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Of Truth, in Science and in Law

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When Questions of Science Come to the Courtroom, Truth Has Many Faces

—headline in the *New York Times*[1]

I. INTRODUCTION: FACTUAL TRUTH, SUBSTANTIAL JUSTICE, AND THE LAW OF EVIDENCE

In 1966, the United States Supreme Court averred that "[t]he basic purpose of a trial is the determination of truth."[2] In 1993, in the landmark ruling in *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, that set new standards for the admissibility of expert scientific testimony, Justice Blackmun was a bit more cautious, writing that "there are important differences between the quest for truth in the courtroom and the quest for truth in the laboratory."[3]

In my view, however, a trial—even a trial at which the main issue is a matter of fact, such as whether it was the defendant or someone else who pulled the trigger, or whether it was the drug in question or something else that caused the injury—*isn't* exactly a "search for truth." Rather, a trial is better described as a late stage of a whole process of determining a defendant's guilt or liability: the stage at which, under the legal guidance of the court, advocates for each side present evidence in the light most favorable to their case, and

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[1] Cornelia Dean, *When Questions of Science Come to a Courtroom, Truth Has Many Faces*, N.Y. TIMES, Dec. 5, 2006, at F3.

[2] Tehan v. United States, 382 U.S. 406, 416 (1966).

[3] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579, 597-97 (1993).

the finder of fact sifts through it and assesses whether it establishes guilt or liability to the required degree of proof.[4]

Qua litigators, the attorneys for parties at trial are primarily engaged, not in inquiry, but in advocacy. And the whole process is constrained by legal desiderata which are not directly, or even not at all, truth-related: that defendants' constitutional rights not be violated,[5] that certain policy considerations (such as not discouraging landlords from making repairs that might prevent accidents)[6] be served, and so on. Relevant evidence is thus sometimes excluded for reasons that have nothing to do with truth. Moreover, legal proceedings operate under a kind of time constraint alien to the search for truth in physics, history, and the rest. To quote Justice Blackmun again, the legal system seeks "quick, final and binding . . . judgments";[7] the desideratum of promptness imposes time constraints at one end of the process, the desideratum of finality-and-bindingness at the other.

Nevertheless, truth is surely *relevant* to legal proceedings, for we want, not simply resolutions, but *just* resolutions; and substantial justice requires factual truth.[8] In its efforts to arrive at factually correct verdicts, the legal system has come to rely a good deal on scientific experts, who by now testify on just about every scientific, and quasi-scientific, subject imaginable: experts on blood, bullets, bite-

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[4] Here and in the next paragraph I have drawn on an earlier paper of mine: Susan Haack, *Epistemology Legalized: Or, Truth, Justice, and the American Way*, 49 AM. J. JURIS. 43, 50 (2004).

[5] As the Supreme Court recognized in *Tehan*: "By contrast, the Fifth Amendment privilege against self-incrimination is not an adjunct to the ascertainment of truth. . . . [but] stands as a protection of quite different constitutional values." *Tehan*, 382 U.S. at 416.

[6] FED. R. EVID. 407 (providing that "evidence of the subsequent [remedial] measures" is (normally) inadmissible; e.g., that the landlord subsequently fixed the steps on which the plaintiff fell and broke her leg cannot be introduced in arguing that the landlord's negligence makes him liable for the injury).

[7] *Daubert*, 509 U.S. at 597.

[8] This principle has sometimes been deliberately compromised; the classic instance is *Summers v. Tice*, 199 P.2d 1, 3 (Cal. 1948) (holding that "where a group of persons are . . . engaged in the use of firearms, and two of them are negligent in firing in the direction of a third person who is injured thereby, both . . . are liable for the injury . . . although the negligence of only one of them could have caused [it]"). See also the DES (diethylstilbestrol) cases: *Sindell v. Abbott Laboratories*, 607 P.2d 924, 937 (Cal. 1980) (holding that "[e]ach defendant will be held liable for the proportion of the judgment represented by its share of that market unless it demonstrates that it could not have made the product which caused plaintiff's injuries"), and, most striking of all, *Hymowitz v. Eli Lilly & Co.*, 539 N.E.2d 1069, 1078 (1989) (holding that "there should be no exculpation of a defendant who, although a member of the market producing DES for pregnancy use, appears not to have caused a particular plaintiff's injury").

marks, battered wives; experts on PCBs,[9] paternity, poisons, post-traumatic stress; experts on radon, recovered memories, rape trauma syndrome, random-match probabilities; experts on psychosis, asbestosis, silicosis (and for all I know, on psittacosis!).[10] There's no question that the testimony of scientific experts is often essential, or that it can be enormously useful; but there's also little question that these interactions between science and the legal system can be quite problematic.

The announcement of this conference invites participants to consider "if and how [the inquiry into the reliability of proffered scientific testimony mandated by *Daubert*] relates to 'truth' and whose view of the truth should prevail." I'm not entirely sure what to make of this, especially of those scare quotes around "truth." Could they be an expression of skepticism, in the fashion of the post-modernists, about the legitimacy of the very concept of truth?[11] Or are they perhaps an implicit endorsement of the strong forms of social constructivism fashionable among some practitioners of Sociology of Scientific Knowledge ("SSK"), Science and Technology Studies ("STS"), "ethnomethodology of science," and other related fields?[12] Or might they be just a way of conveying the modest-enough idea that certain scientific claims come to be thought of as true, not as a result of scientific investigation, but by becoming entrenched in legal decisions?

In any case, I hope the account of truth in the sciences offered here will be on point. It will combine a full acknowledgment of the fallibility and incompleteness of the scientific enterprise with a robustly objective conception of

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[9] Polychlorinated biphenals, the highly toxic substance at issue in *General Electric Co. v. Joiner*, 522 U.S. 136, 139 (1997).

[10] The list is taken from another earlier paper of mine: Susan Haack, *What's Wrong with Litigation-Driven Science? An Essay in Legal Epistemology*, presented at the Coronado III Conference organized by the Project on Scientific Knowledge and Public Policy (SKAPP) in 2006 (forthcoming in Seton Hall Law Review), *available at* http://www.defendingscience.org/coronado_conference_papers/upload/Haack_Litigation.

[11] I don't believe such skepticism is warranted. *See* Susan Haack, *Staying for an Answer: The Untidy Process of Groping for Truth*, TIMES LITERARY SUPPLEMENT, July 9, 1999, at 12, *reprinted in* SUSAN HAACK, PUTTING PHILOSOPHY TO WORK: INQUIRY AND ITS PLACE IN CULTURE: ESSAYS ON SCIENCE, RELIGION, LAW, LITERATURE, AND LIFE (forthcoming) (Prometheus Books 2008); Susan Haack, *The Unity of Truth and the Plurality of Truths*, 9 PRINCIPIA 87 (2005), *reprinted in* HAACK, PUTTING PHILOSOPHY TO WORK, *supra*.

