technique to be an admissible subject of testimony by scientific experts.

An adequate specification arises from recognizing that while acceptance by contemporary scientists is an imperfect indicator of scientific merit, it is also the only indicator on which judges (or any non-scientists) can rationally rely to decide on the scientific status of contemporary work. As previously noted, time sometimes upholds the judgments of scientific majorities but sometimes vindicates those of scientific minorities. Accordingly, no amount of current acceptance by scientists can ever guarantee future scientific acceptance. It follows that no more than dual iterated disinterested acceptance can rationally be deemed necessary for a theory or technique to count as scientific. Thus, if dual iterated disinterested acceptance exists within a relevant scientific community, a judge has minimally good reasons to conclude that a theory or technique is scientific.

At the same time, we have seen that the American legal system's treatment of scientific evidence makes sense only if scientists are assumed to be specially qualified to judge the merits of allegedly scientific procedures, reasoning, and claims. This assumption in turn implies that as acceptance increases beyond the minimum of dual iterated disinterested acceptance, a judge has better reasons to believe that a theory or technique is scientific. Accordingly, how much acceptance the law should require depends on whether the prerequisite for testimony by scientific experts should be minimally good judicial reasons, or some stronger basis, for according scientific status to a theory or technique.

An adequate answer involves a further, major departure from prevailing interpretations of *Frye*. Virtually all courts and scholars assume that regardless of which party seeks to admit evidence, there is one standard of general acceptance.[278] In contrast, on the basis of a consideration of the interests affected by the admission or exclusion of expert testimony, Part VII defines different requisite amounts of acceptance depending on whether the prosecution, a criminal defendant, or a civil litigant seeks to admit evidence. Thus, this Article's revamped *Frye* standard amounts to a three-tiered family of standards.

VII. A THREE-TIERED, PHILOSOPHICALLY REVAMPED STANDARD

A. The Legal Argument for Three Tiers

A three-tiered standard is justified by the American legal system's central normative assumption that different parties to litigation have interests of unequal weight. The Supreme Court articulated this assumption in *In re Winship*:[279] "The accused during a criminal prosecution has at stake interests of immense importance, both because of the possibility that he may lose his liberty upon conviction and because of the certainty that he would be stigmatized by the conviction."[280] According to *Winship*, the criminal defendant's liberty and reputation interests are of "transcending value" compared to the government's interest in convicting a guilty defendant.[281] Based on these asymmetrical interests, due process allows conviction of a criminal defendant only upon proof beyond a reasonable doubt.[282]

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278. *See infra* Part VII.

279. 397 U.S. 358 (1970).

280. *Id.* at 363.

281. *See id.* at 364. *See also id.* at 372 (Harlan, J., concurring). For the view that American law is overly protective of the interests of criminal defendants, see HAROLD J. ROTHWAX, GUILTY: THE COLLAPSE OF CRIMINAL JUSTICE (1996).

Many scholars contend that although formal American law accords greater weight to the criminal defendant's than to the government's interests, the opposite weighting is implicit in the informal process of plea bargaining. For the classic statement of this position, see HERBERT L. PACKER, *Two Models of the Criminal Process*, in THE LIMITS OF THE CRIMINAL SANCTION (1968). For an argument that this contrast between formal law and informal process is overly simple, see Adina Schwartz, *Who's the We?: Relations among the Citizenry, Criminal Defendants and the Police*, Presentation at the 46th Annual Meeting of the American Society of Criminology (Nov. 11, 1994), and Adina Schwartz, *"A Man's House Is His Castle": The Supreme Court's New Decision on the "Knock and Announce" Rule*, Presentation at the 47th Annual Meeting of the American Society of Criminology (Nov. 16, 1995).

282. *See Winship*, 397 U.S. at 364, 371-72 (Harlan, J., concurring)

In contrast, Justice Harlan's famed *Winship* concurrence asserted that the monetary interests on both sides of a civil litigation generally have equal value.[283] Accordingly, preponderance of the evidence is the appropriate standard of proof for civil cases.[284]

In addition to justifying the different standards of proof in criminal and civil trials, the law's differential valuation of litigants' interests is reflected in the fact that criminal defendants, unlike the prosecution or civil litigants, have Fifth and Sixth Amendment trial rights. In particular, the criminal defendant's "almost uniquely compelling" interests have been held in *Ake v. Oklahoma* to obligate States to provide expert witnesses to indigent criminal defendants.[285] In *Ake*, the Supreme Court reasoned:

