



## FEDERAL PHILOSOPHY OF SCIENCE: A DECONSTRUCTION — AND A RECONSTRUCTION\*

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### I. A TANGLED TALE

When they feel the need to distinguish genuine science—the real thing—from pretenders, or to understand what is distinctive about the scientific method, U.S. courts have sometimes called on Karl Popper’s conception of falsifiability as the hallmark of the genuinely scientific, and his account of the method of science as conjecture and refutation. Sometimes the legal issue before the courts is the interpretation of the Establishment Clause in application to biology teaching in public high schools, and the question is whether “creation science” is really science, or only religion in disguise.<sup>1</sup> Much more often, though, the legal issue is one of evidence

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<sup>1</sup> In *McLean v. Arkansas*, for example, the court reasoned that, unlike the theory of evolution, creation “science” is unfalsifiable, and so is not really science at all, but rather a religious doctrine; hence a statute mandating equal time for evolution and creation science in public high-school biology classes was unconstitutional—in violation of the Establishment

law, and the question is whether this or that scientific expert testimony is reliable enough to be admissible. This will be my focus here.

As anyone familiar with the legal history will know, this story begins with the Supreme Court's first-ever decision on the admissibility of expert testimony, *Daubert v. Merrell Dow Pharmaceuticals, Inc.* (1993).<sup>2</sup> Ruling for a unanimous Court that Federal Rule of Evidence 702 (1975)<sup>3</sup> had superseded the old *Frye* rule,<sup>4</sup> but that courts still had a responsibility to screen proffered expert testimony for both relevance and reliability, Justice Blackmun went on (now speaking only for the majority) to suggest that in determining whether a theory or technique is "scientific knowledge that will assist the trier of fact," ordinarily, "a key question to be answered will be whether it can be (and has been) tested." And in this context, he cites Popper.<sup>5</sup>

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Clause of the First Amendment to the Constitution, which provides that "Congress shall make no law respecting an establishment of religion." U.S. CONST. amend. I; *McLean v. Ark. Bd. of Educ.*, 529 F. Supp. 1255 (E.D. Ark. 1982). Popper's philosophy of science was invoked first by Michael Ruse as expert witness for the plaintiffs, and then (without Popper's name) by Judge Overton in his ruling, to argue that the theory of evolution is science, but "creation science" is not. Michal Ruse, *Witness Testimony Sheet: McLean v. Arkansas*, in *BUT IS IT SCIENCE? THE PHILOSOPHICAL QUESTION IN THE CREATION/EVOLUTION CONTROVERSY* 287 (Michael Ruse ed., 1996).

<sup>2</sup> 509 U.S. 579.

<sup>3</sup> At the time of *Daubert*, FED. R. EVID. 702 (1975) read: "If scientific, technical, or other specialized knowledge will assist the finder of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise." (In 2000 Rule 702 was modified to read: "If scientific, technical, or other specialized knowledge will assist the trier of fact to understand the evidence or to determine a fact in issue, a witness qualified as an expert by knowledge, skill, experience, training, or education, may testify thereto in the form of an opinion or otherwise, if (1) the testimony is based upon sufficient facts or data, (2) the testimony is the product of reliable principles and methods, and (3) the witness has applied the principles and methods reliably to the facts of the case.")

<sup>4</sup> *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923) (affirming judgment excluding results of a then-new blood-pressure deception test, finding that, "while courts will go a long way in admitting expert testimony from a well-recognized scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to be generally accepted in the field to which it belongs" (emphasis added)).

<sup>5</sup> *Daubert*, 509 U.S. at 592 (citing KARL R. POPPER, *CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE* 37 (5th ed. 1989) (1967)). Falsifiability is the

In an article published the following year, Professor Allen commented that, with *Daubert*, the Supreme Court had “replaced a judicial anachronism [*Frye*] with a philosophical one [Popper].”<sup>6</sup> It’s a nice one-liner, and there is an element of truth in it. For – while for decades Popper was not only enormously influential in philosophy of science (though, interestingly, less so in the United States than elsewhere), but also much admired by some important scientists of the day – by the time *Daubert* came down, the year before his death at the age of ninety-three, he was no longer the major player he had once been. The English edition of his *Logic of Scientific Discovery* first appeared in 1959.<sup>7</sup> Since that time, numerous rivals to his falsificationist approach had found supporters: Thomas Kuhn’s picture of routine, “normal” science conducted under a ruling paradigm, and the overturning and replacement of an old paradigm by a new in periods of “revolutionary” science (1962);<sup>8</sup> Imre Lakatos’s post-Kuhnian, quasi-Popperian attempt to distinguish progressive from degenerating research programs (1965);<sup>9</sup> Paul Feyerabend’s methodological anarchism (1975);<sup>10</sup> and, more recently, the many and various more or less radical styles of “science studies,” and the

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first of four indicia of reliability (now known as the “*Daubert* factors”) suggested in the ruling: whether the theory or technique at issue can be (and has been) tested; the known or potential error-rate; whether the work has been subjected to peer review and publication; and (in a nod to *Frye*) acceptance in the relevant scientific community. *Daubert*, 509 U.S. at 593–94.

<sup>6</sup> Ronald J. Allen, *Expertise and the Daubert Decision*, 84 J. CRIM. L. & CRIMINOLOGY 1157, 1164 (1994) (quoting Christopher Kamper, *Paradigms Talking Past Each Other: Expert Testimony and Problems of Translation* (1993) (unpublished manuscript, on file with The Journal of Criminal Law & Criminology)); *id.* at 1164 n.14. I note that *Frye* is not quite the legal anachronism Professor Allen’s observation suggests; it remains the law in seventeen states (Alabama, Arizona, California, Florida, Hawaii, Illinois, Kansas, Maryland, Minnesota, New Jersey, New York, Nevada, North Carolina, Pennsylvania, South Carolina, Washington, and Wisconsin). TERENCE J. CAMPBELL & DEMOSTHENES LORANDOS, *CROSS EXAMINING EXPERTS IN THE BEHAVIORAL SCIENCES* §1:16.1 n.7 (2009).

<sup>7</sup> KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* (English ed., 1959) (1934).

<sup>8</sup> THOMAS S. KUHN, *THE STRUCTURE OF SCIENTIFIC REVOLUTIONS* (1962).

<sup>9</sup> Imre Lakatos, *Falsification and the Methodology of Scientific Research Programmes*, in 4 *CRITICISM AND THE GROWTH OF KNOWLEDGE: PROCEEDINGS OF THE INTERNATIONAL COLLOQUIUM IN THE PHILOSOPHY OF SCIENCE, LONDON, 1965*, at 91 (Imre Lakatos & Alan Musgrave eds., 1970).

<sup>10</sup> PAUL K. FEYERABEND, *AGAINST METHOD* (rev. ed. 1978) (1975).

Bayesian currents in philosophy of science.<sup>11</sup> Popper still has his devotedly loyal followers, though they are fewer than they once were. But others dismiss him as a philosophical “sloganeer,”<sup>12</sup> trafficking in superficially appealing but ultimately disappointing philosophical “soundbites”;<sup>13</sup> and the reviewer of a biography of Popper writes in the *New York Times Book Review* that, because of his notorious unwillingness to listen to anyone who dared to criticize his views, Popper had “condemned himself to a lifetime in the service of a bad idea.”<sup>14</sup>

However, Professor Allen’s comment may suggest that Justice Blackmun was knowingly endorsing Popper’s philosophy “whole cloth” – which would be a serious over-simplification. For, in the same sentence in which he cites Popper, Justice Blackmun hedged his bets by also referring to another, far less radical philosopher of

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<sup>11</sup> See, e.g., THE STANFORD ENCYCLOPEDIA OF PHILOSOPHY (Edward N. Zalta ed., 2009), <http://plato.stanford.edu>, in which no fewer than thirteen entries refer to “Bayesianism” – the more relevant being James Joyce, *Bayes’ Theorem*, <http://plato.stanford.edu/entries/bayes-theorem>; Thomas Kelly, *Evidence*, <http://plato.stanford.edu/entries/evidence>; Alan Hajek, *Interpretations of Probability*, <http://plato.stanford.edu/entries/probability-interpret/>; and James Hawthorne, *Inductive Logic*, <http://plato.stanford.edu/entries/logic-inductive/>.

<sup>12</sup> Noretta Koertge, Lecture at the Summer School for Theory of Knowledge, Warsaw Madralin: Popper and the Science Wars (Aug. 16–31 1997), available at <http://www.indiana.edu/~koertge/PopLect1.html>. This Summer School was supported by the financier George Soros, a long-time admirer of Popper (and especially of his political philosophy). *Id.* See also William Shawcross, *Turning Dollars into Change*, TIME, Sept. 1, 1997, at 51 (“After leaving Soviet-controlled Hungary for London in 1947, Soros fell under the spell of . . . Karl Popper,” whose philosophy informed his banking practice, prompted his founding of the Open Society Institute, and influenced “his whole life.”).

<sup>13</sup> Rebecca Goldstein, *The Popperian Sound Bite*, in WHAT HAVE YOU CHANGED YOUR MIND ABOUT?: TODAY’S LEADING MINDS RETHINK EVERYTHING 8 (John Brockman ed., 2009).

<sup>14</sup> David Papineau, *The Proof is in the Disproof*, N.Y. TIMES, Nov. 12, 2000, § 7, at 28 (reviewing MALACHI HAIM HACOEN, KARL POPPER – THE FORMATIVE YEARS, 1902–1945: POLITICS AND PHILOSOPHY IN INTERWAR VIENNA (2000)), available at <http://www.nytimes.com/2000/11/12/books/the-proof-is-in-the-disproof.html>. Another reviewer of the same biography, also commenting on Popper’s notoriously difficult personality, conjectures that he adopted the manner and speech of a much bigger man to compensate for being so short in stature. Ivor Grattan-Guinness, *Truths and Contradictions about Karl Popper*, 59 ANNALS SCI. 89, 93 (2002).

science, Carl Hempel;<sup>15</sup> and this suggests that he was not fully aware how radical Popper's philosophy of science really is.

In any case, "out of date" just doesn't cut it as a criticism of a philosophical theory. The real difficulty with Daubert's appeal to Popper is not, as Professor Allen's comment might suggest, that by 1993 Popper's ideas were going out of style, overshadowed by the success of Thomas Kuhn's *Structure of Scientific Revolutions*.<sup>16</sup> No: the most glaring problem—as anyone familiar with Popper's philosophy of science would realize—is much more serious: it was downright bizarre to call on Popper—Popper, of all people!—to help determine whether expert scientific testimony is sufficiently reliable to be admissible. For a key thesis of Popper's is that *scientific claims can never be shown to be true, probable, or reliable*. A second problem follows hard on the heels of the first: if Popper's account were true, there would be *no* way to recognize reliable scientific testimony, so that the Court's preoccupation with the reliability of such testimony would rest on a serious misconception.

Some readers of this journal will, I suspect, be more familiar with Popper as a political philosopher than as a philosopher of science. So I will begin by presenting his philosophy of science in enough detail to show that it can't possibly provide a criterion of the reliability of scientific testimony (Part II). The next step will be to spell out how Justice Blackmun misconstrues Popper's ideas, and to identify some sources of this misunderstanding in the amicus briefs in *Daubert* and in the then-recent legal literature, as well as in Popper himself (Part III). Then it will be time to look at what federal courts have made of the Supreme Court's allusions to Popper as *Daubert* has played out in subsequent rulings on the admissibility of scientific testimony—which will reveal that courts and legal scholars have continued to misunderstand how radical Popper's ideas really are and, more importantly, how unsuitable for their purposes (Part IV). This will conclude the deconstruction of which my title speaks.

But, as my title also signals, my ultimate purpose is reconstruction: I hope, that is, to make some positive headway on legal issues

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<sup>15</sup> *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 592 (1993) (citing CARL G. HEMPEL, *PHILOSOPHY OF NATURAL SCIENCE* 49 (1966)).

<sup>16</sup> In fact, by 1993 Kuhn was no longer so dominant a figure as *he* had once been, either.

about scientific testimony. So my concluding argument will be, first, that the justice system's concern with reliability is both legally essential and philosophically legitimate; and second that, ironically enough—though the philosophy of science to which *Daubert* appeals is less than no help in determining reliability—the misinterpretation many federal courts have given the first, quasi-Popperian *Daubert* factor is closer to the truth than the Popperian philosophy of science from which it ostensibly derives (Part V).