[12] As this suggests, there are stronger and weaker forms of social constructivism, some (in my opinion) true but not very exciting, others exciting but false. They are distinguished and disentangled in SUSAN HAACK, DEFENDING SCIENCE—WITHIN REASON: BETWEEN SCIENTISM AND CYNICISM 190-91 (2003).

truth. It will, in consequence, be distinctly inhospitable both to fashionable forms of skepticism about the very idea of truth, and to strong forms of social constructivism about the sciences. But its combination of fallibilism and objectivity will help us understand why the legal system often gets less than the best out of science, shed some light on the vagaries of the legal use of the word "reliable," and clarify the process by which scientific claims, true *or* false, can become legally entrenched as (supposedly) reliable science.

The first step (Section II) is to sketch the legal history leading up to *Daubert*, and to explore some of the difficulties *Daubert* brought in its wake; the next (Section III) to develop an account of truth in the sciences; then (Section IV) to articulate why the legal system so often fails to get the best scientific information available; and finally (Section V) to show how false scientific claims sometimes get legally entrenched as reliable science.

II. A LEGAL TANGLE: FROM *FRYE* TO *DAUBERT* TO *KUMHO TIRE* TO REVISED FEDERAL RULE OF EVIDENCE 702

Ever since the legal system began to call on scientific witnesses, there have been complaints both from legal commentators and from scientists themselves about the defects and drawbacks of the process. Judges, attorneys, and legal scholars complain about the venality of scientists willing to testify to just about anything for money, about the naiveté of scientists who apparently don't think concerns about conflict of interest apply to them,[13] and so forth; scientists complain about judges', jurors', and attorneys' ignorance and credulity on scientific matters, about the professional insult of being "Dauberted" or "dauberted out,"[14] and so on. And while for

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[13] *See, e.g.*, Winans v. N.Y. & Erie R.R. Co., 62 U.S. 88, 101 (1858) ("Experience has shown that opposite opinions of persons professing to be experts may be obtained to any amount."); Lee M. Friedman, *Expert Testimony, Its Abuse and Reformation*, 19 YALE L.J. 247, 247 (1910) ("[T]here is a constant complaining and mistrust on the part of judges, juries and lawyers of the expert witness.").

[14] The phrase, once heard mainly in conversation, has entered the literature: "A 'dauberted out' economic expert in injury and wrongful death cases can be particularly disastrous." LostCompensation.com, De-Daubertizing Economic Damages Evidence, http://www.lostcompensation.com/newsletters/v3_i1_2006.html (last visited Oct. 2, 2007). "A witness is said to be *'Dauberted'* if excluded from testifying. Being 'dauberted out' may make scientists even less willing to testify." Shirley S. Abrahamson, *Legal Standards for Expert Testimony*, CIVIL ACTION, Spring 2006, at 5, http://www.vis-res.com/pdf/Civil-Action-Spring2006.pdf (excerpted from a speech

many decades now, from *Frye* (1923)[15] to the Federal Rules of Evidence ("FRE") (1975) to *Daubert* (1993)[16] and beyond, there have been legal efforts to domesticate scientific testimony by means of rules of evidence, these efforts have hardly been an unqualified success.

The Supreme Court's ruling in *Daubert*, with its many ambiguities and confusions, is emblematic of the difficulties. The core of the *Daubert* ruling is that FRE 702, according to which the testimony of a qualified expert is admissible provided it is relevant and not otherwise legally excluded, supersedes the old *Frye* Rule,[17] according to which novel scientific testimony is admissible only if it is "sufficiently established to be generally accepted in the field to which it belongs."[18] But the *Daubert* ruling goes on to explain that courts should screen proffered expert testimony for reliability as well as relevance.[19] Moreover, the intended meaning of "reliable," in the specialized, "evidentiary" sense Justice Blackmun gives it, and its relation to our ordinary notions of reliability, remains far from transparent.

Courts are to assess reliability, Justice Blackmun continues, by determining whether proffered expert evidence is genuinely "scientific . . . knowledge,"[20] and this determination is to be focused on experts' methodology, not their conclusions. He seems to run "reliable" together with "scientific," and "genuinely scientific" with "conducted in accordance with the scientific method" (elisions perhaps aided and abetted by his conflation of Karl Popper's and Carl Hempel's incompatible philosophies of science).[21] The reference to "peer review and publication" in his list of indicia of reliability veers unsteadily between suggesting that courts determine whether the science

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presented at the Arthur M. Sackler Colloquia of the National Academy of Sciences (Nov. 17, 2005)).

[15] Frye v. United States, 293 F. 1013 (D.C. Cir. 1923).

[16] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579 (1993).

[17] *Id.* at 589.

[18] *Frye*, 293 F. at 1014.

[19] *Daubert*, 509 U.S. at 589.

[20] *Daubert*, 509 U.S. at 590, 590 n.8. This is the Court's interpretation of the 1975 Federal Rule of Evidence 702; but as the ellipses indicate, Justice Blackmun omits several words from the text of the Rule, which spoke of "scientific, technical, or other specialized knowledge." FED. R. EVID. 702 (1975).

[21] For more detailed discussion, see Susan Haack, *Trial and Error: The Supreme Court's Philosophy of Science*, 95 AM. J. PUB. HEALTH S66 (2005), *reprinted in* 41 INT'L SOC'Y BARRISTERS Q. 376 (2006), and HAACK, PUTTING PHILOSOPHY TO WORK, *supra* note 11.

on which proffered testimony is based has survived the pre-publication peer-review process of the scientific journals, and suggesting that courts determine whether it has survived (or will survive?) the long-run scrutiny of the relevant scientific community.[22] And so on.

Moreover, while the ostensible intent of the *Daubert* ruling was to relax the "austere standard" of the older *Frye* rule in accordance with FRE 702—a point that states that decided to stick with *Frye* sometimes emphasize[23]—it is far from clear that this has really been its effect. Though a 2001 study from the RAND Institute for Civil Justice indicated that the full story may be more complicated than this,[24] commentators often suggest that on the whole, at least in civil cases, *Daubert* has made it harder, not easier, to get scientific testimony admitted.[25]

Since *Daubert*, the Supreme Court has twice returned to the question of scientific testimony, first in *Joiner* (1997)[26] and

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[22] For more detailed discussion, see Susan Haack, *Peer Review and Publication: Lessons for Lawyers*, 36 STETSON L. REV. 789 (2007).

[23] *E.g.*, People v. Leahy, 882 P.2d 321, 331 (Cal. 1994) (declining to adopt *Daubert* on the grounds that "*Kelly* sets forth the various reasons why the more 'conservative' *Frye* approach . . . [is] an appropriate one") (referring to People v. Kelly, 549 P.2d 1240 (Cal. 1976)); Brim v. State, 695 So. 2d 268, 271-72 (Fla. 1997) ("Despite the federal adoption of a more lenient standard in *Daubert v. Merrell Dow Pharmaceuticals, Inc.* we have maintained the higher standard of reliability as dictated by *Frye*." (citation and footnote omitted)); Blum v. Merrell Dow Pharm., Inc., 764 A.2d 1, 3 (Pa. 2000) (while the Blums' expert testimony was arguably admissible under "the somewhat less exacting standard of *Daubert*," it was inadmissible under the *Frye* Rule); Olivier A. v. Christina A., No. 30779/2002, 2005 WL 2171176, at *24 (N.Y. Sup. Ct. Aug. 25, 2005) ("The current controversy seeks to use *Daubert* to restrict and invalidate prior types of admissible evidence rather than expand what is admissible. This is inapposite . . . .").

