

**Disentangling *Daubert*:  
An Epistemological Study in Theory and Practice<sup>1</sup>**

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Sometimes the word ["science"] degenerates into a vague honorific, synonymous with the advertiser's "reliable" or "guaranteed"... [JACQUES BARZUN]<sup>2</sup>

In *Frye* (1923) the D.C. Court upheld the exclusion of testimony of the results of a then-new blood-pressure deception test on the grounds that novel scientific testimony "crosses the line between the experimental and the demonstrable," and so is admissible, only if it is "sufficiently established to have gained general acceptance in the particular field to which it belongs."<sup>3</sup> Ignored for a decade, rarely cited for a quarter-century, over time the "*Frye* test" became increasingly influential, until by the early 1980s it had been adopted by 29 states.

In 1975, however, newly-enacted Federal Rules of Evidence had set a seemingly less restrictive standard: the testimony of a qualified expert, including a scientific expert, is admissible provided it is relevant (unless it is excluded, under Rule 403, on grounds of unfair prejudice, waste of time, or confusing or misleading the jury). In *Barefoot*, a 1983 constitutional case, the Supreme Court affirmed that the rights of a Texas defendant were not violated by the jury's being allowed to hear psychiatric testimony of his future dangerousness at the sentencing hearing -- even though an amicus brief from the American Psychiatric Association reported that 2 out of 3 such predictions are mistaken. Writing for the majority, Justice White observed that state and federal rules of evidence "anticipate that relevant, unprivileged testimony should be admitted and its weight left to the fact-finder, who would have the benefit of cross-examination and contrary evidence by the opposing party."<sup>4</sup> Justice Blackmun wrote an angry dissent.

In 1991, amid increasing public concern that the tort system was getting out of hand, Peter Huber argued in his influential *GALILEO'S REVENGE* that under the Federal Rules worthless "junk science," which would have been excluded by the *Frye* test, was flooding the courts. In 1992 proposals to tighten up the Federal Rules were before Congress. In 1993 the Supreme Court issued its ruling in *Daubert*<sup>5</sup> -- the first case in its 204-year history where the central questions concerned the admissibility of scientific testimony. The *Frye* rule arose in a criminal case, and had for most of its history been cited in criminal cases;

but *Daubert* was a tort action in which the trial court had relied on *Frye* in excluding the plaintiffs' experts' testimony that the morning-sickness drug Bendectin was teratogenic. So the Supreme Court was to determine whether the FRE had superseded *Frye*, and in particular how Rule 702 was to be interpreted.

Yes, Justice Blackmun wrote for the majority, the FRE *had* superseded *Frye*; but the Rules themselves require judges to screen proffered expert testimony not only for relevance, but also for reliability. In doing this courts must look, not to an expert's conclusions, but to his "methodology," to determine whether proffered evidence is really "scientific ... knowledge," and hence reliable. As to what that methodology is, citing an article by law professor Michael Green citing Karl Popper, and quoting an observation of Carl Hempel's for good measure, the *Daubert* ruling suggests four factors that courts might use in assessing reliability: "falsifiability," i.e., whether proffered evidence "can be and has been tested"; the known or potential error rate; peer review and publication; and (in a nod to *Frye*), acceptance in the relevant community.<sup>6</sup>

In dissent, however, pointing out that the word "reliable" nowhere occurs in the text of Rule 702, Justice Rehnquist anticipated difficulties over whether and if so how *Daubert* should be applied to non-scientific expert testimony; worried aloud that federal judges were being asked to become amateur scientists; and questioned the wisdom of his colleagues' readiness to get involved in philosophy of science. I think he was right to suspect that something was seriously amiss; in fact, what I shall have to say here might be read as an exploration, amplification, and partial defense of his reservations about that philosophical excursus.