## II. WILL THE REAL KARL POPPER PLEASE STAND UP?

Popper's work poses considerable difficulties for an expositor, not least because—to adapt a turn of phrase from J. L. Austin—there are the parts where Popper says it, and then there are the parts where he takes it back.<sup>17</sup> So, besides what I take to be the authentic, tough-minded falsificationist Popper, there is also a kind of shadow Popper: a more moderate and more plausible Popper, perhaps, but a more moderate and more plausible Popper who offers not so much a fully articulated philosophy of science as a congeries of plausible fallibilist aperçus and a couple of very appealing metaphors which, however, are neither easily reconciled with the main thrust of his arguments nor easily put together to form a better alternative picture. I'll get to this shadow Popper later; but let me begin with what I take to be the core themes of Popper's official account.

His Big Idea came to him, Popper tells us, around 1919 (when, I note, he would have been seventeen years old).<sup>18</sup> Many years later, he explained that it was disenchantment with the Marxist "scientific socialism" with which he had been enamored at sixteen that first made him aware how crucial the difference is between dogmatic

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<sup>17</sup> J.L. AUSTIN, *Performative Utterances*, in PHILOSOPHICAL PAPERS 233, 241 (J.O. Urmson & G.J. Warnock eds., 3d ed. 1979) (1956) (asking "now we feel the firm ground of prejudice glide away beneath our feet . . . what next?" and replying, "[y]ou will be waiting for . . . the bit where we take it all back"); J.L. AUSTIN, *SENSE AND SENSIBILIA* 2 (G.J. Warnock ed., 1962) (noting that philosophers who profess to believe that the objects of perception are not physical objects but sense data sometimes say that really this is just what we believed all along: "[t]here's the bit where you say it and the bit where you take it back").

<sup>18</sup> KARL POPPER, *UNENDED QUEST: AN INTELLECTUAL AUTOBIOGRAPHY* 31–38 (rev. ed. 1976) (1974).

thinking (bad) and a critical attitude (good).<sup>19</sup> This awareness, he continues, was reinforced by his encounters with Freud's and Adler's psycho-analytic theories, and emphatically underscored when Einstein's eclipse predictions were "successfully tested."<sup>20</sup>

The specific shape that Popper's Big Idea took is best understood in terms of his reaction to the approach taken by the Logical Positivists of the Vienna Circle,<sup>21</sup> the main thrust of which was to draw the line between good, clean scientific work and meaningless metaphysical speculation. The Logical Positivists proposed *verifiability* as the criterion of demarcation of *meaningful* from *meaningless* statements, and envisaged scientific theories' being confirmed *inductively*: i.e., as being warranted by evidence which, though not absolutely conclusive, makes it likely that the theory is true.<sup>22</sup> But Popper came to see the asymmetry between verification and falsification as crucial: positive instances, no matter how many, cannot show that an unrestricted universal claim is true, whereas a single counter-instance is enough to show that it is false. Moreover, he argued, induction is neither necessary nor justifiable: scientists do not arrive at hypotheses by inductive reasoning from particular instances, nor are hypotheses ever inductively supported by positive evidence.

So Popper turned Logical Positivism on its head.<sup>23</sup> In *The Logic of Scientific Discovery* (first published, in German, in 1934), he proposed

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<sup>19</sup> *Id.* at 38.

<sup>20</sup> *Id.* at 37-38. The ambiguity of the phrase "successfully tested" is worthy of note. Popper is not entitled to any meaning stronger than "the tests were conducted according to plan and Einstein's account was not falsified." However, what the phrase inevitably suggests is that the tests were conducted, Einstein's predictions confirmed, and his account shown to be successful.

<sup>21</sup> Popper was not a member of the Circle, but presented papers at what Professor Singer describes as "epicycles" of the group. Peter Singer, *Discovering Karl Popper*, N.Y. REV. BOOKS, May 2, 1974 at 22, 22 (book review), available at <http://www.nybooks.com/articles/9523>.

<sup>22</sup> See, e.g., A.J. AYER, ED., LOGICAL POSITIVISM (1959) (including classic papers by various Logical Positivists, and an introductory history of the movement).

<sup>23</sup> David Stove—probably Popper's severest critic, and certainly the funniest—observes that "the idea of reversal . . . is also the key to Popper's philosophy of science," and that "[a] Freudian might see, or imagine he sees, something more than adolescent revolt, something actually obsessive, in Popper's compulsion to *reverse* things." David Stove, *Cole Porter and Karl Popper: The Jazz Age in the Philosophy of Science*, in AGAINST THE IDOLS OF THE AGE 3, 5, 7 (Roger Kimball ed., 1999).

*falsifiability* as a criterion of demarcation of *science* from *non-science* and a purely *deductive* account of scientific method.<sup>24</sup> The core ideas of this—as one might call it—Logical Negativist<sup>25</sup> position are, in brief:

- Falsifiability is a criterion for demarcating science, the real thing, both from pseudo-sciences, such as “scientific socialism” and psychoanalysis, and from history, metaphysics, mythology, religion, “pre-science,” etc.<sup>26</sup>
- A statement is falsifiable, and hence scientific, only if it is incompatible with some basic statement,<sup>27</sup> i.e., a statement reporting the occurrence of an observable event at a specified place and time.<sup>28</sup>
- A statement is falsified when a basic statement with which it is incompatible is accepted.<sup>29</sup>
- The acceptance of basic statements is a matter for decision on the part of the relevant scientific community. What a scientist observes may *motivate* a scientist to accept a basic statement, but no observation can ever constitute evidence *justifying* or *warranting* the acceptance of such a statement.<sup>30</sup>

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<sup>24</sup> POPPER, *supra* note 7, at 40–41.

<sup>25</sup> This label, though very apt, is not very common; Popper’s position is more often called “Critical Rationalism,” “falsificationism,” or “deductivism.” (If I recall correctly, I learned the phrase “Logical Negativism” from my former colleague David Miller.)

<sup>26</sup> POPPER, *supra* note 7, at 40 (“[T]he falsifiability of a system is to be taken as a criterion of demarcation.”). In UNENDED QUEST, Popper tells us that the criterion of demarcation was originally intended to exclude Marxism and psychoanalysis, and only later extended to exclude metaphysics. POPPER, *supra* note 18, at 41.

<sup>27</sup> POPPER, *supra* note 7, at 86 (explaining that a theory is falsifiable if “it divides the class of all possible basic statements . . . into . . . those basic statements with which it is inconsistent . . . [and] the class of those basic statements which it does not contradict”). In other words, as Popper also puts it, “the class of its potential falsifiers is not empty.” *Id.*

<sup>28</sup> *Id.* at 102–03. “Basic statements are . . . statements asserting that an observable event is occurring in a certain individual region of space and time.” *Id.* at 103.

<sup>29</sup> *Id.* at 86 (“We say that a theory is falsified only if we have accepted basic statements which contradict it.”). Popper goes on to add that “[w]e shall take [a claim] as falsified only if we discover a *reproducible* effect which refutes the theory.” *Id.* Note that this addendum runs together *basic statements* and *the events they describe*.

<sup>30</sup> *Id.* at 105 (“Experiences can *motivate* a decision, and hence an acceptance or rejection of a statement, but a basic statement cannot be justified by them—no more than

- The only logical relations are deductive. There is no inductive logic, nor does science use induction.<sup>31</sup>
- Science proceeds by “conjecture and refutation”: a scientist makes an informed guess about the explanation of some puzzling phenomenon, deduces consequences of this guess, and—this is the distinctively Popperian methodological point—*tries to refute it* by subjecting those consequences to the severest possible tests.<sup>32</sup>
- This method uses only deductive logic—most importantly, the deductive rule of *modus tollens*, which licenses the inference from “if p then q” and “not q” to “not p,” used in the refutation phase.<sup>33</sup>
- Scientists should make bold, highly falsifiable conjectures; test them as severely as possible; and, should they be falsified when they are tested, drop them and start again rather than making *ad hoc* adjustments to save them.<sup>34</sup> This willingness to accept falsification is another criterion of the genuinely scientific.<sup>35</sup>

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by thumping the table.”). The argument seems to be twofold. First, justification is a logical relation, and logical relations can hold only among statements; so, since observations are not statements but events, they cannot stand in any kind of logical relation to a statement. Second, even a basic statement like “here is a glass of water” is imbued with theory. So the content of basic statements goes beyond what can be determined by observation; and if observations *could* stand in logical relations to basic statement, the relation would have to be an inductive (or, better, an ampliative) one—but there are no such relations. (This involves some rational reconstruction of Popper’s text, disentangling two strands of argument that he runs together. See SUSAN HAACK, EVIDENCE AND INQUIRY 144–49 (2d ed., 2009) (1993)).

<sup>31</sup> POPPER, *supra* note 7, at 29–30 (arguing that “the various difficulties of inductive logic . . . are insurmountable. . . . The theory to be developed in the following pages stands directly opposed to all attempts to operate with the idea of inductive logic”).

<sup>32</sup> *Id.* at 32–33; see also POPPER, *supra* note 7, at 51 (“[T]here is no more rational procedure than the method of trial and error – of conjecture and refutation: of boldly proposing theories; of trying our best to show that these are erroneous; and of accepting them tentatively if our critical efforts are unsuccessful.”).

<sup>33</sup> POPPER, *supra* note 7, at 76 (“The falsifying mode of inference . . . is the *modus tollens* of classical logic.”).

<sup>34</sup> *Id.* at 82 (urging that we should decide that “in the case of a threat to our system, we will not save it by any kind of *conventionalist stratagem*.”).

<sup>35</sup> *Id.* (explaining that “my criterion of demarcation cannot be applied immediately to a *system of statements*,” but that “[o]nly with reference to the method applied . . . is it at

- The probability of a claim is inversely related to its content; i.e., the more a statement says—and hence, the more falsifiable it is—the more *improbable* it is.<sup>36</sup>
- Theories which have been tested but not (yet) falsified have been—in a technical sense explained below— “corroborated,” to a degree depending on the severity of the tests passed.<sup>37</sup>
- To say that a theory is corroborated is to say that it has been subjected to such-and-such tests, and has not, so far, been falsified. This is strictly a report on the past. That a theory has been corroborated, to however high a degree, doesn’t show that it is true, that it is probable, that there is reason to believe it—or that it is reliable.

On this last point—which in the present context is obviously crucial—Popper is unambiguously clear: “Corroboration (or degree of corroboration) is . . . an evaluating *report of past performance*. . . . [I]t says nothing whatever about future performance, or about the ‘reliability’ of a theory.”<sup>38</sup> Again: “I lay stress on *negative* arguments, such as negative instances or counter-examples, refutations, and attempted refutations—in short, criticism—while the inductivist lays stress on ‘*positive instances*’ from which he draws ‘non-demonstrative *inferences*’ and which he hopes will guarantee the ‘*reliability*’ of those inferences.”<sup>39</sup>

In short, the core Popperian philosophy—which eschews verifiability, inductive logic, confirmation, supportive evidence, *and* reliability, and which urges scientists to make bold, highly falsifiable, and hence improbable, conjectures—is thoroughly negative. In fact, it’s far more negative than Popper acknowledges. Presenting himself

all possible to ask whether we are dealing with a conventionalist or an empirical theory”).

<sup>36</sup> *Id.* at 269; see also KARL R. POPPER, *Conjectural Knowledge, in* OBJECTIVE KNOWLEDGE: AN EVOLUTIONARY APPROACH 1, 18 (1972) (arguing that the degree of testability of a theory is inversely related to its probability).

<sup>37</sup> POPPER, *supra* note 7, at 265–69. The English translation of the heading of this section of the book speaks of “*How a Hypothesis May ‘Prove its Mettle.’*” However, in footnote \*1 on pages 53 and 251 of the English edition, Popper acknowledges that this phrase is potentially misleading.

<sup>38</sup> POPPER, *Conjectural Knowledge, in* OBJECTIVE KNOWLEDGE, *supra* note 36, at 18.

<sup>39</sup> *Id.* at 20.

as a champion of science, Popper purports to provide a thoroughly fallibilist, but still fully cognitivist, picture. But what he actually gives us is a kind of covert skepticism.<sup>40</sup> Indeed, his account is even more negative than we have seen thus far. For if, as Popper maintains, induction is wholly unjustifiable, there can be no reason to believe that a theory that passed a certain test today would pass the same test tomorrow. Moreover, if, as he maintains, the acceptance of basic statements is not justified by scientists' observations but is a matter of decisions on the part of the scientific community, there is no guarantee that a scientific statement that has been "falsified" is actually false; and this implies that scientific claims can no more be shown to be false than they can be shown to be true.

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Still, as Kierkegaard observes, "[i]n relation to their systems most systematizers are like a man who builds an enormous castle and lives in a shack nearby."<sup>41</sup> Popper is no exception. When he finds his forbidding Logical Negativist castle uninhabitable, he takes refuge in humbler but more comfortably fallibilist quarters. And this shadow Popper qualifies, amends, amplifies, and restates his Logical Negativism in ways that obfuscate matters considerably. Numerous qualifications obscure both the character and the motivation of the Logical Negativist criterion of demarcation. Appealing analogies purportedly illustrating Logical Negativism suggest, rather, a much more modest fallibilism. New ideas are added that seem, superficially, to moderate Logical Negativism, but on closer inspection turn out to leave it untouched. And, all along, Popper continues to use familiar, reassuring words while, all along, stripping them of essential meaning.