[24] The study, based on data from 399 federal district court opinions in civil cases between 1980 and 1999, found that in the years immediately following *Daubert* judges scrutinized reliability more carefully, applied stricter standards, excluded more evidence, and issued more summary judgments; but that after about 1996 the rate of successful challenges began to fall, in part, they conjecture, because the parties responded to the changes in standards. LLOYD DIXON & BRIAN GILL, CHANGES IN THE STANDARDS FOR ADMITTING EXPERT EVIDENCE IN FEDERAL CIVIL CASES SINCE THE *DAUBERT* DECISION 7-8, 22-27 (RAND Inst. for Civil Justice, 2001).

[25] *See, e.g.*, Lisa Heinzerling, *Doubting* Daubert, 14 J.L. & POL'Y 65, 68 (2006) (arguing that "[t]he *[Daubert]* Court's casually offered guidelines on admitting expert scientific evidence have served as the vehicle for transforming *Daubert* from an evidence-liberalizing decision into an evidence-narrowing one"); *The Changing Role of Judges in the Admissibility of Expert Evidence*, CIVIL ACTION, Spring 2006, at 1, 3, http://www.vis-res.com/pdf/Civil-Action-Spring2006.pdf (reporting that "[t]he study [of product liability filings in the Delaware Superior Court] shows that the failure to 'survive' the *Daubert* challenge is contributing to the Vanishing Trial").

[26] General Electric Co. v. Joiner, 522 U.S. 136 (1997).

then in *Kumho Tire* (1999).[27] In these later rulings, the Court seems to have backed quietly away from some of the *Daubert* Court's more ambitious philosophical observations: Justice Rehnquist's ruling in *Joiner* suggests that the distinction between methodology and conclusions stressed in *Daubert* may be less than robust;[28] Justice Breyer's ruling in *Kumho Tire* insists that when FRE 702 refers to "scientific or other technical knowledge" the key word is "knowledge," not "scientific."[29] But these later rulings hardly solved all the old problems; and they introduced new problems of their own. *Joiner*, confirming that evidentiary rulings under *Daubert* are reviewable only for abuse of discretion,[30] brings the possibility to the fore that different courts in the same jurisdiction may rule inconsistently on the admissibility of the very same evidence. *Kumho Tire*, confirming that *Daubert* applies to non-scientific as well as to scientific expert testimony, and that courts may use any, all, or none of the *Daubert* factors, or such other factors of their own devising as they deem appropriate,[31] leaves courts with startlingly little substantive guidance.

In December 2000, FRE 702 was modified to make courts' obligation to screen proffered expert testimony for reliability explicit: to be admissible, expert testimony must be based on "sufficient" data, the result of "reliable" methods, "reliably [applied] to the facts of the case."[32] With its stress on the need for courts to determine the reliability both of the underlying data and of its application to the case at issue, this revision went somewhat beyond simply articulating what, according to the *Daubert* Court, was already implicit in the original Rule 702; and some courts have understood the revised Rule as having tightened the *Daubert* standard.[33] Still, however often the word "reliable" and its cognates are repeated, it is

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[27] Kumho Tire Co. v. Carmichael, 526 U.S. 137 (1999).

[28] *Joiner*, 522 U.S. at 146.

[29] *Kumho*, 526 U.S. at 147. Surprisingly, Justice Breyer suggests that this was already clear in *Daubert*. *Id.* at 147-48.

[30] *Joiner*, 522 U.S. at 143 ("[T]he question of admissibility of expert testimony . . . is reviewable under the abuse of discretion standard").

[31] *Kumho*, 526 U.S. at 141 ("*Daubert*'s general holding . . . applies not only to testimony based on 'scientific' knowledge, but also to testimony based on 'technical' and 'other specialized' knowledge. . . . But . . . *Daubert*'s list of specific factors neither necessarily nor exclusively applies to all experts or in every case.").

[32] FED. R. EVID. 702 (2000) (*and* FED. R. EVID. 702 (2006)).

[33] *See, e.g.*, Rudd v. General Motors Corp., 127 F. Supp. 2d 1330, 1336 (M.D. Ala. 2001) ("[T]he new Rule 702 appears to require a trial judge to make an evaluation that delves more into the facts than was recommended in *Daubert* . . . .").

hard to see how any such verbal formula could, by itself, enable courts to discriminate genuinely reliable testimony from the unreliable stuff.

Even after all these legal efforts, I suspect, no one believes that the legal system always gets the most or the best scientific information that, ideally, it might. This, however, presupposes that there are more and less reliable scientific claims, and that scientific claims are (normally) either objectively true or else objectively false. But this presupposition stands in need of more detailed articulation and more careful argument.

III. TRUTH IN SCIENCE: THE CRITICAL COMMON-SENSIST PERSPECTIVE[34]

The word "truth" is sometimes used as an abstract noun, referring to the concept of truth (or as some would prefer to say, to the property of being true, or to the meaning of the word "true" and its synonyms in other languages).[35] The word is often also used, however, to refer to the particular propositions, etc., which *are* true; in this use it takes the plural form, as in: "we hold these truths to be self-evident . . . ."[36]

People also write (or, "drawing" the quotation marks in the air with their fingers, speak) of "'truth,'" and "'truths.'" In this use, quotation marks signal doubts, reservations, or outright skepticism about the propriety of the word or phrase they enclose, and are known in the jargon of philosophers as scare quotes. Their effect is, roughly, to turn a term meaning "X" into a term meaning "so-called 'X'." So "truths" means "so-called 'truths'" or "purported truths," that is, propositions

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[34] "Critical common-sensism" (a phrase borrowed from C.S. Peirce) is my name for the philosophy of science offered in HAACK, DEFENDING SCIENCE—WITHIN REASON, *supra* note 12, on which I shall draw throughout this section.

[35] Though I write here of "the property of being true," I do not mean to foreclose questions in the philosophy of logic about whether "true" really is a predicate, really does represent a property, or whether it is, as Frank Ramsey suggested, something more like a sentential quantifier. *See generally* FRANK PLUMPTON RAMSEY, ON TRUTH: ORIGINAL MANUSCRIPT MATERIALS (1927-1929) FROM THE RAMSEY COLLECTION AT THE UNIVERSITY OF PITTSBURGH (Nicholas Rescher & Ulrich Majer eds., 1991); María-José Frápolli, *The Logical Enquiry into Truth*, 17 HIST. & PHIL. LOGIC 179 (1996); Susan Haack, *The Unity of Truth and the Plurality of Truths*, *supra* note 11, at 90-91.