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Apparently equating the question of whether expert testimony is reliable with the question of whether it is genuinely scientific, taking for granted that there is some scientific "methodology" which, faithfully followed, guarantees reliable results, and casting about for a philosophy of science to fit this demanding bill, the *Daubert* Court settled on an unstable amalgam of Popper's and Hempel's very different approaches -- neither of which, however, is suitable to the task at hand.

Popper describes his philosophy of science as "Falsificationist," by contrast with the Verificationism of the Logical Positivists, because his key theme is that scientific statements can never be shown conclusively to be true, but can sometimes be shown conclusively to be false. Hence his criterion of demarcation: to be genuinely scientific, a statement must be "testable" -- meaning, in Popper's mouth, "refutable" or "falsifiable," i.e., susceptible to evidence that could potentially show it to be false (if it is false). Curiously, Popper acknowledged from the beginning that his criterion of demarcation is a "convention"; and in 1959, in his Introduction to the English edition of THE LOGIC OF SCIENTIFIC DISCOVERY, affirmed that scientific knowledge is continuous with common-sense knowledge.<sup>7</sup> Nevertheless, his whole philosophy of science turns on his criterion of demarcation. Falsifiability is to discriminate real empirical science, such as Einstein's

theory of relativity, from pre-scientific myths, from non-empirical disciplines like pure mathematics or metaphysics, from non-scientific disciplines like history, and from such pseudo-sciences as Freud's and Adler's psychoanalytic theories and Marx's "scientific socialism."<sup>8</sup> Falsifiability is also central to Popper's account of the method of science as "conjecture and refutation": making a bold, highly falsifiable guess, testing it as severely as possible, and, if it is found to be false, giving it up and starting over rather than protecting it by *ad hoc* or "conventionalist" modifications. (This readiness to accept falsification and eschew *ad hoc* stratagems is Popper's "methodological criterion" of the genuinely scientific.)

Popper also describes his philosophy of science as "Deductivist," by contrast with "Inductivism," whether in the strong, Baconian form that posits an inductive logic for arriving at hypotheses or in the weaker, Logical Positivist form that posits an inductive logic of confirmation. According to Popper, Hume showed long ago that induction is unjustifiable. But science doesn't need induction; the method of conjecture and refutation requires only deductive logic -- specifically, *modus tollens*, the rule invoked when an observational result predicted by a theory fails.

Theories which have been tested but not yet falsified are "corroborated," degree of corroboration at a time depending on the number and severity of the tests passed. That a theory is corroborated, to however high a degree, doesn't show that it is true, or even probable; indeed, the degree of testability of a hypothesis is *inversely related* to its degree of logical probability.<sup>9</sup> Corroboration is not a measure of verisimilitude, but at best an indicator of how the verisimilitude of a theory *appears*, relative to other theories, at a time;<sup>10</sup> and that a theory is corroborated doesn't mean that it is rational to believe it. (It *does* mean, Popper writes, that it is rational to prefer the theory as the basis for practical action; not, however, that there are good reasons for thinking the theory will be successful in future -- there *can be no* good reasons for believing this.<sup>11</sup> So it seems that all this "concession" amounts to is that in deciding how to act we can do no better than go with theories we don't so far know to be false.)

The first problem with the *Daubert* Court's reliance on Popper is that applying his criterion of demarcation is no trivial matter; as Justice Rehnquist pointed out, observing wryly that, since *he* didn't really know what is meant by saying that a theory is "falsifiable," he doubted federal judges would, either.<sup>12</sup> Indeed, Popper himself doesn't seem quite sure how to apply his criterion. Sometimes, for example, he says that the theory of evolution is not falsifiable, and so is not science; at one point he suggests that "survival of the fittest" is a tautology, or "near-tautology," and elsewhere that evolution is really a historical theory, or perhaps metaphysics. Then he changes his mind: evolution *is* science, after all.<sup>13</sup> It's ironic; for Popper's criterion of demarcation had already found its way into the U.S. legal system, a decade before *Daubert*, in a 1982 first-amendment case: *MacLean v. Arkansas Board of Education*, where Michael Ruse's testimony that creation science is not science, by Popper's criterion, but the theory of evolution is, apparently persuaded Judge Overton.<sup>14</sup>