Despite his stress on the importance of distinguishing genuine science from impostors – and his claim to have discovered what is wrong

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<sup>40</sup> "Skepticism" is used here in the philosophical sense in which it refers to the thesis that we can know nothing, not in the ordinary-language usage in which it means "taking a critical, questioning attitude."

<sup>41</sup> SØREN KIERKEGAARD, *THE JOURNALS OF SØREN KIERKEGAARD* 156 (Alexander Dru ed., Oxford Univ. Press 1938) (1846).

with Marxism, Freudian psychoanalytic theory, etc.<sup>42</sup>—in the introduction to the English edition of *The Logic of Scientific Discovery*, Popper tells us that scientific knowledge is continuous with everyday empirical knowledge;<sup>43</sup> and in the body of the book, he describes his criterion of demarcation as a convention<sup>44</sup>—leaving one wondering what, exactly, the motivation is for wanting a criterion of demarcation in the first place. Then, in section 9, he acknowledges that the deduction of basic statements from a scientific theory will require auxiliary assumptions, and that by modifying these, we could shield a theory from falsification by contrary evidence.<sup>45</sup> So his criterion of demarcation is not, after all, purely logical, but partly methodological; and his methodological advice is not, after all, categorical (“drop a theory when it is falsified”), but conditional (“drop a theory if you can’t find a non-*ad-hoc* way of sidestepping contrary evidence”).<sup>46</sup> By now one is left wondering what, exactly, the criterion amounts to; what, exactly, it excludes;<sup>47</sup>

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<sup>42</sup> See, e.g., POPPER, *Science: Conjectures and Refutations*, in CONJECTURES AND REFUTATIONS, *supra* note 5, at 34 (recalling that his initial question, when it all began in 1919, was “[w]hat is wrong with Marxism, psycho-analysis, and individual psychology?”); POPPER, UNENDED QUEST, *supra* note 18, at 38 (recalling that by the end of that year he had arrived at the conclusion that “the scientific attitude . . . did not look for verifications but for crucial tests; tests which could *refute* the theory tested, though they could never establish it”).

<sup>43</sup> POPPER, *supra* note 7, at 18 (explaining that “scientific knowledge can only be an extension of common-sense knowledge”).

<sup>44</sup> *Id.* at 37 (“My criterion of demarcation will . . . have to be regarded as a . . . *convention*.”).

<sup>45</sup> *Id.* at 49–50.

<sup>46</sup> *Id.* at 82 (suggesting that it is willingness to accept falsification, rather than simply falsifiability, that makes a theory scientific). But in *Conjectural Knowledge*, *supra* note 36, Popper returns in a footnote to the old, logical understanding: “The ‘problem of demarcation’ is what I call the problem of finding a criterion by which we can distinguish the *statements* of empirical science from non-empirical *statements*.” *Id.* at 12 n.19.

<sup>47</sup> At one time, Popper himself suggested that the theory of evolution is not a scientific theory, but a “metaphysical research programme.” POPPER, *supra* note 18, at 167. He later changed his mind: the theory of natural selection *is* testable, and so it *is* science, after all. Karl R. Popper, *Natural Selection and Its Scientific Status*, in POPPER SELECTIONS 239–246 (David Miller ed., Princeton Univ. Press 1983) (1977). Ruse’s testimony in *McLean v. Arkansas Board of Education*, 529 F. Supp. 1255 (E.D. Ark. 1982), mentioned none of this, though a book he published the year before reveals that he was well aware of it. MICHAEL RUSE, *DARWINISM DEFENDED: A GUIDE TO THE EVOLUTION CONTROVERSIES* 132–33 (1982) (acknowledging that at one time Popper had denied that the theory of evolution is a scientific theory).

and what, exactly, it is intended to demarcate from what. Does it apply to theories? And if so, is it intended to demarcate the scientific from the non-scientific, or the empirical from the non-empirical? Or is it intended, rather, to demarcate scientific from non-scientific procedures — or what?

In *The Open Society and its Enemies* (1945), Popper tells us that the problem with Marxist scientific socialism was not, after all, that it was unfalsifiable, but that after it was falsified by the events of the Russian revolution, Marxists evaded this refutation by reinterpreting the theory.<sup>48</sup> But by the time of “Conjectural Knowledge” (1971), recognizing that modifying a theory in the face of contrary evidence isn’t *always* bad practice, Popper acknowledges “the value of a *dogmatic* attitude,” writing that “somebody [has] to defend a theory against criticism or it would succumb too easily.”<sup>49</sup> One is left unsure whether Popper is really offering a stringent methodological regimen, or only the tritest of methodological bromides.

In *The Logic of Scientific Discovery*,<sup>50</sup> and again in *Conjectures and Refutations*,<sup>51</sup> Popper describes the relation of science to its “empirical basis” as like piles driven into a swamp—a nice analogy<sup>52</sup> which, however, suggests a plausible fallibilist picture of basic statements as partially but not fully justified by scientists’ observations. But this picture can’t possibly be squared with Popper’s insistence that observation is irrelevant to justification. And in an article published in 1968, Popper describes scientific work as like building

<sup>48</sup> KARL R. POPPER, *THE OPEN SOCIETY AND ITS ENEMIES* 374 (rev. ed., 1950) (1945) (“Experience shows that Marx’s prophecies were false. But experience can always be explained away. And, indeed, Marx himself, and Engels, began the elaboration of an auxiliary hypothesis to [evade falsification].”); see also POPPER, *supra* note 18, at 43.

<sup>49</sup> POPPER, *supra* note 36, at 30.

<sup>50</sup> POPPER, *supra* note 7, at 111.

<sup>51</sup> POPPER, *supra* note 5, at 377. See also A.J. Ayer, *Truth, Verification and Verisimilitude*, in 2 *THE PHILOSOPHY OF KARL POPPER* 684, 684 (Paul Arthur Schilpp ed., 1974), and Popper’s reply, *Ayer on Empiricism and Against Verisimilitude*, *id.* at 1100; A.M. Quinon, *The Foundations of Knowledge*, in *BRITISH ANALYTICAL PHILOSOPHY* 55 (Bernard Williams & Alan Montefiore eds., 1966).

<sup>52</sup> A nice analogy anticipated more than fifty years earlier by C.S. Peirce. CHARLES SANDERS PEIRCE, *Pragmatism and Pragmaticism* (1898), in 5 *COLLECTED PAPERS OF CHARLES SANDERS PEIRCE* para. 589 (Charles Hartshorne & Paul Weiss eds., 1934) (“[Our knowledge] is not standing upon the bedrock of fact. It is walking upon a bog, and can only say, this ground seems to hold for the present.”) (references to the *COLLECTED PAPERS* are by volume and paragraph number).

a cathedral<sup>53</sup>—a really splendid analogy which, however, suggests a more or less cumulative picture of scientific progress. But this picture can't possibly be squared with the falsificationist conception, according to which science would be more like a Kafkaesque building site where, each day, workers try to demolish the previous day's work and, when they succeed, begin building anew—until the next day.<sup>54</sup> One is left with the distinct impression that Popper wants to have it both ways: that he is anxious to claim credit for a big, radical idea, but unwilling to swallow its big, radical consequences.

In a note appended to the English edition of *The Logic of Scientific Discovery*, Popper tells us that after he encountered Alfred Tarski's theory of truth<sup>55</sup>—which he, unlike Tarski himself, saw as a version of the correspondence theory<sup>56</sup>—he overcame his earlier reluctance to speak of the truth of scientific theories.<sup>57</sup> In due course he developed his account of “verisimilitude,” or as he also says,

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<sup>53</sup> POPPER, *On the Theory of the Objective Mind*, in OBJECTIVE KNOWLEDGE, *supra* note 38, 153, 185 (“Science . . . is a branch of literature; and working on science is a human activity like building a cathedral”).

<sup>54</sup> Popper adds that “[t]he method of problem solving, the method of conjecture and refutation, is practised by both [science and the humanities].” He goes on to compare constructing a theory of radioactivity and reconstructing a damaged text. *Id.* But once the method of conjecture and refutation has been elided into generic “problem-solving,” and applied to the humanities and even to literature, it is not clear what reason remains for worrying about demarcation.

<sup>55</sup> ALFRED TARSKI, *The Concept of Truth in Formalized Languages*, LOGIC, SEMANTICS, METAMATHEMATICS 152 (John Corcoran ed., J.H. Woodger trans., 2nd ed. 1983) (1956); Alfred Tarski, *The Semantic Conception of Truth and the Foundations of Semantics*, in READINGS IN PHILOSOPHICAL ANALYSIS 52, 53 (Herbert Feigl & Wilfrid Sellars eds., 1944) [hereinafter TARSKI, *Semantic Conception*].

<sup>56</sup> Compare Alfred Tarski, *Semantic Conception*, *supra* note 55, at 54 (claiming that several accounts of truth, among them the correspondence theory, “can lead to various misunderstandings,” and that “none of them can be considered a satisfactory definition”) with POPPER, *supra* note 18, at 98 (reporting that in 1935, after Tarski explained his theory of truth to him, he realized that “[Tarski] had finally rehabilitated the much maligned correspondence theory of truth”).

<sup>57</sup> POPPER, *supra* note 7, at 274 (“We need not say that the theory is ‘false’, but we may say instead that it is contradicted by a certain set of accepted basic statements. Nor need we say of basic statements that they are ‘true’ or ‘false’, for we may interpret their acceptance as the result of a conventional decision . . . .” Note \*1 added in the English edition “Owing to Tarski’s teaching, I am no longer hesitant in speaking of ‘truth’ and ‘falsity’.”).

“truth-likeness,” or “nearness to the truth.”<sup>58</sup> But it turns out that degree of corroboration is not (as one might have hoped) a measure of degree of verisimilitude, but only of what the verisimilitude of a theory *appears* to be, relative to other theories, at a given time.<sup>59</sup> Again, in *Unended Quest*, Popper tells us that it is rational to act on the basis of a well-corroborated theory. But—since he insists that the fact that a theory is corroborated, to however high a degree, is absolutely no reason to believe that it is true, that it is probable, or that it is reliable—the only rationale he can offer is that “actions . . . are ‘rational’ . . . if they are carried out in accordance with the state . . . of the critical scientific discussion.” This may sound somewhat reassuring, but the next sentence reveals that, by Popper’s lights, it is a trivial verbal truth: “[t]here is no better synonym for ‘rational’ than ‘critical.’”<sup>60</sup> So, after all, Popper has given no substantive reason for thinking that it is more rational to act on the basis of well-tested theories than on the purely speculative.

And, as we saw, in *The Logic of Scientific Discovery*, Popper uses words like “knowledge” and “discovery” without their usual connotation of truth,<sup>61</sup> and (though less openly) “falsified” without its usual connotation of “falsehood.”<sup>62</sup> He also tells us that what he

<sup>58</sup> POPPER, *Truth, Rationality, and the Growth of Scientific Knowledge*, 215 CONJECTURES AND REFUTATIONS, *supra* note 5.

<sup>59</sup> POPPER, *Two Faces of Common Sense*, in OBJECTIVE KNOWLEDGE, *supra* note 36, at 103. As Popper defines it, the “verisimilitude” of a theory is the proportion of its truth-content to its falsity-content; so his gloss “nearness to the truth” seems to be somewhat misleading.

<sup>60</sup> POPPER, *supra* note 18, at 87; *see also* POPPER, *Conjectural Knowledge*, in OBJECTIVE KNOWLEDGE, *supra* note 36, at 22 (acknowledging that “choosing the best-tested theory as the basis of action . . . is *not* ‘rational’ in the sense that it is based upon *good reasons* for expecting that it will in practice be a successful choice: *there can be no good reasons* in this sense”).

<sup>61</sup> *See* DAVID STOVE, POPPER AND AFTER: FOUR MODERN IRRATIONALISTS (1982) (reprinted under the title ANYTHING GOES: ORIGINS OF THE CULT OF SCIENTIFIC IRRATIONALISM (1999)) (criticizing Popper’s penchant for “neutralising success-words”). Stove does not, however, note that Popper also neutralizes failure-words like “falsified.”

<sup>62</sup> Later, Popper tells us that the “objective knowledge” to which he refers consists of “theories published in journals and books . . . ; discussions of such theories; difficulties or problems . . . with such theories,” and even “the logical content of our genetic code”; and that it belongs not to “world 1” (the realm of physical objects) or “world 2” (the realm of mental states), but to “world 3” (the realm of abstract objects such as numbers). POPPER, *supra* note 59, at 73.

calls “objective scientific knowledge” is all “conjectural,”<sup>63</sup> meaning that none of it is believed, any or all of it may turn out to be false, and none of it is ever warranted by good evidence—in fact, it is nothing but “a woven web of guesses.”<sup>64</sup> Again, his repeated references to “objective scientific knowledge” may sound reassuring, but the fact is that none of this does anything to alleviate the covert skepticism.