[36] Though I write here of the truth (and falsity) of propositions (and later of the truth (and falsity) of claims, theories, etc.), I do not mean to foreclose issues about the precise nature of the truth-bearers, a topic long debated in philosophy of logic. *See* SUSAN HAACK, PHILOSOPHY OF LOGICS 74-85 (1978).

which are *taken to be* or *presented as* truths; but conveys the suggestion that they may not really be true. And "truth" means "so-called 'truth,'" that is, the *supposed* concept of truth or property of being true; but conveys the suggestion that the concept may not really be a legitimate one.

This much, though seldom spelled out explicitly,[37] is not very controversial; now we get to the controversial part, for as the philosopher Nicholas Rescher once observed, "If two people agree, one of them isn't a philosopher."[38] What I offer here is not "*the* philosophical perspective" on truth in the sciences; it is *my* philosophical perspective on truth in the sciences. But it is not (as I see it!) just one perspective on an equal footing with any other; for that it is my perspective means precisely that this is the conception I believe to be correct. I have no reservations about the legitimacy of the concept of truth, and so, where appropriate, will speak without apology of truth, not "truth." However, I also have no doubt that propositions are often taken for true which are in fact false, that is, are only "truths," not truths.

I should add that what I offer here is not an account of *scientific* truth, if that is taken to imply that "true" has a special, distinctive meaning when applied to scientific propositions. My observations about the concept of truth are intended to apply to the truth not only of claims and theories in the sciences, but also to the truth of propositions of other kinds.

As I see it, the terms "science" and "the sciences" are best construed as referring to a loose federation of kinds of inquiry, roughly characterizable by the questions within their scope. A large range of factual questions, including questions about the consequences of putting this or that policy into effect or about the risks and benefits of a drug, fall within the scope of one (or sometimes more than one) of the various, ramifying branches of science; but questions about whether this or that policy is preferable, or whether the risks outweigh the benefits, are not themselves scientific questions. This is not to suggest that the distinction is always perfectly clear; nor, more generally, that there is a clean, sharp line dividing scientific

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[37] The honorable exception is the now-classic critique of the deceptive power of scare quotes in D.C. STOVE, POPPER AND AFTER: FOUR MODERN IRRATIONALISTS (1982); *reprinted under the title* ANYTHING GOES: ORIGINS OF THE CULT OF SCIENTIFIC IRRATIONALISM (1998).

[38] NICHOLAS RESCHER, THE STRIFE OF SYSTEMS: AN ESSAY ON THE GROUNDS AND IMPLICATIONS OF PHILOSOPHICAL DIVERSITY 3 (1985).

questions from others—cosmological from metaphysical questions, for example, or questions in theoretical psychology from questions in philosophy of mind.[39] Nor is it to deny that, when scientific inquiry tackles old questions, it almost always raises new ones, and sometimes leads to the conclusion that an older question has no true or false answer, but is flawed by false presuppositions.

The goal of scientific inquiry, as of any kind of inquiry, is to discover the answer to some question or questions; the true answer, that is.[40] This is not to suggest that scientists seek THE TRUTH, in some quasi-religious sense (the sense, whatever it is, of Jesus' claim that he is "the Way, the Truth, and the Light"); it is not to suggest that scientists collect true propositions, as some people collect rare stamps or antique furniture; and it is not to intimate that scientific truths are the only truths there are. But it is to say that the goal of an investigation into the structure of DNA is to reach the answer that DNA is a double-helical, backbone-out macromolecule with like-with-unlike base pairs if DNA *is* a double-helical, backbone-out macromolecule with like-with-unlike base pairs, or the answer that DNA is a triple-helical, backbone-in macromolecule with like-with-like base pairs if DNA *is* a triple-helical, backbone-in macromolecule with like-with-like base pairs, . . ., or that it's more complicated than that if it *is* more complicated than that; . . . and so on, *mutatis mutandis*, for other questions.

This is fully in accordance with Aristotle's dictum that "[t]o say of what is that it is not, or of what is not that it is, is false, while to say of what is that it is, and of what is not that it is not, is true";[41] and with Frank Ramsey's laconic observation that "[a] belief that p . . . is true if and only if p; for instance, a

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[39] *See also* Susan Haack, *Not Cynicism, But Synechism: Lessons from Classical Pragmatism*, 41 TRANSACTIONS CHARLES S. PEIRCE SOC'Y 239 (2005), *reprinted in* COMPANION TO PRAGMATISM 141 (John Shook & Joseph Margolis eds., 2006) *and in* HAACK, PUTTING PHILOSOPHY TO WORK, *supra* note 11.

[40] As this suggests, I accept neither an Instrumentalist view (according to which theoretical "statements" in science are not really genuine statements, and hence are neither true nor false), nor a Constructive Empiricist view (according to which, though theoretical statements are statements, and do have truth-values, the goal of science is empirical (observational) adequacy, not truth). I cannot argue either point here, but they are discussed in HAACK, DEFENDING SCIENCE—WITHIN REASON, *supra* note 12, at 137-41.

[41] ARISTOTLE, METAPHYSICS Book IV, 7, 1011b25 (W.D. Ross trans., Clarendon Press 1924), *reprinted in* THE BASIC WORKS OF ARISTOTLE 749 (Richard McKeon ed., 1941).

belief that Smith is either a liar or a fool is true if Smith is either a liar or a fool and not otherwise."[42] (This is "merely a truism," Ramsey continues, needing to be stressed only because "there is no platitude so obvious that eminent philosophers have not denied it."[43]) It is also enough to tell us that whether a scientific claim or theory is true or is false is normally an objective matter; that is, it is neither necessary nor sufficient for a proposition's being true that you, or I, or anyone believe it.[44]

To be sure, scientific claims and theories come into being as the result of scientists' intellectual work; so in one sense scientists might be said to *make* scientific truths. But it is not scientists' intellectual work, but the nature of the phenomena and events in the world that those claims and theories describe, that makes those scientific truths *true*. Natural-scientific claims and theories are about natural phenomena and events, and so (except in special cases involving phenomena and events that occur only in the laboratory) are about things not of our making; and while social-scientific claims and theories are about social phenomena, institutions, roles, rules, etc., which *are* of our making, whether those claims and theories are true or are false is still independent of whether you, or I, or anyone, *thinks* they are.

So scientific claims and theories are (normally) either true or else false, and their truth or falsity is (normally) an objective matter. The objectivity of *scientists*, however, is another question, for the meaning of "objective" shifts somewhat when it is applied to persons. To say that not all scientists are objective means, first, that not all scientists are unemotional, stolid types; some are deeply engaged with and excited by their work. And second, most to the present purpose, it means that not all scientists are unbiased. A few scientists have been outright dishonest or fraudulent; many more, probably, are self-deceived on some matters and in some degree; most, almost certainly, have some preconceptions on

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[42] RAMSEY, *supra* note 35, at 12.

[43] *Id.*

[44] Of course, this falls well short of a complete theory of truth. I do not claim to have such a theory, but I have nibbled away at the problem in HAACK, PHILOSOPHY OF LOGICS, *supra* note 36, at 86-134; SUSAN HAACK, *Confessions of an Old-Fashioned Prig*, *in* MANIFESTO OF A PASSIONATE MODERATE: UNFASHIONABLE ESSAYS 7, 7-30 (1998); Susan Haack, *The Unity of Truth and the Plurality of Truths*, *supra* note 11.

some questions that make them less responsive to certain evidence than they should ideally be.