But there is an even more serious problem with the *Daubert* Court's reliance on Popper, of

which Justice Rehnquist doesn't seem aware: Popper's philosophy of science is signally inappropriate to the Court's concern with reliability. When Popper describes his approach as "Critical Rationalism," it is to emphasize that the rationality of the scientific enterprise lies in the susceptibility of scientific theories to criticism, i.e., to testing, and potentially to falsification, *not* in their verifiability or confirmability. True, early on Carnap translated Popper's word "Bewahrung" by "confirmation"; and for a while, thinking the issue merely verbal, Popper let it go -- even, occasionally, using "confirm" himself. But in a footnote to the English edition of *THE LOGIC OF SCIENTIFIC DISCOVERY* he comments that this had been a bad mistake on his part, conveying the false impression that a theory's having been corroborated means that it is probably true.<sup>15</sup> Except for the weak moments when he condoned Carnap's (mis)translation, Popper insisted that corroboration must not be confused with confirmation. The degree of corroboration of a theory represents its past performance only, and "*says nothing whatever about future performance, or about the 'reliability' of a theory*"; even the best-tested theory "is not 'reliable'"<sup>16</sup> -- so scornful is Popper of the concept of reliability that he refuses even to use the word without putting it in precautionary scare quotes! Reiterating that he puts the emphasis "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 the conclusions of these inferences," Popper specifically identifies Hempel as representative of those inductivists with whom he disagrees.<sup>17</sup>

Hempel is not, perhaps, the prototypical inductivist: he describes the method of science as "hypothetico-deductive"; he affirms that scientific claims should be subject to empirical check or testing; and he doesn't follow Reichenbach and Carnap in explaining confirmation by appeal to the calculus of probabilities. Nevertheless, Popper is surely right to see Hempel's approach as very significantly at odds with his own: Hempel is not centrally concerned with demarcating science; he questions the supposed asymmetry between verification and falsification, and argues that Popper's criterion "involves a very severe restriction of the possible forms of scientific hypotheses," e.g., in ruling out purely existential statements;<sup>18</sup> when he speaks of "testing" he envisages both disconfirmation *and confirmation* of a hypothesis; and one of his chief projects was to articulate the "logic of confirmation," i.e., of the support of general hypotheses by positive instances.

Apparently the Supreme Court hoped, by combining Hempel's account of confirmation with Popper's criterion of demarcation, to craft a crisp test to identify genuine, and hence reliable, science. But, though Hempel's philosophy of science is more positive than Popper's, it isn't much more help with the question of reliability. For one thing, the confirmation of generalizations by positive instances which preoccupies Hempel is just too simplified to apply to the enormously complex congeries of epidemiological, toxicological, etc., etc., evidence at stake in a case like *Daubert*. For another, Hempel himself seems eventually to have concluded (rightly, I believe) that the "grue" paradox shows that confirmation isn't a purely syntactic or logical notion after all,<sup>19</sup> and late in life began to think that maybe Kuhn had been on the right track.<sup>20</sup>

But the most fundamental problem is that what Hempel offered was an account of supportiveness of evidence, or as he said, of "relative confirmation," the *relation between* observational evidence and hypothesis, expressible as "E confirms H [to degree n]," or "H is confirmed [to degree n] by evidence E." This, as Hempel acknowledged, falls short of an account of "absolute confirmation," the warrant of a scientific claim, which would be expressed in non-relative terms, as "H is confirmed [to degree n], period." To discriminate reliable testimony from unreliable, however, would require an account of the non-relative concept -- which Hempel doesn't supply.

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So, the *Daubert* Court mixes up its Hoppers and its Pempels; but isn't this just a slip, of merely scholarly interest? No: it is symptomatic of the serious misunderstanding of the place of the sciences within inquiry generally revealed by the Court's equation of "scientific" and "reliable."