And, most consequentially for present purposes, before Popper realized how misleading this was, and adopted the word “corroboration” instead, he went along with Rudolf Carnap’s translation of his word “*Bewährung*” as “confirmation,”<sup>65</sup> and for a while even used the word “confirmation” himself. But in a footnote added to the English translation of *The Logic of Scientific Discovery*, he writes:

Carnap translated my term ‘degree of corroboration’ . . . as ‘degree of confirmation’ . . . I fell in with his usage, thinking that words do not matter. . . . I myself used the term ‘confirmation’ for a time . . . .

Yet it turned out that I was mistaken: the association of the word ‘confirmation’ did matter . . . . [d]egree of confirmation’ was soon used . . . as a synonym . . . of ‘probability’. I have therefore now abandoned it . . . .<sup>66</sup>

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<sup>63</sup> See, e.g., *id.* at 1-31.

<sup>64</sup> KARL R. POPPER, Lecture at the University of Tübingen: Toleration and Intellectual Responsibility (May 26, 1981), in POPPER, IN SEARCH OF A BETTER WORLD 188 (1992) (The phrase comes from Xenophanes, but Popper is here using it on his own behalf). Popper adds: “scientific knowledge . . . consists of . . . conjectures only. . . . The content of these . . . conjectures may be called *knowledge in the objective sense*.” *Id.* at 197-98.

<sup>65</sup> It is not entirely clear that Carnap’s was a mistranslation of the German word Popper had used. See COLLINS GERMAN UNABRIDGED DICTIONARY (5th ed. 2004), available at <http://dictionary.reverso.net/german-english/Bew%C3%A4hrung> (explaining “*Bewährung*” as “proving oneself or one’s worth,” “proving itself or its worth,” and giving as a secondary meaning “probation”). It is clear, however, that given the state of play in philosophy of science at the time, the effect of translating “*Bewährung*” as “confirmed” was extremely misleading.

<sup>66</sup> POPPER, *supra* note 7, at 251-52 n.\*1.

Small wonder, then, that—though Popper’s official story, the Big Idea, is about as ill-suited as it could be to discriminate reliable from unreliable scientific testimony—the parts where he takes it back make it all too easy to misconstrue what the Popperian story really is. So it should come as no surprise to find that the idea that Popper’s philosophy of science will be helpful to courts needing to determine whether scientific testimony is reliable enough to be admitted turns out to rest on mistakenly taking him to hold – what he repeatedly and emphatically denied – that *a claim that has been tested but not falsified is thereby confirmed, i.e., shown to be probable, warranted, valid, or reliable.*<sup>67</sup>

### III. HOW DID DAUBERT GET POPPER SO WRONG?

Popper’s ideas entered judicial thinking on the admissibility of expert testimony, as we saw earlier, with Justice Blackmun’s observations about how federal courts might go about determining whether such testimony is reliable enough to be admitted. Since Rule 702 refers to “scientific . . . knowledge,” Justice Blackmun argued, courts must determine whether proffered scientific evidence really *is* scientific knowledge.<sup>68</sup> “[T]he word ‘knowledge’ connotes more than subjective belief or unsupported speculation,” he explained, citing Webster’s dictionary; and “[t]he adjective ‘scientific’ implies a grounding in the methods and procedures of science.”<sup>69</sup>

But what is the mark of genuine science, and what are the methods and procedures followed in genuinely scientific work? The unmistakably Popperian flavor of Justice Blackmun’s answer is clear from the first consideration on his “flexible list” of indicia of reliability:

[A] key question . . . in determining whether a theory or technique is scientific knowledge . . . [is] whether it can be

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<sup>67</sup> It is worth noting that, in his testimony in *McLean*, Michel Ruse misinterpreted Popper in just this way. Ruse, *supra* note 1, at 303 (claiming that according to Popper, “if . . . tests are positive then the theory is confirmed, at least in the circumstances of that test. And, as a theory is confirmed in more and more circumstances, it is given increasing weight by science”).

<sup>68</sup> *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 590 (1993); *see also id.* at 588 (citing FED. R. EVID. 702)

<sup>69</sup> *Id.* at 590 (citations omitted).

(and has been) tested. "Scientific methodology today is based on generating hypotheses and testing them to see if they can be falsified; indeed, this methodology is what distinguishes science from other fields of human inquiry."<sup>70</sup>

The internal quotation here is from a law review article by Michael Green;<sup>71</sup> but in the next sentence Justice Blackmun cites Popper himself: "the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability."<sup>72</sup>

Unfortunately, the article of Popper's from which this quotation is taken, first published in 1957, is one of the places where he used Carnap's word, "confirmation" – which, only two years later, he would abandon as misleading. Perhaps this begins to explain why, in the very same sentence, Justice Blackmun *also* quotes another philosopher of science, Carl Hempel: "the statements constituting a scientific explanation must be capable of empirical test."<sup>73</sup> Apparently Justice Blackmun was quite unaware that Popper's understanding of "testable" ("potentially falsifiable") and Hempel's ("potentially *confirmable* or falsifiable") are quite different; that Popper specifically identifies Hempel as a proponent of the inductivist philosophy of science he repudiates;<sup>74</sup> and that by 1959, Popper had realized the danger of confusing his negativist, deductivist concept of corroboration with the positivist, inductivist idea of confirmation. And neither, obviously, does Justice Blackmun realize that Popper expressly disavows any interest in the reliability of scientific

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<sup>70</sup> *Id.* at 593 (citations omitted).

<sup>71</sup> Michael D. Green, *Legal Theory: Expert Witnesses and Sufficiency of Evidence in Toxic Substances Litigation: The Legacy of Agent Orange and Bendectin Litigation*, 86 NW. U. L. REV. 643, 645 (1992).

<sup>72</sup> *Id.* The citation is to CONJECTURES AND REFUTATIONS, POPPER, *supra* note 5, at 37. Justice Blackmun cites the 5th ed. (1989). But this book, which was first published in 1963, is an anthology of previously-published papers; and the article Justice Blackmun cites, *Science: Conjectures and Refutations*, was first published under the title *Philosophy of Science: A Personal Report*, in BRITISH PHILOSOPHY IN MID-CENTURY (C.A. Mace ed., 1957). Unfortunately, when this paper was reprinted in CONJECTURES AND REFUTATIONS, Popper did not note the misunderstanding over "confirmed" and "corroborated." Notice also that in this article Popper writes of falsifiability as "the," not "a," criterion of demarcation.

<sup>73</sup> *Daubert*, 509 U.S. at 593 (citing CARL G. HEMPEL, PHILOSOPHY OF NATURAL SCIENCE 49 (1966)).

<sup>74</sup> POPPER, *supra* note 36, at 20 n.29.

theories, and indeed avoids even using the word “reliable” without precautionary scare quotes.

It looks as if—perhaps unduly influenced by that honorific use of “science,” “scientific,” etc., as generic terms of epistemological praise, and perhaps forgetting that not all scientific expert testimony is reliable, nor all reliable expert testimony scientific<sup>75</sup>—Justice Blackmun ran “reliable” and “scientific” together. Then, casting about for some criterion to distinguish the genuinely scientific from pretenders, he fastened on Popper’s criterion of falsifiability but—not realizing that this was part of a thoroughly negative philosophical package that is no help at all on the question of reliability—ran Popper’s and Hempel’s quite different understandings of “testable” together.<sup>76</sup>

But *Daubert*’s pseudo-Popper was probably not entirely Justice Blackmun’s own creation, for the same misinterpretation of Popper was to be found in several amicus briefs filed in the case, as well as in the law review article cited in the relevant part of the ruling. Three of the four “*Daubert* factors” were already prefigured in an amicus brief submitted by the U.S. Department of Justice: arguing that expert testimony must reach a certain level of reliability if it is to be, as Rule 702 required, helpful to the trier of fact, these amici suggested error rates, peer review, and acceptance in the field as indicia of reliability.<sup>77</sup> And the first, quasi-Popperian *Daubert* factor, though not found in this amicus brief, is prefigured in several of the others.<sup>78</sup>

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<sup>75</sup> FED R. EVID. 702 speaks of “scientific, technical, or other specialized knowledge” (emphasis added); so the ellipses in Justice Blackmun’s reference to “scientific . . . knowledge” are significant.

<sup>76</sup> It may also be relevant that the usual meaning of “corroborated” in legal contexts is something like “confirmed by other witnesses.” See, e.g., 81 AM. JUR. 2D *Witnesses* § 968 (2009).

<sup>77</sup> Brief for the United States as Amicus Curiae Supporting Respondent, *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579 (1993) (No. 92-102), 1993 WL 13006291 at \*11-\*12. However, these amici suggest that acceptance by at least a significant minority in the field would suffice, whereas *Daubert* says that “[w]idespread acceptance can be an important factor in ruling particular evidence admissible....” *Daubert*, 509 U.S. at 594.

<sup>78</sup> See Brief for the American Ass’n for the Advancement of Science, et al. as Amici Curiae Supporting Respondent, *Daubert v. Merrell Dow Pharms., Inc.*, 609 U.S. 579 (1993) (No. 92-102), 1993 WL 13006281; Brief of the American Medical Ass’n et al. as Amici Curiae in Support of the Respondent, *Daubert*, 509 U.S. 579 (No. 92-102), 1993 WL 13006385; Brief of the Carnegie Commission on Science, Technology & Govern-

For example, an amicus brief submitted by the American Medical Association, et al., which avers that “[a]n opinion is only based upon scientific knowledge if it is developed in accordance with the scientific method,” goes on to say – citing Popper – that “[i]f a hypothesis is repeatedly corroborated by empirical testing, it is . . . generally accepted as valid.” In principle, these amici admit, “no scientific theory is ever definitively confirmed”; however, they continue, “[a]s a practical matter . . . , some theories are so thoroughly tested that they become virtually incontrovertible.”<sup>79</sup> Given how close being incontrovertible is to being unfalsifiable, this is about as un-Popperian as it could be.

Another amicus brief, from the Product Liability Advisory Council, et al., describes the scientific method like this:

(1) first set forth a hypothesis, (2) design an experiment . . . or experiments, to test the hypothesis, (3) conduct the experiment, collect the data, and then analyze those data, (4) publish the results so that they may . . . be subject to external scrutiny, and (5) ensure that those results are replicable and verifiable.

Each of these clauses has its own citation (omitted here); the last – appended to the part about the results being “replicable and verifiable” – is to *The Logic of Scientific Discovery*.<sup>80</sup> Given Popper’s repudiation of verificationism and his conventionalism about basic statements, this too is an exegetical travesty.

An amicus brief from the Carnegie Commission on Science, Technology, and Government argues that “opinions based on claims that are not capable of being tested should not be admitted into evidence” and cites Popper.<sup>81</sup> It immediately adds, citing a report from a

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ment as Amicus Curiae in Support of Neither Party, *Daubert*, 509 U.S. 579 (No. 92-102), 1992 WL 12006530; Brief of Product Liability Advisory Council et al. as Amici Curiae in Support of the Respondent, *Daubert*, 509 U.S. 579 (No. 92-102), 1993 WL 13006388.

<sup>79</sup> Brief of the American Medical Ass’n, *supra* note 78, at \*11 (emphasis added).

<sup>80</sup> Brief of Product Liability Advisory Council, *supra* note 78, at \*23, n.20 (emphasis added) (citations omitted).

<sup>81</sup> Brief of the Carnegie Commission on Science, Technology & Government, *supra* note 78, at \*13 and \*14, n. 12.

Panel of the National Academy of Sciences,<sup>82</sup> that results that cannot be replicated should also be excluded and that “scientists have the responsibility to replicate *and reconfirm* their results.”<sup>83</sup> Fair enough, except that the failure of these amici to note that Popper expressly eschews the notion of confirmation conveys the false impression that claims that have been subjected to repeated tests but not falsified have been “reconfirmed.” But of course results can’t be *reconfirmed* unless they have first been *confirmed*.