The evidence with respect to scientific claims and theories is usually very complex, ramifying in every direction. It often depends on the reliable working of instruments of various kinds, or on the soundness of elaborate statistical techniques or computer programs; and it is almost always the work of many people—whether collaborators or rivals, and whether working together or many miles or decades apart—who rely, explicitly or implicitly, on the competence and honesty of others involved. Evidence can be misleading, ambiguous, hard to interpret—and it is virtually always incomplete. Getting more evidence may be difficult, prohibitively expensive, or outright impossible in the current state of knowledge or technology; worse, it isn't always clear even what additional evidence is needed (for whether this evidence is relevant to that claim depends on facts about the world, facts about which we may be mistaken).[45] Whether or not they articulate it explicitly, most serious scientists have a firm-enough grasp of the complexities of evidence; this is why, wary of claiming to have found the truth, they prefer to say, "this seems like a promising idea," "this model seems to fit what we know so far," "probably the value of c is approximately n," "perhaps the explanation might be this," "possibly, it's this way," and such.

At any time there is a whole continuum of scientific ideas, claims, and theories: some so well-warranted by such strong evidence that it is most unlikely they will have to be revised; some not quite so well-warranted but still pretty solidly established; some promising but as yet far from certain; some new and exciting but highly speculative and as yet untested; and some so wild that few mainstream scientists are willing even to listen to them. (The proportion of the well-warranted to the highly-speculative varies, obviously, across fields and sub-fields.) A few of the exciting but as yet untested ideas, and a very, very few of the wildest ideas, will eventually turn out to be warrantable, but most will not. A few of the now pretty-well-warranted ideas, and perhaps even a very, very few

[45] Was I the only person in the country who didn't laugh derisively at Donald Rumsfeld's observations about those "unknown unknowns" in military intelligence? From a strictly epistemological point of view, this was an unusually sophisticated remark.

of the best-warranted ideas, will eventually be overturned by overwhelming contrary evidence, but most will not.

There is no algorithmic scientific method guaranteeing success, or even progress. Rather, the many and various techniques and technologies the sciences have gradually devised—the instruments of observation, the cunningly devised experiments, the advanced mathematical and statistical techniques, the ever-fancier computer programs, etc.—have gradually made it possible to get more evidence, more exact evidence, and more focused evidence, and to work out the consequences of evidence, and assess its worth, more accurately. Nor are there "rules of acceptance and rejection" governing when a claim is well-enough warranted to be accepted into the corpus of scientific knowledge, and when badly-enough undermined to be rejected. Rather, virtually any scientific community will include some who are temperamentally inclined to work patiently to modify and adapt an old idea in the light of awkward new evidence, and some who are temperamentally more disposed to move on, to start looking for a fresh approach. And as new evidence comes in, there will be ongoing shifts of opinion out of which—though it may take many years or even decades—consensus will eventually emerge.

"Warrant" is an epistemological term of art:[46] how warranted a claim is depends on how good the evidence with respect to that claim is. "Acceptance" and "consensus," by contrast, are psycho-sociological terms, referring to scientists' agreement on the correctness of this or that idea. Though doubtless individual scientists often hope to persuade others that their approach or their conjecture is the right one, scientific investigation is not an effort to reach consensus; it is, rather, an effort to answer the questions at issue. Consensus is ordinarily a byproduct that arises when enough members of the relevant scientific community come to see the evidence as warranting a claim or a theory.

As the previous paragraph suggests, warrant and consensus would, ideally, be tightly correlated: as the evidence favorable to a claim gets stronger, more scientists in the field will be inclined to accept it, or will be inclined to accept it more firmly; and as the evidence against a claim gets stronger,

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[46] "Epistemology" (from the Greek, *episteme*, "knowledge") refers to the field of philosophical investigation focused on such concepts as knowledge, evidence, warrant, justification, inquiry, method, and intellectual character.

more scientists in the field will be inclined to reject it, or to reject it more confidently. At any time, there will be many scientific questions with respect to which the available evidence is still incomplete or ambiguous enough to leave room for legitimate disagreement among competent specialists about whether this account or that is more likely right, or even about whether any conclusion at all can be drawn on the basis of the evidence thus far. As more and stronger evidence comes in, however, more will be persuaded, until the former skeptics are convinced (or perhaps marginalized, or retired), and consensus solidifies.

This seems to have been pretty much what happened, for example, with James Watson and Francis Crick's model for the structure of DNA—where, by Crick's reckoning, it took about 30 years, and a lot more evidence than they had in 1953, before theirs became the standard, well-accepted view.[47] But there is no guarantee that warrant and acceptance will always run in parallel; for the process by which bad ideas and false theories get filtered out and rejected, and good ideas get established and accepted, is fallible, untidy, and ragged. Nor, of course, is there any guarantee that all the scientific claims accepted as true at any time *are* true; almost certainly, some will eventually turn out to have been, not truths, but only "truths."

Given the investigative character of the scientific enterprise and the pervasive reliance of individual scientists on evidence discovered by others, the core values are honesty (both with yourself and with other people) about what the evidence is and where it leads, and willingness to share evidence unreservedly with other investigators.[48] Along with its constantly evolving ways of acquiring and appraising evidence, the scientific community has found ways to sustain commitment to these core epistemological values. This has been a matter less of regulation than of ethos: ideally, the values of honesty and evidence-sharing will be instilled in young

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[47] FRANCIS CRICK, WHAT MAD PURSUIT? A PERSONAL VIEW OF SCIENTIFIC DISCOVERY 71 (1988).

[48] My terminology is new, and I hope less potentially confusing; but these terms correspond approximately to what Robert Merton called the norms of "disinterestedness" and "communism." ROBERT MERTON, SOCIAL THEORY AND SOCIAL STRUCTURE 307-16 (1949); *see also* Susan Haack, *The Integrity of Science: What It Means, Why It Matters*, Ética e Investigacão nas Ciências da Vida, Actas do 10º Seminário de Conselho Nacional de Ética para as Ciências da Vida, 9 (2007), *reprinted in* PUTTING PHILOSOPHY TO WORK, *supra* note 11.

scientists in the course of their long apprenticeship, and will be sustained by the incentive of renown for successful work and the disincentive of loss of reputation for cheating, by conscientious peer review at journals and grant-giving bodies, and by the commitment of the universities to a culture of investigation. So long as these social mechanisms work tolerably well, dishonest or sloppy work will be discouraged or, at worst, will be detected and discarded—not always, but at least when it is significant enough scientifically that, sooner or later, someone thinks it worth his while to try to build on it.