> The private interest in the accuracy of a criminal proceeding that places an individual's life or liberty at risk is almost uniquely compelling
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> . . . .
>
> The State's interest in prevailing at trial — unlike that of a private litigant — is necessarily tempered by its interest in the fair and accurate adjudication of criminal cases.[286]

Ake held, on this basis, that once an indigent criminal defendant makes an ex parte showing that his or her sanity at the time of the offense is likely to be a significant trial issue, the state's interests in accurate proceedings obligate it to provide a psychiatrist to assist in the accused's defense.[287]

Various provisions of the Federal Rules of Evidence also reflect the view that the interests of a criminal defendant are paramount. For example, Rule 201(g) requires a court to instruct a jury in a civil case that they must accept judicially noticed facts as conclusive, but prohibits mandatory instructions in criminal cases.[288] In order to safeguard a criminal defendant's Sixth Amendment right to a jury trial, the proper

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283. *See id.* at 371 (Harlan, J., concurring).

284. *See id.*; *see also infra* note 333.

285. Ake v. Oklahoma, 470 U.S. 68, 78 (1985).

286. *Id.* at 78-79.

287. *See id.* at 83.

288. Rule 201(g) provides: "*Instructing jury.* In a civil action or proceeding, the court shall instruct the jury to accept as conclusive any facts judicially noticed. In a criminal case, the court shall instruct the jury that it may, but is not required to, accept as conclusive any fact judicially noticed." FED. R. EVID. 201(g).

instruction in a criminal case is that the jury may, but need not, accept judicially noticed facts.[289]

Similarly, under Federal Rule of Evidence 404(a), it is only on the initiative of a criminal defendant that character evidence may be used to prove that an individual acted in accord with her character on a particular occasion.[290] This special criminal defense prerogative is justified because "the criminal defendant stands in a position of great peril."[291]

It follows that different standards for when a theory or technique constitutes an admissible subject of expert scientific testimony should be applied depending on whether a criminal defendant's, the prosecution's,

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289. United States v. Gould, 536 F.2d 216 (8th Cir. 1976), explains:

> In the proposed Federal Rules of Evidence, forwarded by the Supreme Court of the United States to Congress on February 5, 1973, rule 201(g) did not draw this distinction between civil and criminal cases. The proposed rule 201(g) provided that '[t]he judge shall instruct the jury to accept as established any facts judicially noticed.' Congress disagreed with this unqualified rule requiring mandatory instructions in all cases. It was feared that requiring the jury to accept a judicially noticed adjudicative fact in a criminal case might infringe upon the defendants'[sic] Sixth Amendment right to a trial by jury. H.R. REP. NO. 650, 93d Cong., 1st Sess. 6-7 (1973), *reprinted in* 4 U.S.C.C.A.N. 7075, 7080 (1974). Consequently, Congress adopted the present text of rule 201(g) which requires a mandatory instruction in civil cases but a discretionary instruction in criminal cases.

Id. at 219 n.4. *See also* Advisory Committee's Note to Federal Rule of Evidence 201(g), 46 F.R.D. 161, 204 (1969) ("The considerations which underlie the general rule that a verdict cannot be directed against the accused in a criminal case seem to foreclose the judge's directing the jury on the basis of judicial notice to accept as conclusive any adjudicative facts in the case.").

290. Rule 404(a) provides in pertinent part:

> (a) *Character evidence generally.* Evidence of a person's character or a trait of character is not admissible for the purpose of proving action in conformity therewith on a particular occasion, except:
>
> (1) *Character of accused.* Evidence of a pertinent trait of character offered by an accused, or by the prosecution to rebut the same;
>
> (2) *Character of victim.* Evidence of a pertinent trait of character of the victim of the crime offered by an accused, or by the prosecution to rebut the same, or evidence of a character trait of peacefulness of the victim offered by the prosecution in a homicide case to rebut evidence that the victim was the first aggressor; . . . .

FED. R. EVID. 404(a).

291. Perrin v. Anderson, 784 F.2d 1040, 1044 (10th Cir. 1986). *See also* H. Richard Uviller, *Evidence of Character to Prove Conduct: Illusion, Illogic, and Injustice in the Courtroom*, 130 U. PA. L. REV. 845, 855 (1982) ("[T]he rule was relaxed to allow the criminal defendant with so much at stake . . . to tell the fact finder just what sort of person he really is.").

or a civil litigant's interests are at stake.[292] Specifically, the requisite amount of acceptance from scientists should increase depending on which type of party seeks the admission of testimony about a theory or technique.