And an amicus brief from the American Association for the Advancement of Science, et al., though it doesn’t mention Popper by name, makes free use of his term “corroborated,” and comments that: “[S]cience . . . proceed[s] through a series of interrelated steps centered on the generation and testing of hypotheses. Hypotheses are educated guesses about a particular phenomenon or event . . . . [S]cientists conduct rigorous experimental testing in order to falsify hypotheses.”<sup>84</sup> This all sounds entirely Popperian—until the next sentence: “An hypothesis is *accepted as generally valid to the extent that it has survived repeated attempts at falsification.*”<sup>85</sup> This, of course, is the by now familiar pseudo-Popperian line.<sup>86</sup>

The crucial misreading of Popper was also, apparently, circulating in the law reviews, and is found specifically in the article by Michael Green<sup>87</sup> that Justice Blackmun quotes in *Daubert*. The key passage of Green’s paper—a very small part of a long article, most

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<sup>82</sup> 1 PANEL ON SCIENTIFIC RESPONSIBILITY AND THE CONDUCT OF RESEARCH, NAT’L ACADEMY OF SCIENCES, RESPONSIBLE SCIENCE: INSURING THE INTEGRITY OF THE RESEARCH PROCESS (1992).

<sup>83</sup> Brief of the Carnegie Commission, *supra* note 78, at \*14 n.13 (emphasis added). The passage from the NAS report concludes by speaking of “an ongoing process of revision and refinement that corrects errors and strengthens the fabric of research.” 1 PANEL ON SCIENTIFIC RESPONSIBILITY AND THE CONDUCT OF RESEARCH, *supra* note 82.

<sup>84</sup> Brief for the American Ass’n for the Advancement of Science, *supra* note 78, at \*8-\*9.

<sup>85</sup> *Id.* at \*9 (emphasis added).

<sup>86</sup> In a paper published the same year, one of the signatories, Bert Black, had published an article (written jointly with Francisco Ayala) which calls explicitly on Popper—and perpetrates the same misunderstanding. Francisco J. Ayala & Bert Black, *Science and the Courts*, 81 AM. SCIENTIST 230, 237 (1993) (citing Popper for the argument that “[b]ecause scientific hypotheses can be falsified but not absolutely established, they can only be accepted contingently,” and then giving “[a]n example of how a critical test can crystallize understanding *and certainty*”) (emphasis added).

<sup>87</sup> Green, *supra* note 71.

of which is taken up with a complex discussion of issues about epidemiological evidence in toxic-tort litigation—reads as follows:

Hume criticized the inductive, rather than the deductive, methodology. From that criticism emerged the idea that while induction could never conclusively prove a proposition, it could falsify one. Thus, based on the framework provided by Karl Popper, knowledge is gained by attempting to falsify a hypothesis based on empirical investigation. Scientific methodology today is based on generating hypotheses and testing them to see if they can be falsified; indeed, this methodology is what distinguishes science from other fields of human inquiry. Of course, *if a hypothesis repeatedly withstands falsification, we may tend to accept it, even if conditionally, as true.*<sup>88</sup>

Setting aside the first sentence, and skating over the second (where, I suspect, Green said “induction” when he meant to say “deduction”), I turn to the sentence Justice Blackmun quotes, the one beginning “Scientific methodology today . . . .”

This sentence vaguely suggests that Popper’s ideas were by then the consensus position in contemporary philosophy of science, or among scientists themselves—or perhaps, even, that working scientists *en masse* had by then come to recognize the virtues of the methodology Popper recommended. This suggestion is misleading, to say the least. As I said earlier,<sup>89</sup> at one time Popper’s ideas were not only very influential among philosophers of science, but also endorsed by a number of distinguished scientists, among them Sir Herman Bondi, Sir Peter Medawar, and Sir John Eccles—the “Popperian knights.”<sup>90</sup> And as I also said, there are still enthusiastic

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<sup>88</sup> *Id.* at 645–46 (emphasis added).

<sup>89</sup> See *supra* p. 396.

<sup>90</sup> I suspect that what scientists found attractive about Popper’s ideas may have been his picture of the scientist as making bold conjectures and fearlessly testing them and his stress on the rational, critical character of science and the objectivity of scientific knowledge. But John Eccles, who seems to have had the clearest understanding of the views he was endorsing, makes it very plain that their real appeal, for him, was the idea that it was not shameful, but a good thing, if your hypothesis was refuted—which, he reports, helped him out of a severe depression. John C. Eccles, *The World of Objective Knowledge*, in 1 THE PHILOSOPHY OF KARL POPPER, *supra*, note

Popperians about. But Popper's philosophy of science was *never* "generally accepted in the field to which it belongs;"<sup>91</sup> and by the time of Green's article, only a relatively few Popperian philosophers of science remained. It is the last clause of the last sentence of this passage, however, that is most to the present purpose: "if a hypothesis repeatedly withstands falsification, we may tend to accept it, even if conditionally, as true." This is a real rhetorical hum-dinger, managing to suggest, without ever actually saying, that a claim that has been tested but not falsified is thereby shown to be ("conditionally") true—a completely un-Popperian suggestion.

In a footnote to the passage I quoted, Green cites *The Logic of Scientific Discovery*—the whole thing, giving no page numbers; and in the same footnote, he cites a law review article by David Faigman, published shortly before his own. It seems possible that Green hadn't actually read Popper, but was relying on Faigman's account.<sup>92</sup> If so, it was not an entirely happy choice of source. Here is Faigman on Popper's philosophy of science:

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51, at 350. Peter Medawar writes that "[s]cientific methodology has to do with . . . validation and justification," and though later in the same paper he sounds somewhat more Popperian, he adds that the critical part of scientific reasoning aims to find out whether scientists' imaginative stories "are stories about real life." Peter Medawar, *Science and Literature*, ENCOUNTER, Jan. 1969, 15, at 17, 20 (emphasis added). And I can testify from personal experience that Hermann Bondi did not fully understand what he was endorsing. In the course of a 1998 lecture at the University of Miami, which he opened by explaining that he was a strong Popperian, Bondi told us that cosmology became a science in 1826, when Wilhelm Olbers made the first falsifiable cosmological conjecture; and that this conjecture was in due course falsified, and a new conjecture devised—a new conjecture which, he continued, was by now "well-confirmed by observation." Hermann Bondi, Lecture in the Dep't of Physics at Univ. of Miami (1998).

<sup>91</sup> D.H. Mellor's critical notice of the two large volumes of *THE PHILOSOPHY OF KARL POPPER* conveys something of the state of play by that point. He writes, for example, "Take Popper's attitude to induction, a central point of Popperian method and mythology. Popperians find us obtuse who do not see that Popper has solved the problem of induction. The feeling is mutual. . . . [A.J.] Ayer here repeats some long-standing objections to Popper's solution, of which Popper again fails to see the force." D.H. Mellor, *The Popper Phenomenon*, 52 PHIL. 195, 196 (1977).

<sup>92</sup> In July 2009, I asked Professor Green whether, when he wrote this paper, he had read Popper or had relied on Faigman's article; he replied that it was too long ago to remember. E-mail from author to Michael D. Green (Jul. 8, 2009) (on file with author); Email from Michael D. Green to author (Jul.10, 2009) (on file with author).

Falsifiability or testability represents the line of demarcation between science and pseudo-science, and *the strength of particular scientific statements depends on the extent to which they have been tested appropriately*. . . . Popper devoted much of his philosophical efforts to articulating a criterion by which scientific statements could be distinguished from nonscientific statements, especially pseudo-scientific, pre-scientific, and metaphysical statements. . . . *Empirical research might corroborate [a] hypothesis by finding evidence supporting it . . .*<sup>93</sup>

Faigman apparently *did* read (some) Popper, but didn't understand him very well. For one thing, the word "strength" in his first sentence is a fudge.<sup>94</sup> For another, there is slippage between this first sentence, which talks about science versus pseudo-science, and the next, which also includes pre-science, etc., under non-science; but there is no indication that Faigman realizes that the fact that Popper's criterion is intended to do several different jobs itself presents problems. But most importantly, the last sentence here, according to which corroboration is a matter of finding supportive evidence, encapsulates the key misunderstanding of Popper—the misunderstanding that will be passed down, via Professor Green, to Justice Blackmun.

#### IV. FALSIFIABILITY IN THE FEDERAL COURTS

Only two of Justice Blackmun's colleagues on the *Daubert* Court—then-Chief Justice Rehnquist, and Justice Stevens, who joined in Justice Rehnquist's partial dissent<sup>95</sup>—seem to have been even half-aware of how muddled the philosophy of science built into the majority ruling was. Yes, Justice Rehnquist wrote, *Frye* had been superseded; and yes, nevertheless, courts' responsibility for

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<sup>93</sup> David Faigman, *To Have and Have Not: Assessing the Value of Social Science to Law as Science and Policy*, 38 EMORY L.J. 1005, 1015–18 (1989) (emphasis added).

<sup>94</sup> Popper would indeed say that the strength of a claim depends on its content, i.e., that more falsifiable hypotheses are "stronger," in one sense. But Faigman's words vaguely suggest, without actually saying, that well-tested hypotheses are "strong" in the sense of "well-confirmed."

<sup>95</sup> *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 598 (1993) (Rehnquist, C.J., dissenting in part).

screening expert testimony remained.<sup>96</sup> But, he continued, there was no explicit reference in Rule 702 to reliability, and Justice Blackmun's observations about "scientific . . . knowledge" were too vague and too general to be helpful.<sup>97</sup> These observations, he argued, were bound to raise difficulties down the road, when courts have to determine how to handle *non-scientific* expert testimony;<sup>98</sup> and, frankly, he was baffled—as well he might be—by Justice Blackmun's allusions to falsifiability: "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."<sup>99</sup>

Subsequent rulings in which federal judges refer to Popper, falsifiability, or testability suggest that Justice Rehnquist was right to suspect that they would be confused. Sometimes, after quoting the passage in *Daubert* presenting the Court's "flexible list" of indicia of reliability, courts quietly set the Popperian rhetoric aside<sup>100</sup> and focus instead on some other aspect of the proffered testimony. But some courts take the first, Popperian *Daubert* factor to be primary;<sup>101</sup> and when courts actually try to *use* this factor, the results generally have been quite strange.

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<sup>96</sup> *Id.*

<sup>97</sup> *Id.* at 600.

<sup>98</sup> Indeed there were difficulties—resolved in 1999, when the Supreme Court ruled that *Daubert* applies to *all* expert testimony, not only the scientific. *Kumho Tire Co. v. Carmichael*, 526 U.S. 137 (1999).

<sup>99</sup> *Daubert*, 509 U.S. at 600.

<sup>100</sup> See, e.g., *Savage v. Union Pac. R.R.*, 67 F. Supp. 2d 1021 (E.D. Ark. 1999) (citing the four *Daubert* factors, but relying primarily on the fact that the plaintiff did not prove his level of exposure to creosote); *Nat'l Bank of Commerce v. Associated Milk Producers*, 22 F. Supp. 2d 942, 947 (E.D. Ark. 1998) (citing the four *Daubert* factors, but relying on Bernard D. Goldtsein & Mary Sue Heniflin, *Reference Guide on Toxicology*, in FED. JUDICIAL CTR., REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 181 (1st ed. 1994)).

<sup>101</sup> *Bradley v. Brown*, 42 F.3d 434, 438 (7th Cir. 1994) ("The first and most significant *Daubert* factor is whether the scientific theory has been subjected to the scientific method."); *Chikovsky v. Ortho Pharm. Corp.*, 832 F. Supp. 341, 345 (S.D. Fla. 1993) (arguing that, since *Daubert*, "[a]n issue of primary importance in the determination of whether a theory or technique is 'scientific knowledge' that will assist the trier of fact is 'whether it can be (and has been) tested'" (citing *Daubert*, 509 U.S. at 593); see also *Haggerty v. Upjohn Co.*, 950 F. Supp. 1160, 1163 (S.D. Fla. 1996) (citing *Bradley*, 42 F.3d at 438; *Chikovsky*, 832 F. Supp. at 345) (arguing that testimony having been arrived at by the scientific method is "the most significant of the *Daubert* factors").

A particularly egregious example—almost amusing, if what was at stake were not so serious—is *United States v. Bonds*, which came down very shortly after *Daubert*. Faced with a defense challenge to the FBI laboratory’s DNA analyses, the court read its new gatekeeping obligations strictly *au pied de la lettre*. The defendants proffered evidence that DNA identifications conducted by the FBI laboratory had been found to be unreliable, but the court reasoned that nonetheless the FBI identifications were admissible under *Daubert*; arguing that, in proffering evidence about the deficiencies of the FBI lab, “the defendants have conceded that the theory and methods can be tested. The dispute . . . is over *how* the results have been tested, not over *whether* the results can be or have been tested.”<sup>102</sup> In other words, the fact that the FBI lab’s DNA work had been tested and shown to be unreliable showed that the FBI’s testimony could be and had been tested; and hence was grounds for admitting it—as reliable!

Sometimes courts engage in a little light philosophy of science on their own behalf. In *United States v. Hines*, the court relates in a footnote that the *Daubert* ruling had been accused of “simply tak[ing] the definition of science from Karl Popper, a definition that others have criticized as *deriving from a culturally defined, time-bound paradigm*.”<sup>103</sup> Perhaps this vaguely Kuhnian talk signals that Green’s reference to “scientific methodology today” had not gone unnoticed. Also worthy of note is *Bitler v. A. O. Smith Corp.* where—instead of alluding, like Justice Blackmun, to the incongruous philosophical firm of Popper, Hempel & Associates—the court actually distinguishes the falsificationist Popper from the verificationist Hempel;<sup>104</sup> but unfortunately it fails to note that Popper’s and Hempel’s views are incompatible.