But the social mechanisms that sustain commitment to the core epistemological values of the scientific enterprise are far more fallible and fragile than the technical helps to scientific inquiry—that is, the instruments, computer programs, and such. These social mechanisms are especially susceptible to pressures from the competing values of the society in which scientific work is undertaken: the commercial values of industrial sponsors of science, for example, or the ideological or political values of government sponsors. Presently, moreover, they are under such strain from changes in science funding, changes in the universities, changes at the scientific and medical journals, etc., that the integrity of science is in some danger of erosion.[49]

The pressure of competing values is not felt evenly, but is more severe in some areas of science than others; and it is most severe, probably, in certain areas of the sciences, the biomedical sciences in particular, on which the legal system very often finds itself calling. This, as we shall see, is one of many reasons why the science that enters the legal system so often disappoints us.

IV. SCIENCE IN THE BRAMBLE PATCH

"One of many reasons"—almost too many, and too untidily intermingled, to list; but I will do my best.

The first thing to stress, though, is that the enterprise to which we refer by the commodious word "science" is enormously complex, and uneven both in character and in quality. "Science" encompasses some of the most remarkable achievements of the human mind, some near-miracles of

[49] *See* HAACK, *The Integrity of Science*, *supra* note 48.

"patience and postponement, [of] choking down of preference"[50] as those heroes of the history of science have figured out this or that aspect of the world, and a good deal more routine but solid and significant investigation. But it also includes plenty of (sometimes far-fetched) speculation; and, as with everything human, much that is lazy, sloppy, corner-cutting, self-promoting, or self-deceptive, and some that is flaky or even fraudulent. Moreover, the scientific enterprise—complex, ragged, fallible, fumbling, but thus far remarkably successful, as human enterprises go—operates within the larger society on which it relies for resources, and is vulnerable to pressures to conform to commercial, ideological, etc., values at odds with its own core epistemological norms.

Against this background, it isn't hard to see why the legal system has had difficulties in handling scientific testimony. It often calls on the weaker areas of science and/or on weak or marginal scientists in an area; moreover, its adversarial character may mean that even solid scientific information gets distorted; it may suppress or sequester relevant data; it may demand scientific answers when none are yet well-warranted; it may fumble in applying general scientific findings to specific cases; and it may fail to adapt appropriately as a relevant scientific field progresses. Let me take these points in turn.

Much of the scientific work on which the law calls comes from weaker or less mature sciences or fields of science.[51] Some comes from fields the market for which is largely, if not exclusively, created by the legal system itself: for example, psychiatric theorizing about (purportedly) recovered memories, surely at the weaker end of the social, or as we sometimes say, the "soft" sciences; and forms of forensic identification, such as latent fingerprint identification, about the reliability of which much is claimed, but remarkably little seems to be known.[52]

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[50] WILLIAM JAMES, *The Will to Believe*, *in* THE WILL TO BELIEVE AND OTHER ESSAYS IN POPULAR PHILOSOPHY 1, 7 (Dover 1956) (1897).

[51] And, as Justice Breyer observes, general acceptance in a discipline is little assurance of reliability if the discipline in question is weak or dubious. *Kumho*, 526 U.S. at 151.

[52] *See, e.g.*, Jennifer Mnookin, *Fingerprints: Not a Gold Standard*, ISSUES IN SCI. & TECH., Fall 2003, at 47; Sharon Begley, *Despite Its Reputation, Fingerprint Evidence Isn't Really Infallible*, WALL ST. J., June 4, 2004, at B1; Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Rulings from* Jennings *to* Llera Plaza *and Back Again*, 41 AM. CRIM. L. REV. 1189, 1191-1208 (2004); Simon A. Cole, *Does "Yes" Really Mean Yes? The Attempt to Close Debate on the Admissibility of Fingerprint Evidence*, 45 JURIMETRICS J. 449, 453-58 (2005); Simon A. Cole, *Is*

Even when the science crucial to litigation comes from relatively stronger scientific fields where there is a substantial body of well-established knowledge, these are quite often the very areas of science where scientists' commitment to the norms of honesty and evidence-sharing is most sorely tried—making it more likely that awkward evidence will be withheld, or "spun" to reach a desired conclusion. The scientific evidence in toxic tort litigation, for example, often hinges on epidemiological studies undertaken by defendant manufacturers, whether for the purpose of obtaining Food and Drug Administration ("FDA") approval or in anticipation of the possibility of such litigation. Sometimes such studies are designed to make it more likely that they will detect favorable data than that they will detect unwelcome trends.[53]

Moreover, even when the relevant areas of science are relatively solid and respectable, the scientific issues in litigation tend to turn not on firmly-established, well-warranted core scientific principles, but on still-controversial scientific issues where there remains room for reasonable disagreement even among competent, honest scientists in the field. The better established it is that this substance is harmless and inert in the human body, for example, the less likely it is that it will be the subject of toxic tort litigation, and the better established it is that it is dangerous to humans, the likelier that cases will be settled; the better established it is that this technique of forensic identification is bullet-proof, the likelier that cases will be pled out.

And because of its adversarial character, the legal system tends to pull in scientists from the farther ends of the spectrum of scientific opinion: those ready to give a confident answer before others think any answer is warranted, those more scientifically radical, or more scientifically conservative, than most of their colleagues, those whose views have become

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Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents' Discourse, 28 LAW & POL'Y 109, 109-10 (2006). Fingerprints first acquired their supposedly authoritative status, apparently, when English police found they could elicit confessions by confronting suspects with this damning evidence against them. Simon Cole, *What Counts for Identity? The Historical Origins of the Methodology of Latent Fingerprint Identification*, SCI. IN CONTEXT, Spring 1999, at 139, 151.

[53] For example, the first large clinical trial of Vioxx, the VIGOR trial, conducted by Merck scientists, continued to track gastrointestinal effects (anticipated to be favorable) after it stopped tracking cardiovascular effects (anticipated to be unfavorable). *See* HAACK, *The Integrity of Science*, *supra* note 48; David Armstrong, *Bitter Pill: How the New England Journal Missed Warning Signs on Vioxx*, WALL ST. J., May 15, 2006, at A1.

dogmatically entrenched in the course of their involvement in the litigation process—and, no doubt, a few outright cranks and a few outright whores.[54] Even when a scientific question is regarded by most people in the relevant scientific community as pretty well-settled, attorneys for the parties to a suit are motivated to seek out as expert witnesses those scientists who still have reservations on the matter. And even when a question is still within the realm of reasonable scientific disagreement, the attorneys are motivated to seek out those scientists who are already most firmly convinced one way or the other. As a result, the legal process can sometimes create spurious, artificial scientific certainty, and spurious, artificial scientific doubt.[55]

Moreover, because advocates will try to get evidence unfavorable to their case excluded, and because settlements may require confidentiality, the adversarial system can contribute to scientific secrecy (withholding evidence) as well as spin (distorting evidence). Not surprisingly, also, when scientific developments attract the attention of the legal community, researchers may find their work interrupted by subpoenas and depositions, and sometimes attacked, as attorneys seek to discredit it.[56]

Because they are specific to a particular case or individual, the questions to which the legal system needs answers are rarely exactly the questions on which the relevant scientific work would ordinarily focus.[57] There may, for

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[54] It has also been suggested that the unappealing possibility of being Dauberted out makes some scientists, including some of those one would like to have involved, reluctant to participate in legal disputes. *See, e.g.*, Heinzerling, *supra* note 25, at 75-77.