So successful have the natural sciences been that the words "science," "scientific," and "scientifically" are often used as generic terms of epistemological praise, meaning vaguely "strong, reliable, good" -- as, in television advertisements, actors in white coats urge viewers to get their clothes cleaner with new, "scientific," Wizzo. This honorific usage is unmistakably at work in the *Daubert* ruling; indeed, it seems to be implicit even in the way Justice Blackmun writes of "scientific ... knowledge," strategically excising a significant phrase from the reference in FRE 702 to "scientific or other technical knowledge," and apparently signalling an expectation that a criterion of the genuinely scientific will also discriminate reliable testimony from unreliable.

If "scientific" is used honorifically, it is a tautology that "scientific" = "reliable"; but this tautology, obviously, is of no help to a judge trying to screen proffered scientific testimony. If "scientific" is used descriptively, however, "scientific" and "reliable" come apart: for, obviously, physicists, chemists, biologists, medical scientists, etc., are sometimes incompetent, confused, self-deceived, dishonest, or simply mistaken, while historians, detectives, investigative journalists, legal and literary scholars, plumbers, auto mechanics, etc., are sometimes good investigators. In short, not all, and not only, scientists are reliable inquirers; and not all, and not only, scientific evidence is reliable. Nor is there a "scientific method" in the sense the Court assumed: no uniquely rational mode of inference or procedure of inquiry used by all scientists and only by scientists. Rather, as Einstein once put it, scientific inquiry is "nothing but a refinement of our everyday thinking,"<sup>21</sup> superimposing on the inferences, desiderata, and constraints common to all serious investigation a vast variety of constantly evolving local ways and means of stretching the imagination, amplifying reasoning power, extending evidential reach, and stiffening respect for evidence.

*Every* kind of empirical inquiry, from the simplest everyday puzzling over the causes of delayed buses or spoiled food to the most complex investigations of detectives, of historians, of legal and literary scholars, and of scientists, involves making an informed

guess about the explanation of some event or phenomenon, figuring out the consequences of its being true, and checking how well those consequences stand up to evidence. This is the procedure of all scientists; but it is not the procedure *only* of scientists. Something like the "hypothetico-deductive method," really *is* the core of all inquiry, scientific inquiry included. But it is not distinctive of scientific inquiry; and the fact that scientists, like inquirers of every kind, proceed in this way tells us nothing substantive about whether or when their testimony is reliable.

The sciences have extended the senses with specialized instruments; stretched the imagination with metaphors, analogies, and models; amplified reasoning power with numerals, the calculus, computers; and evolved a social organization that enables cooperation, competition, and evidence-sharing, allowing each scientist to take up his investigation where others left off. Astronomers devise ever more sophisticated telescopes, chemists ever more sophisticated techniques of analysis, medical scientists ever more sophisticated methods of imaging bodily states and processes, and so on; scientists work out what controls are needed to block a potential source of experimental error, what statistical techniques to rule out a merely coincidental correlation, and so forth. But these scientific "helps" to inquiry are local and evolving, not used by *all* scientists.<sup>22</sup>

You may object that, since I have acknowledged that scientific inquiry is continuous with everyday empirical inquiry, I have in effect agreed with Popper that science is an extension of common sense. Indeed, I think science *is* well-described, in Gustav Bergmann's wonderfully evocative phrase, as the Long Arm of Common Sense. But the continuity is not between the content of scientific and of common-sense knowledge, but between the basic ways and means of everyday and of scientific inquiry; and it is precisely because of this continuity that the Popperian preoccupation with the "problem of demarcation" is a distraction.