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<sup>102</sup> *United States v. Bonds*, 12 F.3d 540, 559 (6th Cir. 1993).

<sup>103</sup> *United States v. Hines*, 55 F. Supp. 2d 62, 65 n.7 (D. Mass. 1999) (emphasis added) (citing, *inter alia*, Alexander Morgan Capron, *Daubert and the Quest for Value-Free “Scientific Knowledge” in the Courtroom*, 30 U. RICH. L. REV. 85 (1996)). The court does *not* note, however, that Professor Capron had quoted a passage in which Popper expressly repudiates any interest in whether a theory is true or acceptable. *Id.* at 92 n.23 (citing Popper, *supra* note 5, at 33). Capron’s is, in fact, the *only* law review article I have found that gets Popper right on this.

<sup>104</sup> 400 F.3d 1227, 1235 (10th Cir. 2004) (referring to the Supreme Court’s citation to “Popper’s method of falsification” and (a few lines later) to “the logical positivist Carl Hempel”).

Legally more significant, probably, are two fingerprint cases that misinterpret *Daubert's* reference to "testability" as referring not to scientific, empirical testing, but to legal, dialectical testing in court. In *United States v. Havvard*, the court reasons that "[the] claim of uniqueness and permanence [of fingerprints] is a scientific claim in the sense that it can be falsified. . . . In the roughly 100 years since fingerprints have been used for identification purposes, no one has managed to falsify the claim of uniqueness . . . ." <sup>105</sup> Of course, the crucial issue is not really whether fingerprints are unique, but whether accurate identifications can be made on the basis of latent prints representing, on average, 20% of a full fingerprint; but set that aside. The most interesting point for present purposes is the way the court goes on to construe "testing": in terms, not of empirical testing in a laboratory or in the field, but of in-court "testing" through cross-examination: "[T]he methods of latent print identification can be and have been tested. They have been tested for roughly 100 years. They have been tested in adversarial proceedings with the highest possible stakes—liberty and sometimes life." <sup>106</sup> Whatever, exactly, Popper understood by testing, we can be quite sure it was *not* argument and counter-argument in adversarial legal proceedings. <sup>107</sup>

And in *United States v. Mitchell*, ruling that the lower court did not abuse its discretion in admitting latent-fingerprint identification testimony, the court first looks to Webster's dictionary for a definition of "falsifiable": "capable of being proved false, defeasible"; then glosses this in a way that starts out sounding entirely Popperian: "for

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<sup>105</sup> 117 F. Supp. 2d 848, 852 (S.D. Ind. 2000).

<sup>106</sup> *Id.* at 854.

<sup>107</sup> Nor, I believe—recalling his angry dissent in *Barefoot v. Estelle*—was it what Justice Blackmun had in mind. 463 U.S. 880, 916 (1983) (Blackmun, J., dissenting). Justice White, writing for the majority, had argued that psychiatrists' predictions that the defendant would be dangerous in the future had correctly been admitted, despite the fact that an amicus brief filed by the American Psychiatric Association acknowledged that such predictions were wrong two times out of three. Reliability, he reasoned, was a matter to be determined through cross-examination and the presentation of contrary witnesses. *Id.* at 898–99 (majority opinion). Justice Blackmun, however, argued that in this case the adversarial process had failed to expose unreliable testimony. *Id.* at 929–30 (Blackmun, J., dissenting) ("There is every reason to believe that . . . jurors will be still less capable [than judges] of 'separating the wheat from the chaff,' despite the Court's blithe assumption to the contrary.").

instance, the hypothesis ‘all crows are black’ is falsifiable (because an albino crow could be found tomorrow)”;<sup>108</sup> but then wanders into more comfortable legal territory: “a clairvoyant’s statement that he receives messages from dead relatives is not [falsifiable] (because there is no way *for the departed to deny this*).”<sup>109</sup> Then, after conflating “falsify” and “deny,” the court argues that:

In this case the relevant premises were posed as explicit questions to many of the government experts: (1) Are human friction ridge arrangements unique and permanent? And (2) Can a positive identification be made from fingerprints containing sufficient quantity and quality of detail? *The government’s experts responded in the affirmative.*<sup>110</sup>

But then, apparently realizing that relying on the FBI’s experts’ *ipse dixit* won’t quite do, the court reverts to the same strictly-literal interpretation of the first *Daubert* factor we encountered in *Bonds*: “We must consider not whether we agree as a factual matter . . . but rather whether these hypotheses are testable (or tested). We conclude that they are.”<sup>111</sup>

However, by far the commonest pattern is for courts using the first *Daubert* factor simply to take for granted that theories that have withstood testing without being falsified are thereby shown to be reliable. In *Bradley v. Brown*, Judge Moody observes that “the court must weed out the speculative hypothesis from the tested theory”;<sup>112</sup> evidently he is unaware that, according to Popper, *all* scientific theories are speculative hypotheses. Similarly, in *United States v. Starzeczyzel*—after holding a *Daubert* hearing on forensic document identification testimony at the end of which it ruled that such testimony is not scientific, and hence falls outside the scope of *Daubert*—the court explained that “[t]he *Daubert* test is grounded in the scientific process and directs the judge to evaluate the quality of

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<sup>108</sup> 365 F.3d 215, 235 (3d Cir. 2004).

<sup>109</sup> *Id.* (emphasis added).

<sup>110</sup> *Id.* (emphasis added).

<sup>111</sup> *Id.* at 235–36.

<sup>112</sup> 852 F. Supp. 690, 700 (N.D. Ind. 1994).

the testing supporting the scientific conclusion.”<sup>113</sup> We see the same assumption in *Haggerty v. Upjohn Co.*, where the court excluded plaintiff’s expert Dr. Mash on the grounds that he offered nothing but “a hypothesis which he had yet to attempt to verify or disprove by subjecting it to . . . testing;”<sup>114</sup> and in *In Re TMI Litigation*, where Judge Rambo excluded Dr. Gunckel’s testimony on the grounds that, though he had “advanced a hypothesis capable of falsification,” he had made “no effort . . . to verify either methodology or the conclusions reached.”<sup>115</sup> We see it again in *Moore v. Ashland Chemical, Inc.*, where the court relies on that fatal sentence from Michael Green’s article, that while “[t]heoretically . . . hypotheses are not affirmatively proved, only falsified, . . . of course, if a hypothesis repeatedly withstands falsification, one may tend to accept it . . . [as] true.”<sup>116</sup>

And we see it again in *Downs v. Perstorp Components, Inc.*, where the court reasons that “the scientific method must be an objective one. This is the essence of what the Supreme Court referred to as *scientific validity, also known as ‘falsifiability.’*”<sup>117</sup> Here the court equates scientific validity (which *Daubert* had identified with evidentiary reliability, and defined in a footnote as the testimony’s being trustworthy, i.e., showing what it purports to show)<sup>118</sup> with falsifiability (which, however, is entirely consistent with the testimony’s being plain false). The same year, in *Rogers v. Secretary of Health and Human Services*, the court reasons that “[f]or scientists, a new idea or explanation is not valid unless there is a possibility that empirical testing can prove it false and until it has withstood thoughtful efforts at falsification.”<sup>119</sup>

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<sup>113</sup> *United States v. Starzecpyzel*, 880 F. Supp. 1027, 1040 (S.D.N.Y. 1995) (citing Edward J. Imwinkelried, *The Next Step After Daubert: Developing a Similarly Epistemological Approach to Ensuring the Reliability of Nonscientific Expert Testimony*, 15 CARDOZO L. REV. 2271, 2294 (1994)) (emphasis added).

<sup>114</sup> 950 F. Supp. 1160, 1164 (S.D. Fla. 1996) (emphasis added) (citation omitted).

<sup>115</sup> *In re TMI Litig.* Cases Consol. II, 911 F. Supp. 775, 805 (M.D. Pa. 1996) (emphasis added).

<sup>116</sup> 126 F.3d 679, 685 (5th Cir. 1997) (citing Green, *supra* note 71, at 643, 645–46).

<sup>117</sup> 126 F. Supp. 2d 1090, 1127 (E.D. Tenn. 1999) (emphasis added).

<sup>118</sup> *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 590 n.9 (1993).

<sup>119</sup> *Rogers v. Sec’y of Health and Human Servs.*, No. 94-0089, 1999 WL 809824, at \*12 (Fed. Cl. Sept. 17, 1999) (emphasis added) [The court quotes Bert Black, Francisco J. Ayala & Carol Saffran-Brinks, *Science and Law in the Wake of Daubert: A New Search for Scientific Knowledge*, 72 TEX. L.R. 751, 756 (1994) (but it incorrectly references the TEX. L.J.)]. Black et al. write that “[t]esting either establishes or fails to establish false-

In *Tobin v. Smithkline Beecham Pharmaceuticals*, the court denied the defendant's motion to exclude plaintiff's expert testimony, holding that this testimony had been "tested to an extent sufficient to demonstrate . . . reliability . . ."<sup>120</sup> The same year, in *Cloud v. Pfizer*, the court excluded plaintiff's expert Dr. Johnstone, arguing that, while the proposition that Zoloft causes suicide is testable, the fact that he "[could] not point to one scientific study that supports his conclusion" showed that his testimony is unreliable.<sup>121</sup> Then there's the gloss on the first *Daubert* factor given in *Caraker v. Sandoz Pharmaceuticals*: "The hallmark of [*Daubert*'s] reliability prong is the scientific method, *i.e.*, the generation of testable hypotheses that are then subjected to the real-world crucible of experimentation, *falsification/validation*, and replication."<sup>122</sup> This passage, and especially that fused phrase "falsification/validation," encapsulates the crucial misunderstanding in a nutshell; it is cited verbatim in several subsequent cases.<sup>123</sup>

In one of those cases, *Soldo v. Sandoz Pharmaceuticals*, the court first runs through the epidemiological evidence presented: the "ERI study" (where the relative risk was "not statistically significant");<sup>124</sup> the "Witlin-Sibai study" (which showed a *decreased* risk of postpartum stroke in women taking the drug);<sup>125</sup> the "HCI study" (where

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hood; it never establishes *absolute* truth." *Id.* at 762 (emphasis added). Their word "absolute" hints that corroboration *might* establish provisional truth; and indeed, they continue, "[b]ecause the truth of scientific hypotheses can never be established conclusively, they can only be accepted contingently," and "scientists do not have the same degree of confidence in all hypotheses that have survived falsification." *Id.*

<sup>120</sup> *Estate of Tobin v. SmithKline Beecham Pharm.*, 164 F. Supp. 2d 1278 (D. Wyo. 2001).

<sup>121</sup> *Cloud v. Pfizer, Inc.*, 198 F. Supp. 2d 1118, 1135 (D. Ariz. 2001) (emphasis added).

<sup>122</sup> *Caraker v. Sandoz Pharm. Corp.*, 188 F. Supp. 2d 1026, 1030 (S.D. Ill. 2001) (emphasis added).

<sup>123</sup> See *Krutsinger v. Pharmacia Corp.*, 2004 U.S. Dist. LEXIS 30766, at \*10 (S.D. Ill. May 20, 2004) (using precisely these words from *Caraker*, but citing to *Daubert*); *Bickel v. Pfizer, Inc.*, 431 F. Supp. 2d 918, 922 (N.D. Ind., 2006); *Hardiman v. Davida, Inc.*, No. 2:05-CV-262-JM., 2007 WL 1395568, at \*2 (N.D. Ind. May 10, 2007); *Bauer v. Bayer A.G.*, 564 F. Supp. 2d 365, 380 (M.D. Pa. 2008); *Perry v. Novartis Pharm. Corp.*, 564 F. Supp. 2d 452, 459 (E.D. Pa. 2008).

<sup>124</sup> *Soldo v. Sandoz Pharm. Corp.*, 244 F. Supp. 2d 434, 455 (W.D. Pa. 2003).

<sup>125</sup> *Id.* at 455–56.

there were “huge amounts of uncertainty in the data”);<sup>126</sup> the “Kittner study” (where there was “no evidence whatsoever” in support of the plaintiff’s claim);<sup>127</sup> and the “Herings and Stricker study” (which “does not support” the plaintiff’s hypothesis).<sup>128</sup> Then, to conform to the language of *Daubert*, the court puts all this in terms of the plaintiff’s failure to falsify the null hypothesis—here, the hypothesis that any difference, in the sample studied, between the rate of postpartum stroke among women who take Parlodel and those who do not is the result of chance—and reasons that:

To “falsify” a hypothesis in this context means to prove that the “null hypothesis” — that Parlodel® has no effect on the risk of postpartum stroke—is false, i.e. that Parlodel® in fact significantly increases the risk of postpartum stroke. The failure of plaintiff’s experts to show any study proving that the null hypothesis has been falsified demonstrates that their causal hypothesis has not been *tested or verified* by the means of science.<sup>129</sup>

But as the phrase “tested or verified” suggests, what this really says is that the plaintiff’s experts have produced no statistically significant evidence supporting the claim that Parlodel increases the risk of postpartum stroke.