[55] *See, e.g.*, David Michaels & Celeste Monforton, *Manufacturing Uncertainty: Contested Science and the Protection of the Public's Health and Environment*, 95 (Supp. 1) AM. J. PUB. HEALTH S39 (2005).

[56] *See, e.g.*, Catharine Arnst, *Keeping Lawyers Out of the Lab: Researchers Gripe that Suits Arising from Their Findings Waste Time and Hurt Their Reputations*, 4020 BUS. WK., Feb. 5, 2007, at 54 (reporting that "[s]cientists who would prefer to plug away quietly in their labs say they are dragged into lawsuits the moment their research turns up a hint of the possible cause of a disease").

[57] This may in part explain, for example, why despite the fact that the dangers of PCBs were already so well known that the stuff had been banned for decades, General Electric was able successfully to defend itself against Mr. Joiner's claim. Precisely because PCBs had long been banned, there were no large, directly relevant epidemiological studies on which Mr. Joiner's attorneys could rely; and GE's attorneys could suggest other, arguably more plausible, explanations of what caused him to develop an aggressive lung cancer at an early age. *See* Michael Gottesman, *From* Barefoot *to* Daubert *to* Joiner: *Triple Play or Double Error?*, 40 ARIZ. L. REV. 753, 766-69 (1998). (Mr. Gottesman represented both the Dauberts and the Joiners at the Supreme Court.)

example, be solid scientific work on the distribution of a disease or disorder in the population as a whole, and reasonable theories about its etiology, but there is unlikely to be scientific work directly relevant to whether this risk factor was a significant cause of *this* person's contracting the disease.[58] Or, as in the case of forensic identification by DNA, where the relevant science is very well-warranted indeed, its application to the samples from *this* crime scene and *this* defendant introduces a whole raft of opportunities for corner-cutting, sloppiness, self-deception, and plain dishonesty.

The very structure of evidence law can create further problems. The evidence that warrants a scientific claim is likely to include a complex mesh of interlocking reasons, none of which by itself would be sufficient to warrant the claim. (This is not to suggest, as some proponents of "weight of evidence methodology" seem to think, that the combined weight of evidence can be assessed by some mechanical formula; it is only to say that, where many lines of evidence interlock in the right way, they may jointly warrant a claim even though none would do so by itself.)[59] But courts screening expert scientific testimony will determine, for each proffered witness, whether his testimony is admissible, and often on what specific matters he may offer an opinion. This may mean that even when there

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[58] When *Daubert* was remanded to the Ninth Circuit, Judge Kozinski ruled the plaintiffs' expert epidemiological testimony inadmissible, finding (1) that "California tort law requires plaintiffs to show not merely that Bendectin increased the likelihood of injury, but that it more likely than not caused *their* injuries"; and (2) that

> [i]n terms of statistical proof, this means that plaintiffs must establish not just that their mothers' ingestion of Bendectin increased somewhat the likelihood of birth defects, but that it more than doubled it—only then can it be said that Bendectin is more likely than not the cause of their injury.

Daubert v. Merrell Dow Pharm., Inc., 43 F.3d 1311, 1320 (1995). Judge Kozinski cited *Jones v. Ortho Pharm. Corp.*, 209 Cal. Rptr. 456, 460 (1985) (requiring that it be "more likely than not" that the injury was caused by defendant's action or product) and *DeLuca v. Merrell Dow Pharm., Inc.*, 911 F.2d 941, 958 (3d Cir. 1990) ("[T]he relative risk of limb reduction defects arising from the epidemiological data [Dr.] Done relies upon will . . . have to exceed '2'."). (I note, however, that there is no reference in *Jones* to a doubling of the risk; and that in *DeLuca*, where there is, the court of appeals reversed and remanded the lower court's grant of summary judgment for Merrell Dow, which had been based on its exclusion of the testimony of Dr. Alan Done that Bendectin did double the risk of limb reduction. *DeLuca*, 911 F.2d at 959.)

[59] *See* Susan Haack, *An Epistemologist Among the Epidemiologists*, 15 EPIDEMIOLOGY 521 (2004), *reprinted in* PUTTING PHILOSOPHY TO WORK, *supra* note 11. *Cf.* Oxendine v. Merrell Dow Pharm., Inc., 506 A.2d 1100, 1110 (D.C. 1986) ("Like the pieces of a mosaic, the individual studies showed little or nothing when viewed separately . . . but they combined to produce a whole that was greater than the sum of its parts: a foundation for Dr. Done's opinion that Bendectin caused appellant's birth defects.").

is a congeries of evidence which, taken together, strongly suggests that exposure to this substance is causally related to that disease or disorder, the rules of evidence may preclude its admissibility because no individual component, by itself, is deemed to meet the *Daubert* standard of reliability.[60]

Not only does the legal system quite often want scientific answers when no warranted answers are available, it also quite often fails to adapt, or adapts painfully slowly, as new scientific answers become available. The law looks to precedent, and courts sometimes continue to follow earlier rulings based on now-superseded science. In Texas death-penalty sentencing hearings, for example, courts continue to rely on psychiatric or "soft" social-scientific testimony as to the likelihood that a defendant convicted of first-degree murder will be dangerous in future—even though (somewhat) more accurate actuarial methods of prediction are now available.[61]

It is no mystery, then, why the law often gets less than the best out of science. But one more task remains: to explain how scientific claims that are not true are sometimes transmuted into legal truths.

V. HOW SCIENTIFIC "TRUTHS" GET ENTRENCHED AS LEGALLY RELIABLE

"True" has the same meaning when it is applied to legal propositions as it does when applied to propositions of other kinds. It is true that domestic cats are related to tigers if and only if domestic cats *are* related to tigers; similarly, it is true that Florida law in 2007 requires that novel scientific testimony be "sufficiently established to be generally accepted in the field to which it belongs," if and only if Florida law in 2007 *does* require that novel scientific testimony be sufficiently established to be generally accepted in the field to which it

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[60] The word that came to my mind when I noticed this problem is "atomism." *But see* Thomas O. McGarity, *Science in the Regulatory Process; On the Prospect of "Daubertizing" Judicial Review of Risk Assessment*, 66 L. & CONTEMP. PROBS. 155, 172-78 (2003) (writing of "corpuscularism").

[61] *See, e.g.*, Thomas Regnier, *Barefoot in Quicksand: The Future of Future Dangerousness Predictions in Death Penalty Sentencing in the World of* Daubert *and* Kumho, 37 AKRON L. REV. 469, 485-89 (2004); Erica Beecher-Monas & Edgar Garcia-Rill, *Genetic Predictions of Future Dangerousness: Is There a Blueprint for Violence?*, L. & CONTEMP. PROBS., Winter/Spring 2006, at 301, 301-02. (I don't mean to imply that the fact that a person convicted of murder will be dangerous in future is a reason to sentence him or her to death; but this is not the place to engage with that issue.)

belongs. However, such legal truths are not exactly like, say, the truths of physics, but are more like social-scientific truths.