Or you may object that the *Daubert* Court's Popperian advice that courts ask whether proffered scientific testimony "can be and has been tested" surely is potentially helpful. This is true; but it is no real objection. "Check whether proffered testimony has been tested" *is* very good advice when a purported expert hasn't made even the most elementary effort to check how well his claims stand up to evidence: such as the knife-mark examiner in *Ramirez*,<sup>23</sup> who testified that he could infallibly identify this knife, to the exclusion of all other knives in the world, as having made the wound -- though no study had established the assumed uniqueness of individual knives, and his purported ability to make such infallible identifications was untested. This is not, however, because falsifiability is the criterion of the scientific, but because *any* serious inquirer is required to seek out all the potentially available evidence, and to go where it leads, even if he would prefer to avoid, ignore, or play down information that pulls against what he hopes is true.

Yes, this is a requirement on scientists; as Darwin recognized when he wrote in his autobiography that he always made a point of recording recalcitrant examples and contrary arguments in a special notebook, to safeguard against his tendency conveniently to forget negative evidence.<sup>24</sup> But it is no less a requirement on other inquirers, too; as we all

realized a few years ago when a historian who announced that he had evidence that Marilyn Monroe had blackmailed President Kennedy turned out to have ignored the fact that the supposedly incriminating letters were typed with correction ribbon, and that the address included a zip code -- when neither existed at the time the letters were purportedly written!<sup>25</sup>

"Non-science" is an ample and diverse category, including the many human activities other than inquiry, the various forms of pseudo-inquiry, inquiry of a non-empirical character, and empirical inquiry of other kinds than the scientific; and of course there are plenty of mixed and borderline cases. The honorific use of "science" and its cognates tempts us -- like the *Daubert* Court -- to criticize poorly-conducted science as not really science at all; but "not scientific" is as unhelpful as generic epistemic criticism as "scientific" is as generic epistemic praise. The pejorative tone of the phrase "pseudo-science," which presumably refers to activities which purport to be science but aren't really, derives in part from its imputation of false pretenses, and in part from the favorable connotations of "scientific." But rather than sneering unhelpfully that this or that work is "pseudo-scientific," it is always better to specify what, exactly, is wrong with it: that it is not honestly or seriously conducted; that it rests on vague or flimsy assumptions -- assumptions there is no way to check, or for which there is no good evidence; that it seeks to impress with decorative or distracting mathematical symbolism or elaborate-looking apparatus; that it fails to take essential precautions against experimental error; or whatever.

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So, the *Daubert* Court's philosophy of science was muddled; but haven't subsequent Supreme Court rulings cleared things up? Not exactly: it would be more accurate to say that in *Joiner* (1997) and *Kumho* (1999) the Supreme Court quietly backed away from *Daubert's* confused philosophy of science.<sup>26</sup> At any rate, those references to Hepper, Pompel, falsifiability etc., so prominent in *Daubert*, are conspicuous by their absence from *Joiner* and *Kumho*. But there are points of epistemological interest.

In *Joiner* there is a bit of a kerfuffle about "methodology": Mr. Joiner's attorneys had argued that the lower court erred in excluding their proffered expert testimony because, instead of focusing exclusively on their experts' methodology -- which, they maintain, was the very same "weight of evidence" methodology used by the other party's (G.E.'s) experts -- improperly concerned itself with the experts' conclusions. Apparently anxious to sidestep this argument, the *Joiner* Court (with the exception of Justice Stevens) flatly denies the legitimacy of the distinction between methodology and conclusions. Opining that this is No Real Distinction, the Court sounds like nothing so much as a conclave of medieval logicians; but given their citation to *Paoli*,<sup>27</sup> it seems likely that they didn't really intend to make a profound metaphysical pronouncement, only to acknowledge, as Judge Becker had, that if an expert's conclusions are problematic enough, this alerts us to the possibility of some methodological defect.