B. A Critique of Alternatives

1. The Alleged Clash Between Criminal Defense Rights and *Frye*

A *Harvard Law Review* Case Comment[293] and the recent California Supreme Court case of *People v. Leahy*[294] both suggest that *Frye* may need to be jettisoned in order to safeguard the constitutional rights of criminal defendants.[295] According to the Comment: "Allowing a defendant to introduce exculpatory expert testimony despite the scientific community's misgivings may be a constitutional requirement in a system of justice committed to granting a defendant every opportunity to persuade a jury that there exists a reasonable doubt as to her guilt."[296] At the same time, these authors reason that because *Frye* recognizes the need for judicial deference to scientists, it is "a wise rule that contributes greatly to the integrity of the criminal process."[297] The authors write, "the *Frye* rule ensures that judges or juries with little or no scientific background will not attempt to resolve technical questions on which not

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292. Although the authors of *New Challenges of Scientific Evidence*, *supra* note 9, do not advocate a *Frye*-type standard, they do argue for making it easier for criminal defendants than for the prosecution or civil litigants to have scientific evidence admitted:

> [T]here are compelling reasons — in addition to ordinary due process considerations — for imposing a heavier burden on the prosecution than on the defendant in criminal cases. The defense often lacks the time, money or training necessary to conduct a proper inquiry into the evidence used against it at trial. Unlike parties in civil cases, defendants in criminal cases often lack the financial resources to hire their own experts. . . . [J]udges are often reluctant to authorize payment for experts when criminal defendants are indigent. . . . In contrast, prosecutors typically have access to both the financial and technical resources needed to obtain expert testimony. . . .
>
> Moreover, criminal defendants have less extensive discovery rights because they are usually not permitted to depose an opposing expert . . . .

Id. at 1529-30.

293. *Leading Cases*, *supra* note 170, at 125-27 (footnotes omitted).

294. People v. Leahy, 882 P.2d 321, 330 (Cal. 1994).

295. *See also* Heine, *supra* note 175, at 153-54 ("[D]ue process may require a different standard than general acceptance, thereby making the *Frye* test unconstitutional as applied to evidence offered by criminal defendants.").

296. *Leading Cases*, *supra* note 170, at 126-27 (footnotes omitted).

297. *Id.* at 127.

even experts can reach a consensus . . . ."[298] The Comment assumes that it is not possible to modify *Frye* to simultaneously defer to scientists' opinions and uphold criminal defendants' rights.

A similar assumption pervades the California Supreme Court's landmark opinion in *People v. Leahy*. The *Leahy* court held that, notwithstanding *Daubert*, the *Frye* test continues to govern in California courts.[299] That court noted that the *Harvard Law Review* Comment had "observe[d] that to the extent *Frye* excludes favorable defense evidence, it may be constitutionally deficient."[300] However, the court brushed away this criticism on the ground that "[t]he Harvard note is . . . predominantly favorable toward *Frye*."[301] Accordingly, although the *Leahy* court modified *Frye* by clarifying the requisite amount of acceptance, this failed to accord criminal defendants any special protection.[302] *Leahy*'s clarified standard was intended to apply across-the-board, regardless of which party seeks to admit scientific evidence.[303]

Both the Harvard Comment authors and the *Leahy* court failed to reconcile criminal defense rights with *Frye*'s dictate of judicial deference to scientists. A single general acceptance test should be replaced by a standard under which the requisite amount of acceptance depends on which party seeks the admission of evidence.[304]

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298. *Id.*

299. *See Leahy*, 882 P.2d at 323.

300. *Id.* at 330 (citing *Leading Cases*, *supra* note 170, at 125-27).

301. *Leahy*, 882 P.2d at 331.

302. *See id.* at 328-29, 336-37.

303. *See id.* at 336-37.

304. In considering whether *Frye* is consonant with criminal defense rights, both the *Harvard Law Review* Comment and *Leahy* assume that *Frye*'s general acceptance test is a conservative standard for the admission of evidence. Doubts have been raised, however, about whether *Frye* has actually been a conservative standard as applied. *See, e.g.*, United States v. Yee, 134 F.R.D. 161, 202 (N.D. Ohio 1991) (noting "the reluctance with which reviewing courts have found a want of general acceptance in the scientific community"); Black et al., *supra* note 2, at 740-41.

Instead of criticizing or praising *Frye* for propounding a conservative test, this Article contends that the general acceptance test must be replaced because of its vagueness and its failure to accord differing weights to the interests of criminal defendants, the prosecution, and civil litigants.