And—my personal favorite—in *Fuesting v. Zimmer, Inc.*, supposedly applying *Daubert*’s Popperian clause, the court writes that “Pugh did not *conduct any scientific tests or experiments to bolster his theory relating polyethylene delamination to gamma irradiation in air, nor did he produce or rely on any studies to verify his conclusions.*”<sup>130</sup>

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<sup>126</sup> *Id.* at 456.

<sup>127</sup> *Id.* at 457.

<sup>128</sup> *Id.*

<sup>129</sup> *Id.* (emphasis added).

<sup>130</sup> *Fuesting v. Zimmer, Inc.*, 421 F. 3d, 528, 536 (7th Cir. 2005) (emphasis added); see also *Robinson v. Garlock Equip. Co.*, 2009 WL 104197, at \*2-3 (W.D.N.Y. 2007) (denying motion to exclude expert testimony regarding an allegedly defective spigot on the grounds that “*Daubert* does not require that the hypothesis be tested by its proponent, only that it can be tested,” and that the fact that Dr. Quisnel “*could have bolstered his conclusions through conducting experiments*” goes to weight rather than

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Probably I should also mention the dog that *didn't* bark. Given that it was quite clearly Popper's intent to rule out Freud's and Adlers's psychoanalytical theorizing as *not* genuine science, *not* the real thing, one might expect to find post-*Daubert* federal courts routinely excluding expert psychiatric testimony on the grounds that it is untestable. But the only case I could find where the reasoning on psychiatric testimony is strictly Popperian was *United States v. Carrucci*.<sup>131</sup> Excluding proffered expert testimony to the effect that the same psychological problems that led to the defendant's compulsive gambling also led to his illegal securities trading, Judge Rakoff reasoned that "the psychological construct proffered by the defense is sufficiently flexible to accommodate even . . . calculated misconduct." And in a footnote, citing Popper's *Conjectures and Refutations*, he continued, "[p]ut differently, the construct suffers from being *unfalsifiable, and therefore unverifiable*."<sup>132</sup> The cited passage is exactly on point; Judge Rakoff, it appears, had actually read Popper, or at least the page of Popper that Justice Blackmun cites.

As in *United States v. Hines*, in *Kokoralies v. Illinois Dept. of Corrections*, we encounter a little judicial foray into philosophy of science. Ruling that there was no real basis for psychiatric evidence that the appellant had borderline personality disorder, Judge Zagel notes that Professor Allen disapproves of *Daubert* in part because he "prefers" Kuhn's philosophy of science to Popper's; and comments that "[i]f I had to choose between Popper and Kuhn I would pick Popper despite his flaws and so would nearly all scientists." But, he admits, the point may not be of much practical consequence, since what is seen in the courtroom is usually normal, not revolutionary, science.<sup>133</sup>

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admissibility) (emphasis added). Note that what *Daubert* requires is that evidence "can be (*and* has been) tested" (emphasis added), not "can be (*or* has been) tested." *Daubert v. Merrell Dow Pharms., Inc.*, 509 U.S. 579, 593 (1993).

<sup>131</sup> 33 F. Supp. 2d 302 (S.D.N.Y. 1999).

<sup>132</sup> *Id.* at 303 n.3 (emphasis added) (citing POPPER, *supra* note 5, at 37) (arguing that, because nothing could possibly falsify Freud's or Adler's theories, supposed "confirming" instances are really nothing of the kind). This observation is from the very bottom of the page; the sentence Justice Blackmun had quoted in *Daubert* is near the top of the same page.

<sup>133</sup> *Kokoralies v. Illinois Dept. of Corr.*, 963 F. Supp. 1473, 1489 n.9 (N.D. Ill. 1997) (The same note tells us that Judge Zagel took a logic class with Dudley Shapere.).

As in *United States v. Havvard*, in Judge Garza's concurring opinion in *Flores v. Johnson*, we find "testing" interpreted as referring to adversarial, rather than empirical, trials. Judge Garza argues that psychiatric predictions of future dangerousness flunk all the *Daubert* factors; and, regarding the first factor, reasons that the accuracy of such predictions *has* never really been tested—because such predictions *are not susceptible to "cross-examination and rebuttal."*<sup>134</sup>

But most cases involving psychiatric testimony follow the now-familiar pattern: courts take for granted that a theory's successfully withstanding testing indicates that it is reliable. For example, in *Isely v. Capuchin Province*, the court explains that:

[T]he witness should testify as to whether [his] theory can be, or has been, tested or corroborated and, if so, by whom and under what circumstances; *whether the theory has been proven out or not proven out* under clinical tests or some other accepted procedure *for bearing it out* . . . . [T]he witness must be able to show, through the use of reliable, viable extrinsic evidence, whether repressed memory or post-traumatic stress disorder is . . . accepted in the field of psychology . . . .<sup>135</sup>

The same year, *Isely* is cited in *Shahzade v. Gregory*, where the court also finds recovered memory testimony admissible: "[though] repressed memory, as is true with ordinary memories, 'cannot be tested empirically,' and may not always be accurate, however, the theory itself *has been established to be valid* . . . ." <sup>136</sup>

In *United States v. Hall*, the court finds that "testimony which is simply not amenable to the scientific method should not be subject to the strictures of *Daubert*" but treated as "specialized knowledge"; but qualifies this by noting that if testimony "posits an explanatory theory to show a conclusion or determine causation . . . this would

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<sup>134</sup> *Flores v. Johnson*, 210 F.3d 456, 464–65 (5th Cir. 2000) (Garza, J., concurring) (emphasis added).

<sup>135</sup> 877 F. Supp. 1055, 1064 (E.D. Mich. 1995) (emphasis added) (admitting expert testimony on post-traumatic stress syndrome, with limitations).

<sup>136</sup> *Shahzade v. Gregory*, 923 F. Supp. 286, 290 (D. Mass. 1996) (citing *Isely*, 877 F. Supp. at 1065) (second emphasis added) (ruling testimony on PTSD admissible).

normally require experimental *verification* . . . .<sup>137</sup> And in *Discepolo v. Gorgone*, denying a *Daubert* motion to exclude plaintiff's expert Dr. Pratt from testifying that the plaintiff suffered from PTSD, arguing that the psychiatric evaluation of alleged victims of child sexual abuse is "an inexact science at best,"<sup>138</sup> the court determined that Dr. Pratt's diagnosis had been found to be "*substantially accurate*."<sup>139</sup>

The next year, in *United States v. Thomas*, both sides presented evidence as to the level of risk that would be posed if the defendant were released from pre-trial detention. The court found Supervisory Special Agent Clemente's testimony for the prosecution inadmissible, on the grounds that he was "unable to demonstrate that his risk assessment methodology had been (or could be) tested,"<sup>140</sup> and argued that although, in this context, it is difficult to collect empirical data, "this difficulty cannot, by itself, render a risk assessment methodology reliable or exempt it from any sort of *testing or validation*."<sup>141</sup> However, Dr. Blumberg's testimony for the defendant was found admissible, on the grounds that "there is *substantial support* from a variety of sources, based in part on empirical data" establishing the reliability of his approach.<sup>142</sup>

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"But," you may be thinking, "enough already! So federal judges are not right on top of all the vagaries of twentieth-century philosophy of science. It would be more remarkable, surely, if they *were* fully conversant with the work of Popper, Hempel, et al." Indeed. What is much more interesting, as I will argue in conclusion, is that the conception of science we find implicit in many of these rulings, albeit in an inarticulate and half-baked form, is more plausible than the official Logical Negativist account.

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<sup>137</sup> *United States v. Hall*, 974 F. Supp. 1198, 1200-01 (C.D. Ill. 2005) (emphasis added).

<sup>138</sup> *Discepolo v. Gorgone*, 399 F. Supp. 2d 123, 126 (D. Conn. 2005).

<sup>139</sup> *Id.* at 127 (emphasis added).

<sup>140</sup> *United States v. Thomas*, No. CRIM. CCB-03-0150., 2006 WL 140558, at \*19 (D. Md. Jan. 13, 2006).

<sup>141</sup> *Id.* (emphasis added).

<sup>142</sup> *Id.* at \*16 (emphasis added).

## VI. THE STING IN THE TALE

To be sure, it is a mistake to conflate “scientific” and “reliable,” as Justice Blackmun apparently did; and it is at least arguable that, in requiring courts to assess the reliability of expert testimony, *Daubert* fudged the legal line between questions of the admissibility of evidence (a matter for courts to determine) and questions of its weight (a matter for juries to decide). Moreover, other critics have suggested numerous flaws in, and unintended consequences of, the *Daubert* ruling.<sup>143</sup> Nevertheless, all these issues aside, in any case involving scientific testimony the question of reliability is bound to arise, and must be determined *somehow*. But if Popper’s account were true, the legal system’s interest in the question of the reliability of scientific testimony would be completely misconceived.<sup>144</sup>

I don’t believe that this concern *is* misconceived; and I don’t believe you do, either.<sup>145</sup> Think of Kerry Kotler, who was freed from prison in 1993 after DNA analysis revealed that he was innocent of the rape for which he had served eleven years of a twenty-five to fifty-year sentence; and three years later was convicted of another rape—of which DNA evidence showed he was guilty.<sup>146</sup> If you believe, as I do, that there is such a thing as objectively more and less reliable evidence, it will seem to you that in this instance justice was (probably)<sup>147</sup> well-served by science; but if you believe, as Popper

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<sup>143</sup> See, e.g., Lisa Heinzerling, *Doubting Daubert*, 14 J.L. & POL’Y, 65, 65 (2006) (arguing that “*Daubert* . . . is dubious, for many reasons”).

<sup>144</sup> Indeed, if, as I have argued, Popper’s account is really a kind of closet skepticism, the legal system’s interest in the reliability of testimony generally would be misconceived. I can’t pursue this more general issue here, but see HAACK, *supra* note 30, at 252, where I made the same point, in passing, with respect to Richard Rorty’s repudiation of epistemology.

<sup>145</sup> Nor do I believe that even Popperians *really* believe we never have rational grounds for our expectations. See Mellor, *supra* note 91, at 196 (“Why will Popperians not admit to such beliefs, which they reveal every time they turn on the light or use the telephone? As Carnap would say, none are so inductively blind as those who pretend they cannot see . . .”).

<sup>146</sup> See John T. McQuiston, *Prosecutor Says DNA Evidence May Free Man*, N.Y. TIMES, Dec. 1, 1992, at B7; John T. McQuiston, *Man Freed After a DNA Test is Sentenced in a Second Rape*, N.Y. TIMES, Oct. 24, 1997, at B4, available at <http://www.nytimes.com/1997/10/24/nyregion/man-freed-after-a-dna-test-is-sentenced-in-a-second-rape.html>.

<sup>147</sup> “Probably” because, without knowing a lot more than I do, I can’t say with full confidence that the DNA analyses in question were well-conducted, the chain of

professes to, that there is no such thing as objectively supportive evidence, you will be obliged to conclude that in this—and in *every* legal case turning on scientific evidence—the hope that science could contribute to justice is vain. This doesn't show that Popper is wrong; but it does show how radical the consequences of Popper's Logical Negativist epistemology would be for our—or any<sup>148</sup>—legal system. Nevertheless, I believe courts' misinterpretation of Popper implicitly relies on inarticulate assumptions that are more plausible than Popper's official story.

As we saw, the predominant pattern of federal rulings calling on the first *Daubert* factor is that courts realize that they need to know not only whether the basis of proffered expert testimony is empirically testable, but more importantly whether it has actually been tested, and if it has, how well it has performed on those tests; and, if a theory has succeeded when tested, they take this to be evidence, albeit less than absolutely conclusive evidence, that it is

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evidence impeccable, etc. It appears that both cases were messy, to say the least. See Peter J. Boyer, *DNA on Trial*, NEW YORKER (Jan. 17, 2000), at 42.