This focus on "methodology" -- an accordion concept expanded and contracted as the

argument demands<sup>28</sup> -- obscured a much deeper epistemological question. Mr. Joiner's attorneys proffered a collage of bits of information, none sufficient by itself to warrant the conclusion that exposure to PCBs promoted Mr. Joiner's cancer, but which, they argued, *taken together* gave strong support to that conclusion; G.E.'s attorneys replied, in effect, that piling up weak evidence can't magically transform it into strong evidence. In response, Mr. Joiner's attorneys refer to the EPA guidelines for assessing the combined weight of epidemiological, toxicological, etc., evidence. But no-one ever addresses the key question: is there a difference between a congeries of evidence so interrelated that the whole really is greater than the sum of its parts, and a collection of unrelated and insignificant bits of information, between true consilience and the "faggot fallacy"<sup>29</sup> -- and if so, what is it?

There is a difference. Evidence of means, motive, and opportunity may interlock to support the claim that the defendant did it much more strongly than any of these pieces of evidence alone could do. Similarly, evidence of increased incidence of a disease among people exposed to a suspected substance may interlock with evidence that animals biologically similar to humans are harmed by exposure to that substance, and evidence indicating what chemical mechanism may be responsible, to support the claim that this substance causes, promotes, or contributes to the disease much more strongly than any of these pieces of evidence alone could do. However, the interlocking will be less robust if, e.g., the animals are unlike humans in some relevant way, or if the mechanism postulated to cause damage is also present in other chemicals not found to be associated with an increased risk of disease, or, etc. "Interlocking" is exactly the right word; for evidence is structured like a crossword puzzle, with each claim, anchored by experiential evidence (the analogue of the clues), enmeshed in reasons (the analogue of completed intersecting entries). How reasonable a crossword entry is depends on how well it is supported by the clue and completed intersecting entries, how reasonable those other entries are, independent of this one, and how much of the crossword has been completed; similarly, how warranted a claim is depends on how supportive the evidence is, how secure the reasons are, independent of this claim itself, and how much of the relevant evidence the evidence includes.<sup>30</sup> Because of the ramification of reasons, the desirable kind of interlocking of evidence gestured at in *Joiner* is subtle and complex, not easily captured by any mechanical weighting of epidemiological data relative to animal studies or toxicological evidence. Nor, moreover -- as Justice Rehnquist already pointed out in *Daubert* -- can its quality readily be judged by someone who lacks the necessary background knowledge. In *Kumho* the Supreme Court made a real epistemological step forward. In this products-liability case, focused on the proffered testimony of an expert on tire failure, the Court tried to sort out the problems with non-scientific experts which, as Justice Rehnquist had anticipated, soon arose in the wake of *Daubert*; and ruled that judges can't evade their gatekeeping duty on the grounds that proffered expert testimony is not science: the key word in FRE 702, after all, is "knowledge," not "scientific." No longer fussing over demarcation, recognizing the gap between "scientific" and "reliable," in *Kumho* the Supreme Court acknowledges that *what matters is whether proffered testimony is reliable, not whether it is scientific*. Quite so.

Far from backing away from federal courts' gatekeeping responsibilities, however, the *Joiner* Court had affirmed that a judge's decision to allow or exclude scientific testimony,

even though it may be outcome-determinative, is subject only to review for abuse of discretion, not to any more stringent standard; and the *Kumho* Court, pointing out that, depending on the nature of the expertise in question, the *Daubert* factors may or may not be appropriate, held that it is within judges' discretion to use any, all, or none of them. A year later, revised Federal Rules made explicit what according to *Daubert* had been implicit in Rule 702 all along: admissible expert testimony must be based on "sufficient" data, the product of "reliable" testimony "reliably" applied to the facts of the case. Federal judges now have large responsibilities and broad discretion in screening not only scientific testimony but expert testimony generally -- but very little guidance about how to perform this difficult task.

In short, since *Kumho*'s epistemological step forward, the other problem Justice Rehnquist worried about -- that judges generally lack the background knowledge which may be essential to a serious appraisal of the worth of scientific (or other technical) testimony -- is not merely unresolved, but more acute than ever. Bad epistemology can only get in the way; but better epistemology, unfortunately, can't by itself ensure smooth legal sailing.<sup>31</sup>

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<sup>1</sup> This article was originally published in *APA Newsletter on Philosophy and Law*, Fall 2003. It is abridged from *Trial and Error: The Supreme Court's Philosophy of Science*, Am. J. Pub. Health, 2003. Copyright 2003 Susan Haack.