2. Professor Giannelli's Proposal for Adjusting Standards of Proof

The proposed multi-tiered standard differs significantly from the two-tiered standard proposed by Professor Giannelli in his classic 1980 article, *The Admissibility of Novel Scientific Evidence*.[305] Giannelli argued that testimony by scientific experts should be admissible only if "[t]he prosecution in a criminal case . . . [first] establish[es] the validity of a novel scientific technique beyond a reasonable doubt. Civil litigants and criminal defendants, on the other hand, should [be required to] establish the validity of a novel technique by a preponderance of the evidence."[306]

In Professor Giannelli's proposal, criminal defendants have no advantage over civil litigants regarding the admission of favorable scientific evidence. However, criminal defendants are entitled to a uniquely liberal standard because "the Constitution guarantees criminal defendants 'a meaningful opportunity to present a complete defense.'"[307]

Another problem with Professor Giannelli's proposal is that the tiers of a scientific evidence standard should not be distinguished by differing standards of proof. In fact, such a proposal would conflict with *Bourjaily v. United States*,[308] which established that under the Federal Rules of Evidence, any offering party need only prove by a preponderance of the evidence that evidence is admissible.[309] Our

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305. *See supra* note 46, 1246-50. Giannelli's 1980 proposal incorrectly assumed that the validity of a scientific technique can be established independently of establishing that it is accepted by scientists. *See, e.g., id.* at 1248 ("Although general acceptance by a recognized discipline or profession would be relevant, such acceptance would be neither required nor necessarily sufficient.").

306. *Id.* at 1248. *See New Challenges of Scientific Evidence, supra* note 9, at 1531 (endorsing Professor Giannelli's proposal for different standards of proof for the prosecution and criminal defendants).

307. Crane v. Kentucky, 476 U.S. 683, 691 (1986) (quoting California v. Trombetta, 467 U.S. 479, 485 (1984)). *See also Crane*, 476 U.S. at 687.

308. 483 U.S. 171 (1987).

309. *See id.* at 175-76. *See also* Huddleston v. United States, 485 U.S. 681, 687 n.5 (1988) ("[I]n *Bourjaily* v. *United States*, . . . we concluded that preliminary factual findings under Rule 104(a) are subject to the preponderance-of-the-evidence standard."); Berger, *supra* note 11, at 1365 (noting, in regard to standards for the admission of scientific evidence, that "Rule 104(a) requires the burden of persuasion to remain with the profferor" (footnote omitted)).

Rule 104(a) provides in pertinent part: "*Questions of Admissibility generally.* Preliminary questions concerning . . . the admissibility of evidence shall be determined by the court . . . ." FED. R. EVID. 104(a).

Daubert noted that the question of whether a theory or technique is an admissible subject of scientific expert testimony is to be resolved by the judge pursuant to Rule 104(a). *See Daubert*, 509 U.S. at 592.

proposed three-tiered standard, however, does not violate federal law because its tiers are not distinguished by differing standards of proof.

Even absent this ruling in *Bourjaily*, adjustments of the standard of proof would not be the best way to recognize the different weights of the interests of criminal defendants, the prosecution, and civil litigants. The different weights are recognized if the strength of a judge's reasons for believing that work is scientific must increase depending on whether the offering party is a criminal defendant, a civil litigant, or the prosecution. The strength of a judge's reasons is exclusively determined, however, by whether dual iterated disinterested acceptance, or some greater amount of acceptance, exists within the relevant scientific community. Accordingly, a three-tiered specification of the requisite amount of acceptance beyond dual iterated disinterested acceptance accords with our legal system's differential valuation of litigants' interests.

C. A Criminal Defense Standard

Under our revamped *Frye* standard, a criminal defendant need only provide a judge with minimally good reasons to believe that a theory or technique is scientific. Thus, a theory or technique constitutes an admissible subject of testimony by criminal defense experts so long as dual iterated disinterested acceptance exists within some relevant scientific community. This standard is easily satisfied, and its liberality accords with the unique prerogatives that American law accords to criminal defendants. At the same time, the standard precludes criminal defense experts from testifying about a theory or technique that has been accepted only for idiosyncratic reasons having nothing to do with science. Testimony by criminal defense experts is also precluded if the only people who accept a theory or technique are technicians,[310] a mutual admiration society or cult,[311] scientists whose "'livelihood [is] . . . intimately connected with the new technique,'"[312] or scientists who do not have "a reasonably comprehensive understanding" of the forensic science issues.[313]

The *Harvard Law Review* Comment suggested that it is constitutionally impermissible to condition the admissibility of scientific expert testimony favoring a criminal defendant on scientists' acceptance of the

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310. *See supra* Part V.B.1.

311. *See supra* Part V.A.