<sup>148</sup>In this context it is worth noting that *Daubert* (or something much like it) has been adopted by thirty-one states (Alaska, Arkansas, Colorado, Connecticut, Delaware, Georgia, Idaho, Iowa, Kentucky, Louisiana, Maine, Massachusetts, Michigan, Mississippi, Montana, Nebraska, New Hampshire, New Mexico, Ohio, Oklahoma, Oregon, Rhode Island, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, West Virginia, and Wyoming) and by the Military Courts. CAMPBELL & LORANDOS, *supra* note 6, n.6. According to a report published in 1999, only 3% of judges in *Daubert* states understood the concept of falsification; some, reportedly, explained it as “If there is white-out on the page, then the document has been falsified.” STATE JUSTICE INSTITUTE, A JUDGE'S DESKBOOK ON THE BASIC PHILOSOPHIES AND METHODS OF SCIENCE 31 (1999). Unfortunately, the authors of the Deskbook don't fully understand Popper either; for they write that according to Popper, “predictions are . . . compared with observations to see whether the theory is supported.” *Id.* at 28 (emphasis added).

*Daubert* has also influenced other, non-U.S. jurisdictions. For example, in a 2007 ruling on compulsory DNA testing in paternity cases, the Supreme Court of Mexico suggested indicia of the reliability of scientific testimony which (though the Court didn't say this) are what we know as the *Daubert* factors: admissible scientific evidence, the Court argues, must be both relevant and reliable (“fidedigna,” “trustworthy”); and it lists (among other indicia of reliability familiar from *Daubert*), that the evidence should have been “sujeta a pruebas empiricas, o sea, que la misma haya sido sujeta a pruebas de refutabilidad” (“subject to empirical testing, i.e., that it has been subjected to tests of refutability”). 25 S.J.F. 111, 125 et seq. (Vol. 1, 9th Epoch, Mar. 2007, Mex. Sup. Ct. First Chamber).

reliable—as just about everybody, except for Popper and his most loyal followers,<sup>149</sup> would do. I agree.

It should by now be clear, however, that it is impossible just to add, to Popper's official story, that a claim that has been tested but not falsified is thereby shown to be to some degree reliable; for this would transform Logical Negativism into an idea utterly different from what Popper proposed.<sup>150</sup> So it behooves me at least to sketch what a reconstruction of the philosophy of science implicit in courts' rulings might look like. This reconstructed account will, of course, be thoroughly *unlike* hard-line Popperism (though it will accommodate some elements from the shadow Popper): in particular, it will be, not skeptical, but fallibilist; it will focus less on demarcation than on continuities between scientific and other kinds of empirical inquiry; and it will be, not purely logical, but worldly—i.e., not confined exclusively to statements and their logical relations, but also giving a role to the world and to scientists' interactions with the world. In short, it will be much like the Critical Common-sensist account I developed in *Defending Science—Within Reason*.<sup>151</sup>

The first thing to do is to *get over the Popperian preoccupation with demarcation*:

<sup>149</sup> And, ironically enough, the wildest of the irrationalists against whom Popper set himself. See STOVE, *supra* note 61 (presenting Popper as the father of late twentieth-century scientific irrationalism); Alan Olding, *Popper for Afters*, 19 QUADRANT 21 (1999) (arguing that a historicist brand of relativism was “already a bit more than latent in Popper”); Koertge, *supra* note 12 (suggesting that constructivist postmodernists might find solace in Popper's philosophy of science). In the now-famous words of Louis-Sébastien Mercier, “les extrêmes se touchent” (“extremes touch each other”). LOUIS-SÉBASTIEN MERCIER, TABLEAU DE PARIS (rev. ed. 1782). The phrase is the title of chapter 48 of volume 4.

<sup>150</sup> Nor would it be possible to rely instead on Hempel's inductivist philosophy of science. For—though it is less grossly unsuitable for the Supreme Court's purposes than Popper's account—even if Hempel's “logic of confirmation” were viable (which he himself came to doubt late in life), it would be hopelessly inadequate to cope with the enormously complex congeries of scientific evidence now routinely proffered in toxic-tort and other cases. See Carl G. Hempel, *Studies in the Logic of Confirmation*, in ASPECTS OF SCIENTIFIC EXPLANATION AND OTHER ESSAYS IN PHILOSOPHY OF SCIENCE (The Free Press 1965) (1945); CARL G. HEMPEL, *The Irrelevance of the Concept of Truth for the Critical Appraisal of Scientific Theories*, in CARL G. HEMPEL: SELECTED PHILOSOPHICAL ESSAYS 75 (Richard Jeffreys ed., 2000) (1990).

<sup>151</sup> SUSAN HAACK, *DEFENDING SCIENCE—WITHIN REASON: BETWEEN SCIENTISM AND CYNICISM* (2003).

- Although (no doubt because of the remarkable successes of the natural sciences) the words “science,” “scientific,” and their cognates are often used as generic terms of epistemic praise, this honorific usage is misleading: there is bad scientific work as well as good. Moreover, rather than dismissing bad scientific work with the generic accusation, “pseudoscience,” it is always better to say what, specifically, is wrong with the work.
- That a purported explanation rules out some possible upshots is, not a sign that it is scientific in particular, but a sign that it actually is explanatory.
- Willingness to take negative evidence seriously is a mark, not of the scientist in particular, but of the honest inquirer generally, be he a scientist, a historian, a legal or a literary scholar, or whatever.
- The word “science” (or, better, the phrase “the sciences”) is best construed as referring to a loose federation of kinds of inquiry into natural and social phenomena; and as distinguished from such other, legitimate kinds of inquiry as legal or literary scholarship, history, metaphysics, mathematics, etc., by their subject-matter.

The next step is to *re-think the whole idea of “Scientific Method,”* starting by distinguishing (1) the procedures followed by all serious empirical inquirers, and (2) the specialized instruments, techniques, etc., gradually developed by the various sciences.

- Any serious empirical inquirer will proceed by making an educated conjecture as to the explanation of a puzzling phenomenon or event, figuring out the consequences of the conjecture’s being true, checking how well those consequences stand up to the evidence he has and any further evidence he can obtain, and then using his judgment whether to keep the conjecture, modify it, drop it and start again, or wait for more evidence.
- Over time, the various sciences have gradually developed instruments of observation, techniques of extraction, purification, titration, etc., mathematical tools like the calculus,

statistical techniques, computer programs, incentives to honesty and evidence-sharing, . . . etc., etc., to amplify and refine the ways of all serious empirical inquiry.

Obviously, the procedures of all serious empirical inquiry are not used *only* by scientists, and the gradually evolving specialized scientific tools, techniques, etc., often local to a specific field of science, are not used by *all* scientists. So neither can be identified with “the Scientific Method”; and yet, together, they begin to explain how the sciences have been as successful as they have.

Next, we must *set aside Popper’s conventionalism about the empirical basis of science*. What we need is an account that distinguishes observations from statements reporting what is observed, but replaces a sharp distinction of statements into the observational and the theoretical by a continuum of the more and the less observational; and that recognizes that observation can give a scientist grounds, albeit fallible grounds, for believing that (say) the needle on the dial points to seven, or there is a black swan on the pond, and so can contribute to the solidity of the evidence for a scientific theory, i.e., to how warranted the theory is.

- It is in part scientists’ observations that justify them in accepting claims like “Here is a glass of water”; even though, because even so simple a statement as this carries some theoretical baggage, only in part.
- Because all empirical inquiry ultimately depends on people’s sensory interactions with the world, the degree to which a claim is warranted by the evidence possessed by a person at a time is primary. The degree to which a claim is warranted by the evidence shared by a group of people at a time, and the degree to which a claim is warranted by the evidence available at a time, must be understood as (legitimate but) derivative.<sup>152</sup>

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<sup>152</sup> This contrasts, once again, with Popper’s approach, which takes the impersonal conception of warrant as primitive, and doesn’t so much as acknowledge the legitimacy of the personal conception.

So we will have to embark on the difficult task of *getting a grip on the complexities of evidence and the determinants of evidential quality*.

- The evidence with respect to any serious scientific claim ramifies in all directions, rather as the entries in a crossword puzzle do.
- How well evidence warrants a claim depends on how well it supports it (supportiveness); how secure it is, independent of the claim in question (independent security); and how much of the relevant evidence it includes (comprehensiveness).<sup>153</sup>

Because the determinants of evidential quality are multi-dimensional, they will not necessarily yield a linear ordering; moreover, if there is insufficient evidence either way, neither p nor not-p may be warranted to any degree. So the next step is to *distinguish epistemic likelihoods from probabilities*.

- The better the evidence is with respect to a theory, the likelier that it is true. But these are *epistemic* likelihoods, and cannot be construed in terms of the mathematical calculus of probabilities.

As this reveals, it is possible to repudiate probabilism without, as Popper supposes, *also* rejecting the idea of supportive evidence or well-warranted theory.

Next, we need *an understanding of what makes evidence supportive with respect to a claim*.

- How well evidence supports a claim depends on how tightly the two fit together to form an explanatory account.

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<sup>153</sup> See also Susan Haack, *Proving Causation: The Holism of Warrant and the Atomism of Daubert*, 4 J. OF BIOMEDICAL & HEALTH L. 253 (2008) (applying the analysis of evidence sketched here, and spelled out in DEFENDING SCIENCE, *supra* note 151, to show that some combinations of pieces of evidence, none of which would be sufficient by itself to establish general causation to the required degree of proof, can do so jointly).

Explanation is vocabulary-dependent, since a genuinely explanatory account requires general terms identifying real kinds of things; so supportiveness is not a purely formal, logical relation, but depends in part on the fit of scientific vocabulary to the world.

So the final step is to *stop thinking of scientific rationality in purely logical terms*, and acknowledge that successful scientific inquiry, like successful empirical inquiry of any kind, is a matter in part of our interactions with the world, and so is possible only because we, and the world, are a certain way.

- Our senses give us information about particular things and events in the world, and these things and events are of kinds, kinds the behavior of members of which falls into patterns—the patterns of natural laws.
- And so it is possible, by making guesses as to the possible explanation of puzzling phenomena or events, devising ways to check them, and seeing how well they stand up to evidence—even though there will be many more false starts than successful guesses, and even though every step forward will be fallible and imperfect—to make contributions to the still only very partially-completed cathedral of scientific knowledge.<sup>154</sup>

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Unlike Popper's, this account acknowledges the legitimacy of questions about the reliability of expert testimony. Moreover, it enables us to distinguish "reliable" and "scientific," and to recognize that not all reliable expert testimony is scientific, nor all scientific testimony reliable; and it suggests (precisely in line

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<sup>154</sup> Compare this, from the same paragraph in which Peirce anticipates Popper's "swamp" analogy:

The translations of the cuneiform inscriptions . . . began in mere guesses, in which their authors could have no real confidence. Yet by piling new conjecture upon former conjectures apparently verified, this science has gone on to produce under our very eyes a result so bound together by the agreement of the readings with one another, with other history, and with known facts of linguistics, that we are unwilling any longer to apply the word *theory* to it."

PEIRCE, *supra* note 52, at paragraph 589 (emphasis in original).

with the Supreme Court's ruling in *Kumho Tire*)<sup>155</sup> that what matters legally should be whether expert testimony is reliable, whether or not it is scientific. It reveals that any simple verbal formula supposedly encapsulating "the Scientific Method" is likely to be a distraction from the real complexities of evidence and from the multiple, interconnected questions relevant in assessing reliability. It tells us that the fact that a theory or technique has not been tested is a sign that investigation has not been as thorough or as honest as it should have been; but also that the fact that a theory or technique has performed successfully under rigorous testing is an indication of its reliability. And it tells us (also in line with *Kumho Tire*) that the kinds of test that are appropriate will vary depending on the nature of the evidence in question,<sup>156</sup> and may—for example in the case of rival tests of statistical significance, or of the conclusions to be drawn from a DNA analysis—itsself depend on other scientific knowledge. I believe this conception of science is both philosophically more plausible, and legally more helpful,<sup>157</sup> than the Popperian theory on which post-*Daubert* courts ostensibly rely.

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<sup>155</sup> *Kumho Tire v. Carmichael*, 526 U.S. 137, 147 (1999) (arguing that it is the word "knowledge" in FED. R. EVID. 702, and not the word "scientific," that establishes the standard of evidentiary reliability).

<sup>156</sup> *Id.* at 150 (arguing that "we can neither rule in nor rule out, for all cases and for all times, the applicability of the factors mentioned in *Daubert* . . . Too much depends upon the particular circumstances of the particular case at issue").

<sup>157</sup> My theoretical account will not, of course, provide a simple formula that judges could apply to assess the worth of proffered scientific testimony; *there can be no such formula*. There is no easy substitute for appraisal of the nitty-gritty details of the specific scientific work concerned. See my discussion of "peer review and publication" in *Peer Review and Publication: Lessons for Lawyers*, 36 *STETSON L. REV.* 789 (2007), and of "litigation-driven science" in *What's Wrong With Litigation-Driven Science? An Essay in Legal Epistemology*, 38 *SETON HALL L. REV.* 1053 (2008). My account does, however, as indicated in note 153 above, suggest how best to think about, for example, the joint weight of complex congeries of causation testimony.