<sup>2</sup> JACQUES BARZUN, *SCIENCE: THE GLORIOUS ENTERTAINMENT* (1964) at 14.

<sup>3</sup> *Frye v. United States*, 54 App.D.C. 46, 293 F. 1013 at 1014.

<sup>4</sup> *Barefoot v. Estelle*, 463 U.S. 880 at 898, 103 S.Ct. 3383 (1983) at 3397. Mr. Barefoot was executed in 1984.

<sup>5</sup> *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579, 113 S.Ct. 2786 (1993.)

<sup>6</sup> The *Daubert* Court did not itself scrutinize the disputed testimony; on remand, Judge Kozinski again excluded the plaintiffs' proffered experts, this time under *Daubert* rather than *Frye*. Because of litigation costs, Merrell Dow had already taken Bendectin off the market in 1984. In 2000 the FDA again declared the drug safe.

<sup>7</sup> KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* (1934), 37; *Preface, 1959* to the English edition of this book, 18.

<sup>8</sup> See Karl R. Popper, *Philosophy of Science: A Personal Report*, in *BRITISH PHILOSOPHY IN MID-CENTURY* (C.A. Mace. ed., 1957), reprinted in KARL R. POPPER, *CONJECTURES AND REFUTATIONS: THE GROWTH OF SCIENTIFIC KNOWLEDGE* (1962), 33, and in *SCIENTIFIC INQUIRY* (Robert Klee, ed., 1999), 65; and *The Problem of Demarcation* (1974; reprinted in *THE POCKET POPPER* (David Miller, ed., 1983), 118.

<sup>9</sup> KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* (*supra*, note 7), section 83.

<sup>10</sup> KARL R. POPPER, *OBJECTIVE KNOWLEDGE: AN EVOLUTIONARY APPROACH* (1972), 102.

<sup>11</sup> *Id.* at 22.

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<sup>12</sup> *Daubert*, 509 U.S. 579 at 600, 113 S.Ct. 2786 at 2800. Some federal judges evidently understand falsifiability better than others. In *U.S. v. Havvard*, 117 F.Supp. 2d 848, 854, admitting fingerprint identification testimony, Judge Hamilton observes that "the methods of latent print identification ... have been tested ... for roughly 100 years ... in adversarial proceedings." But in *Llera-Plaza I*, 2002 WL 27305 (E.D.Pa, Jan 2, 2002), 10, imposing restrictions on fingerprint identification testimony, Judge Pollak points out that "'adversarial' testing in court is not ... what the Supreme Court meant when it discussed testing as an admissibility factor."

<sup>13</sup> See K. R. Popper, *Natural Selection and Its Scientific Status* (excerpted from a lecture of 1977, in THE POCKET POPPER (*supra*, note 8)) at 298.

<sup>14</sup> *MacLean v. Arkansas Board of Education*, 529 F.Supp. 1255 (1982). Judge Overton's ruling, and Ruse's testimony, along with Larry Laudan's properly scathing critique, can be found in BUT IS IT SCIENCE? THE PHILOSOPHICAL QUESTION IN THE CREATION/EVOLUTION CONTROVERSY (Michael Ruse, ed. 1996).

<sup>15</sup> KARL R. POPPER, THE LOGIC OF SCIENTIFIC DISCOVERY (*supra*, note 7), 251-2, note \*1, added in the English edition. When Popper uses "confirm" for "corroborate" -- as he does in his 1957 *Philosophy of Science: A Personal Report* (*supra*, note 8) -- the effect is powerfully confusing.

<sup>16</sup> KARL R. POPPER, OBJECTIVE KNOWLEDGE (*supra*, note 10) 18, 22.

<sup>17</sup> *Id.* at 20; the reference to Hempel is in footnote 29.