312. People v. Young, 391 N.W.2d 270, 276 (Mich. 1986). For discussion of this requirement, see *supra* Part V.B.2.

313. United States v. Yee, 134 F.R.D. 161, 195 (N.D. Ohio 1991). *See also supra* Part V.B.3.

underlying theory or technique.[314] However, by requiring dual iterated disinterested acceptance within a relevant scientific community, our revamped *Frye* standard allows criminal defense witnesses to benefit from the special legal prerogatives of expert witnesses only if a judge has reason to believe that the theory or technique about which they propose to testify is scientific. This reasonableness condition for the admission of scientific evidence accords with the fact that "[a] 'reasonable doubt,' at a minimum, is one based upon 'reason.'"[315] Just as the mere existence of a doubt does not constitutionally entitle a defendant to acquittal, a defendant is not entitled to introduce scientific expert testify about a theory or technique that a judge cannot reasonably deem scientific. This proposed requirement of dual iterated disinterested acceptance within a relevant scientific community is not contrary to, but rather dictated by, "a system of justice committed to granting a defendant every opportunity to persuade a jury that there exists a reasonable doubt as to her guilt."[316]

Our reasonableness condition also comports with the recognition that restrictions on criminal defense evidence need not violate a criminal defendant's "fundamental constitutional right to a fair opportunity to present a defense."[317] In *Crane v. Kentucky*, the Supreme Court recognized a criminal defendant's constitutional right to present a defense, but nonetheless stated that "we have never questioned the power of States to exclude evidence through the application of evidentiary rules that themselves serve the interests of fairness and reliability — even if the defendant would prefer to see that evidence admitted."[318] "[T]he interests of fairness and reliability"[319] are served by ensuring that criminal defendants are allowed to present scientific expert testimony that will be accompanied by an aura of scientific expertise and the special evidentiary prerogatives of expert testimony only if a judge has reason to believe that the proferred testimony concerns a genuine subject of scientific expertise.[320]

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314. *See Leading Cases*, *supra* note 170, at 126-27 ("Allowing a defendant to introduce exculpatory expert testimony despite the scientific community's misgivings may be a constitutional requirement.").

315. Jackson v. Virginia, 443 U.S. 307, 317 (1979).

316. *Leading Cases*, *supra* note 170, at 126-27 (footnotes omitted).

317. Crane v. Kentucky, 476 U.S. 685, 687 (1986).

318. *Id.* at 690 (quoting Chambers v. Mississippi, 410 U.S. 284, 302 (1973)).

319. *Id.*

320. The Harvard Comment writers rely on Rock v. Arkansas, 483 U.S. 44 (1987), to reach the contrary position discussed in the text accompanying notes 293, 294, and 312. *See Leading Cases*, *supra* note 170, at 125-27. As the Comment writers recognize, the *Rock* Court was explicitly concerned with the criminal defendant's right to testify on his or her own behalf, as opposed to the criminal defendant's more general right to present a defense. *See Rock*, 483 U.S. at 62 (footnote omitted) ("Arkansas' per se rule excluding all posthypnosis testimony infringes impermissibly on the right of a defendant to testify on his

D. A Prosecution Standard

Under our revamped *Frye* standard, a theory or technique is an admissible subject of scientific expert testimony on behalf of the prosecution only if a judge has maximally good reasons to believe that the theory or technique is scientific. Maximally good reasons exist only if a judge has no good, affirmative reasons for questioning the scientific status of the theory or technique. To establish this, the prosecution must prove that the theory or technique is not rejected by any two independent, disinterested scientists who are accepted as members of a scientific community. In other words, prosecution experts can testify about a theory or technique only if dual iterated disinterested rejection does not exist within any relevant scientific community.

This prosecution standard demands additions to the conceptual apparatus developed in Part V. We stipulated that disinterested

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own behalf."). However, according to the Comment writers, "[*Rock*'s] prohibition against arbitrary restrictions on a defendant's testimony logically applies to the testimony in general of defense witnesses." *Leading Cases, supra* note 170 at 125-26 (footnotes omitted). Consequently, *Rock* casts doubt upon the constitutionality of the *Frye* rule as applied to defense experts.

Notwithstanding the Harvard Comment writers, the *Rock* Court itself explicitly recognized that a criminal defendant's right to present others' testimony may not be as broad as his or her right to testify in his or her own behalf. *See Rock*, 483 U.S. at 58. *Rock* stated, "[t]his case does not involve the admissibility of testimony of previously hypnotized witnesses other than criminal defendants and we express no opinion on that issue," and approvingly cited People v. Shirley, 723 P.2d 1354, 1384 (Cal. 1982), for excluding testimony by a criminal defendant from its otherwise total bar on testimony by any witness who had been hypnotized. *Rock*, 483 U.S. at 58 n.15. This distinction between the criminal defendant's rights in regard to his or her own and others' testimony is consonant with the criminal defendant's having the ultimate authority (as against his or her attorney) to decide whether or not to testify, but not to decide what other witnesses to call. *See* Jones v. Barnes, 463 U.S. 45, 751 (1983). At most, then, *Rock* establishes that our criminal defense standard cannot constitutionally be applied to testimony by a criminal defendant that is induced through an allegedly scientific technique. *Rock* has no bearing on the admissibility of testimony by criminal defense experts.

Rock's implications may not extend even this far. *Rock* invalidated Arkansas' per se rule against posthypnosis testimony on the ground that it was "an arbitrary restriction on the right to testify." *Rock*, 483 U.S. at 61. Far from being arbitrary, our requirement of dual iterated disinterested acceptance within some relevant scientific community is necessary to ensure that defendants gain the advantages attendant on the introduction of scientific evidence if and only if a judge has reason to believe that their proffered evidence is in fact scientific. Thus, applying this Article's criminal defense standard to restrict testimony by a criminal defendant that was induced through a scientific technique is analogous to imposing other restrictions on the right to testify in the interests of fairness. *See id.* at 56 ("In applying its evidentiary rules a State must evaluate whether the interests served by a rule justify the limitation imposed on the defendant's constitutional right to testify."); *see also* Nix v. Whiteside, 475 U.S. 157, 173 (1986) ("Whatever the scope of a constitutional right to testify, it is elementary that such a right does not extend to testifying falsely.").

acceptance cannot come from the proponent(s) of a theory or technique or her collaborators or fellow laboratory members.[321] In addition, we claimed that relevant scientific communities cannot include forensic scientists whose professional status or livelihood is "intimately connected with" the success of a technique.[322] To preserve *Frye*'s insight, our prosecution standard must also prevent scientific evidence from being excluded on the basis of extra-scientific biases. Accordingly, relevant scientific communities cannot include people whose professional status or livelihood is intimately connected with debunking a theory or technique.[323] Nor can disinterested rejection come from collaborators or members of these professional debunkers' laboratories. Joined to Part V's analyses of disinterest and of relevant scientific communities, these additions make it possible to determine whether dual iterated disinterested rejection exists within a relevant scientific community.

Yet, an absence of dual iterated disinterested rejection alone is not sufficient to admit prosecution evidence. Instead of being caused by widespread scientific acceptance, an absence of scientific opposition may result from a failure to disseminate a theory or technique for peer review.[324] Thereby, a theory or technique may fail to engender either

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321. *See supra* Part V.A.

322. *See supra* Part V.B.2.

323. Analogously to the acceptors in relevant scientific communities, the rejecters must be relatively, but not totally, disinterested. *See supra* Part V.B.2. Thus, the *Young* court correctly reasoned that the relevant scientific community included a retired academic biochemist who was a leader in developing electrophoresis for forensic tests on body-fluid enzymes, even though he was an unpaid consultant to a crime laboratory and was "[a]rguably . . . still seeking to vindicate his original position [that a particular bloodstain analysis system should be discontinued]." *People v. Young*, 391 N.W.2d 270, 275-76 n.23 (Mich. 1986).

324. In her concurrence in *People v. Wesley*, Chief Judge Kaye argued that this situation occurred with forensic DNA analysis in 1988, noting:

> Where controversy rages, a court may conclude no consensus has been reached. Here, however, the problem was more subtle: absence of controversy reflected not the endorsement perceived by our colleagues, but the prematurity of admitting this evidence. Insufficient time had passed for competing points of view to emerge. . . . Before bringing novel evidence to court, proponents of new techniques must subject their methods to the scrutiny of fellow scientists, unimpeded by commercial concerns.

People v. Wesley, 633 N.E.2d 451, 464 (N.Y. 1994). *See also id.* at 465 ("Our colleagues' characterization of a dearth of publications on this novel technique as the equivalent of unanimous endorsement of its reliability ignores the plain reality that this technique was not yet being discussed and tested in the scientific community." (footnote omitted)).

Daubert II, 43 F.3d at 1318 (footnote omitted), similarly reasoned:

> None of the plaintiffs' experts has published his work on Benedictin in a scientific journal or solicited formal review by his colleagues.

dual iterated disinterested rejection or dual iterated disinterested acceptance within any relevant scientific community. Our preceding analysis implies that in this situation, a judge has no affirmative reasons for doubting the scientific status of a theory or technique, but also has no reason to believe that the theory or technique is scientific.