<sup>18</sup> Carl G. Hempel, *Studies in the Logic of Confirmation*, 54 MIND 1-26 and 97-121 (1945), reprinted in CARL G. HEMPEL, ASPECTS OF SCIENTIFIC EXPLANATION AND OTHER ESSAYS IN THE PHILOSOPHY OF SCIENCE (1965), 43-4. See also his *Empiricist Criteria of Cognitive Significance: Problems and Changes* (adapted from two papers originally published in 1950 and 1951) and *Postscript (1964) on Cognitive Significance*, 99-122 ASPECTS OF SCIENTIFIC EXPLANATION.

<sup>19</sup> Carl G. Hempel, *Postscript (1964) on Confirmation*, 47 ASPECTS OF SCIENTIFIC EXPLANATION (*supra*, note 18), 51.

<sup>20</sup> Carl G. Hempel, *The Irrelevance of Truth for the Critical Appraisal of Scientific Theories* (1990: reprinted in SELECTED PHILOSOPHICAL ESSAYS (Richard Jeffrey, ed., 2000), 75).

<sup>21</sup> Albert Einstein, *Physics and Reality* (1936), in IDEAS AND OPINIONS OF ALBERT EINSTEIN (Sonja Bargmann, trans., 1954), 290.

<sup>22</sup> For a detailed development of the conception of scientific method on which I have relied here, see SUSAN HAACK, DEFENDING SCIENCE -- WITHIN REASON: BETWEEN SCIENTISM AND CYNICISM (2003), chapter 4.

<sup>23</sup> *Ramirez v. State*, 542 So. 2d 352 (Fla. 1989); *Ramirez v. State*, 651 So. 2d 1164 (Fla. 1995); *Ramirez v. State*, 8120 So. 2d 836 (Fla. 2001). Florida remains officially a *Frye* state, but seems to be rapidly evolving in the direction of (as Michael Saks puts it) *Fryebert*.

<sup>24</sup> CHARLES DARWIN, 45 AUTOBIOGRAPHY AND LETTERS (Francis Darwin, ed., 1893).

<sup>25</sup> See Evan Thomas, Mark Hosenball, and Michael Isikoff, *The JFK-Marilyn Hoax*, Newsweek, June 6, 1997, 36-7.

<sup>26</sup> *General Electric Co. v. Joiner*, 522 U.S. 136, 118 S.Ct. 512 (1997); *Kumho Tire Co. v. Carmichael*, 526 U.S. 137, 119 S.Ct. 1167 (1999).

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<sup>27</sup> In re. Paoli R.R. Yard PCB Litig., 35 F.3d. 717 (3d Cir. 1994).

<sup>28</sup> The term "accordion concept" was introduced in Wilfrid Sellars, *Scientific Realism or Irenic Instrumentalism?*, BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE, 2 (Robert Cohen and Marx Wartofsky, eds., 1965), 172.

<sup>29</sup> The word "consilience," meaning etymologically "jumping together," was coined by William Whewell, and recently made famous as the title of a best-selling book, E. O. WILSON, CONSILIENCE (1998), on the Unity of Science. The phrase "faggot fallacy" was introduced in PETR SKRABANEK AND J. MCCORMICK, FOLLIES AND FALLACIES IN MEDICINE (1997), and adopted by G.E.'s attorneys in *Joiner*.

<sup>30</sup> I first introduced the analogy in *Rebuilding the Ship While Sailing on the Water* (in Roger Gibson and Robert Barrett, eds, PERSPECTIVES ON QUINE, 1990, 111). It was articulated in more detail in SUSAN HAACK, EVIDENCE AND INQUIRY: TOWARDS RECONSTRUCTION IN EPISTEMOLOGY (1993), chapter 4, and is developed further in SUSAN HAACK, DEFENDING SCIENCE -- WITHIN REASON (*supra*, note 22), chapter 3.

<sup>31</sup> My thanks to Mark Migotti for very helpful comments on a draft.