Accordingly, a two-pronged requirement is needed for the admission of prosecution evidence. First, a theory or technique must have gained dual iterated disinterested acceptance within some relevant scientific community. In other words, like the criminal defense, the prosecution is entitled to present scientific expert testimony only if a judge has minimally good reasons to believe that it concerns a genuinely scientific theory or technique. Second, the prosecution, but not the defense, is also required to establish that dual iterated disinterested rejection does not exist in any relevant scientific community. In other words, scientific expert testimony is admissible on behalf of the prosecution only if a judge has no affirmative reasons to doubt that it concerns a genuinely scientific theory or technique.

The two prongs ensure that the hurdles for admitting scientific expert testimony are always greater for the prosecution than for the criminal defense.[325] As a consequence of the requirement of dual iterated disinterested acceptance, prosecution experts can never testify about a theory or technique unless it is also an admissible subject of criminal defense expert testimony. However, the requirement of an absence of dual iterated disinterested rejection may preclude prosecution experts from testifying about a theory or technique even if it is an admissible subject of criminal defense expert testimony.

This prosecution standard accords with a realistic conception of scientific activity. By requiring dual iterated disinterested acceptance as well as the absence of dual iterated disinterested rejection, scientific status is decreased, rather than enhanced, if a theory or technique is not

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> Despite the many years the controversy [over whether Benedictin causes birth defects] has been brewing, no one in the scientific community — except defendant's experts — has deemed these studies worthy of verification, refutation or even comment. It's as if there were a tacit understanding within the scientific community that what's going on here is not science at all, but litigation . . . .

325. *Kelly* also noted that "[e]xercise of restraint [in the admission of scientific evidence] is especially warranted when the identification technique is offered to identify the perpetrator of a crime." *People v. Kelly*, 549 P.2d 1240, 1245, 1248 (Cal. 1976). On the assumption that *Frye*'s general acceptance test is a conservative standard for the admission of evidence, the *Kelly* court saw this as an argument for applying a uniform *Frye* test to all scientific evidence offered by any criminal or civil litigant. *See id.* at 1245. This Article's position, to the contrary, is that the distinctive dangers that criminal defendants face from "scientific" evidence, including identification evidence, argue for subjecting evidence introduced by the prosecution to especially strict requirements of acceptance by scientists.

exposed for review and therefore can neither be accepted nor rejected by disinterested and independent scientists. Thus, this Article's standard takes account of the crucial role of peer review in scientific activity.[326]

In addition, since science proceeds only when there are unresolved questions, unanimity cannot be the requisite degree of acceptance for according scientific status to contemporary work. Dissent among scientists need not amount to dual iterated disinterested rejection within a relevant scientific community. Accordingly, this standard is realistic in the sense that the mere existence of dissent does not preclude prosecution experts from testifying about a theory or technique.

Finally, both prongs of the standard incorporate the recognition that scientific status can be determined only by communities of independent scholars, not by mutual admiration societies or cults. Not requiring a precise amount of acceptance beyond dual iterated disinterested acceptance recognizes that no amount of current acceptance by scientists can ever guarantee future scientific acceptance. Accordingly, since the sheer amount of current scientific acceptance is not an index of ultimate scientific worth, a judge has negative reasons to doubt the scientific status of a theory or technique only if dual iterated disinterested acceptance does not exist within any relevant scientific community. A judge has affirmative reasons for doubt only if dual iterated disinterested rejection exists within some relevant scientific community. Like the criminal defense standard, the two-pronged prosecution standard comports with the fundamental legal notion that the prosecution's task is not to eliminate any and all doubts, but rather to eliminate all reasonable doubt.

E. A Standard for Civil Litigation

The danger of a misleading aura of scientific expertise and the special evidentiary prerogatives of expert witnesses imply that in civil as well as criminal litigation, dual iterated disinterested acceptance within a relevant scientific community is necessary for the admission of expert scientific testimony about a theory or technique. However, since the interests on both sides of a civil litigation are generally of equal legal value, satisfaction of this minimal reasonableness condition is necessary,

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326. *See, e.g., Wesley*, 633 N.E.2d at 465 (Kaye, C.J., concurring) ("The People's effort to gain a consensus by having their own witnesses 'peer review' the relevant studies in time to return to court with supporting testimony was hardly an appropriate substitute for the thoughtful exchange of ideas in an unbiased scientific community envisioned by *Frye*."); *Daubert*, 509 U.S. at 593; *Daubert II*, 43 F.3d at 1318; Note, *The "Brave New World" of Daubert: True Peer Review, Editorial Peer Review, and Scientific Validity*, 70 N.Y.U. L. REV. 100 (1995).

but not sufficient, for a theory or technique to be an admissible subject of scientific expert testimony in a civil case. A civil litigant can justifiably be accorded the special advantages attendant on the introduction of scientific evidence only if the judge has better reasons to believe that a theory or technique is scientific than to believe it is not scientific.[327] Better reasons are not equivalent to maximally good reasons, however. Hence, in contrast to the prosecution, civil litigants should not be required to prove that dual iterated disinterested rejection is absent in all relevant scientific communities. Rather, a civil litigant establishes the requisite better reasons only if he or she shows that dual iterated disinterested acceptance exists within some relevant scientific community and that more disinterested members of relevant scientific communities accept the theory or technique than reject it.

As argued in Part VI.A, judges will be tempted, if not forced, to rest admissibility decisions on their own substantive scientific judgments and/or personal biases if they seek to identify one group of scientists as the relevant scientific community. Accordingly, under the proposed civil as well as prosecution and criminal defense standards, litigants have the task of arguing that opinions within some particular scientific community are relevant to the admissibility of expert testimony about a particular theory or technique. A judge is to accept such proposed communities as relevant so long as their members satisfy Part V.B's uncontroversial conditions of being scientists rather than technicians,[328] having "livelihood[s that are not] . . . intimately connected with the new technique,"[329] and having "a reasonably comprehensive understanding" of the forensic science issues.[330]

Specifically, under the civil standard, the offering party has the initial burden of proving that a theory or technique has attained dual iterated disinterested acceptance within some relevant scientific community. The burden then shifts to the adverse party to establish that dual iterated disinterested rejection also exists within some relevant scientific community. If these burdens are met, the court must then determine whether any additional acceptance or rejection adduced by either party comes from a disinterested member of a relevant scientific community. Counting only the votes of disinterested members of relevant scientific communities and according equal value to all votes,

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327. Bertin and Henifin recognize that an evaluation of the litigants' opposing interests is implicit in judgments of when evidence should be admitted in civil cases. *See* Bertin & Henifin, *supra* note 26, at 8.

328. *See supra* Part V.B.1.

329. People v. Young, 391 N.W.2d 270, 276 (Mich. 1986).

330. United States v. Yee, 134 F.R.D. 161, 195 (N.D. Ohio 1991).

a court may find that a theory or technique is an admissible subject of scientific expert testimony only if it is accepted by a majority.[331]

This three-tiered family of revamped *Frye* standards removes the specter of "junk science" from litigation. Moreover, just as this Article's criminal standards comport with the "transcending value" of criminal defendants' interests,[332] the proposed civil standard comports with the equal value that American law generally accords to the interests of both parties in a civil case.[333]

VIII. CONCLUSION

While *Daubert v. Merrell Dow Pharmaceuticals, Inc.* revolutionized scientific evidence law, this decision was based on a fundamental misunderstanding of the history and philosophy of science. The previous *Frye* standard rests on a major philosophical insight into the social nature of human knowledge in general and scientific knowledge in particular. However, the Supreme Court's misguided decision in *Daubert* cannot be corrected simply by returning to the *Frye* standard; the proposed three-tiered standard includes major changes needed to rescue *Frye*'s philosophical core from the muddle created in seventy-odd years of judicial application.

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331. *See supra* Part VI.B for a discussion of the danger of judicial weighting of opinions within a relevant scientific community.

332. *See In re Winship*, U.S. 358, 364, 372 (1970) (Harlan, J., concurring).

333. Asymmetry in the value of the parties' underlying interests occasionally requires departures from the egalitarian evidentiary rules that generally prevail in civil cases. For example, *In re Winship* reasoned that in civil juvenile delinquency proceedings, proof beyond a reasonable doubt is constitutionally mandated because the youths' reputation and liberty interests are similar to those of criminal defendants. *See id.* at 359, 367-68. Similarly, if one party in a particular type of civil case is adjudged to have interests of "transcending value," the proposed criminal defense and prosecution standards should respectively apply when that party requests the admission or exclusion of scientific evidence.

While civil litigants whose interests do not justify application of the proposed criminal standards might argue for the application of an intermediate standard that is more favorable than the proposed civil standard, a philosophically revamped *Frye* standard should include only three tiers. There is no principled basis for determining how much acceptance and rejection an intermediate standard for the admission of scientific evidence should require and allow. Moreover, the application of any intermediate standard will require more complicated calculations of acceptance and rejection than the three tiers require. As the calculation becomes more complicated and less principled, judges will be tempted, if not forced, to resort to their own substantive scientific judgments and/or personal biases to determine whether particular evidence is admissible.