Truths to the effect that the law is thus and so have to be understood to be specific to a jurisdiction and to a time (as many truths about social institutions have to be understood to be specific to a society and a time). For example, in 2003 it was true that Michigan law required admissible scientific testimony to satisfy the *Frye* test, but since January 1, 2004, it has been true that Michigan law requires expert, including scientific, testimony to satisfy the *Daubert* requirements.[62] Sometimes it is neither true nor false that the law in such-and-such a jurisdiction is thus and so; for example, until the Supreme Court settled the question in *Daubert*, it was neither true nor false that FRE 702 superseded the *Frye* Rule in federal courts. And what makes it true that the law in such-and-such a jurisdiction at such-and-such a time is thus and so is what legislators and courts do.[63]

It is standard to distinguish statements of fact, such as "there was a stop sign at the intersection on the day of the accident," from statements of law, such as "felony murder is an unlawful homicide occurring in the commission or attempted commission of a felony." (It might, perhaps, be better to speak of statements of law versus statements of non-legal fact; but the more standard terminology will serve.) Usually, the intended distinction is straightforward enough, but there are significant borderline and mixed cases, one of which is directly to the present purpose. Questions about admissibility would be classified as questions of law, falling within the province of the court; questions about the weight of evidence would be classified as questions of fact, falling within the province of the jury. But rules about the admissibility of expert testimony have blurred this apparently simple dichotomy.

Specifically, by requiring courts to screen proffered expert scientific testimony for reliability as well as relevance, and thus extending their preserve, *Daubert* has shifted some questions formerly conceived as concerning the weight of

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[62] See People v. Wright, No. 04-001187-FC, 2006 WL 2271264, at *5 (Mich. App. Aug. 8, 2006) (The defendant "argue[d] that [Leuco Crystal Violet] testing cannot pass the *Davis-Frye* 'general acceptance' standard." However, "[Michigan Rule of Evidence] 702 was specifically amended, effective January 1, 2004, to incorporate the *Daubert* standards . . . ." (citations omitted)).

[63] *See also* Susan Haack, *Truth, Truths, "Truth," and "Truths" in the Law*, 26 HARV. J.L. &. PUB. POL'Y 17, 19-20 (2003).

evidence into the category of questions bearing on admissibility.[64] This is why attorneys sometimes complain that, since *Daubert*, they are in effect obliged to try their cases twice—first at an evidentiary hearing before the court, and then a second time before the jury; and why, if one party's expert testimony is excluded, the case may well end in summary judgment against them. In short, the concept of evidentiary reliability seems to straddle the line between law and fact.

Like the legal concepts of insanity, causation, or intoxication, the legal concept of reliability is at once closely related to, and yet importantly distinct from, ordinary, non-technical concepts. The legal concept of insanity isn't exactly the same as everyday conceptions of madness, but isn't entirely disjoint from them; the legal concept of causation isn't exactly the same as everyday conceptions of cause, but isn't entirely disjoint from them; the legal concept of intoxication isn't exactly the same as everyday conceptions of drunkenness, but isn't entirely disjoint from them; and similarly, the legal concept of reliability isn't just the same as everyday understandings of reliability, but isn't entirely disjoint from them, either.

In fact, evidentiary reliability is a puzzling, Janus-faced concept. The doubleness is already close to the surface in *Daubert*, in that curious footnote where Justice Blackmun tells us that reliability, in the specifically legal sense he is articulating, is not to be identified with "scientific reliability" (which he takes to mean that "application of the principle gives consistent results"), but corresponds to "scientific validity" (which he takes to mean that "the principle supports what it purports to show"). But then he goes on to assure us that evidentiary reliability is simply a matter of the "trustworthiness" of the testimony[65]—which, however, is surely significantly stronger than "the principle supports what it purports to show."

You might wonder what *degree* of reliability is required for expert testimony to be admissible.[66] For in ordinary speech,

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[64] Under the *Frye* test, also, rulings on the admissibility of novel scientific testimony involve an element of fact; and commentators have expressed concern that *Frye* might, as Michael Graham puts it, set once-accepted science in "appellate concrete" and fail to adapt if the scientific community subsequently changes its mind in response to new evidence. *Symposium on Science and the Rules of Evidence*, 99 F.R.D. 188, 232-33 (1983). But here I will focus on *Daubert*.

[65] *Daubert*, 509 U.S. at 590 n.9.

[66] *See, e.g.*, Dale Nance, *Two Concepts of Reliability*, 3 APA NEWSL. ON PHIL. & L. (American Philosophical Ass'n) 123, 125-27 (2003); Haack, *Peer Review and Publication*, *supra* note 22.

though "reliable" has quite a tangle of uses, in all them—whether we are speaking of inanimate objects such as watches or motor vehicles, or of persons, or of information, data-bases, etc.—it connotes something, fitness to be relied upon, that comes in degrees. But admissibility is *not* a matter of degree, which means that the *Daubert* ruling requires evidentiary reliability to be categorical.[67] Most to the present point, though, the rationale for introducing the concept of evidentiary reliability depends crucially on its connection with the ordinary concept of reliability-as-trustworthiness; but *qua* legal concept evidentiary reliability simply requires that the testimony at issue satisfy certain legally specified conditions.[68] There is, however, absolutely no guarantee that all, or only, testimony deemed by courts to meet the standards of evidentiary reliability is trustworthy, that is, is reliable in the ordinary sense of the word.[69]

In *Joiner*, rejecting respondents' (Joiner's attorneys') argument that the trial court's evidentiary ruling excluding their proffered expert testimony should have been subject to especially stringent review, given that this exclusion was outcome-determinative, Justice Rehnquist writes: "On a motion for summary judgment disputed issues of fact are resolved against the moving party . . . . But the question of admissibility of expert testimony *is not such an issue of fact* . . . ."[70] Here—even though, had the evidence been admitted, its degree of reliability would have been a factual matter for the jury to determine—the question of evidentiary reliability is treated as a purely legal matter.

Claims to the effect that this or that scientific evidence is legally reliable can be made true by legal decisions.

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[67] *See, e.g.*, *In re* Paoli RR. Yard PCB Litig., 35 F.3d 717, 744 (3d Cir. 1994) (observing that "[t]he evidentiary requirement of reliability is lower than the merits standard of correctness"); *see also* Nenno v. State, 970 S.W.2d 549, 561 (Tex. Crim. App. 1998) (holding that social-scientific evidence should not be required to meet standards of reliability as high as those demanded for the admissibility of natural-scientific testimony).

[68] Conditions which, however, are significantly *less* precisely specified since *Kumho*.

[69] Compare this, on the preponderance of evidence standard of Federal Rule of Evidence 104(a), from *Bourjaily v. United States*, 483 U.S. 171, 175 (1987): "The inquiry made by a court . . . is not whether the proponent of the evidence wins or loses his case on the merits, but whether the evidentiary Rules have been satisfied."

[70] *Joiner*, 522 U.S. at 143 (emphasis added); *see also* Heinzerling, *Doubting* Daubert, *supra* note 25, at 80 (arguing that *Joiner* makes evidentiary reliability a kind of legal chimera, neither an issue of fact to be decided in favor of the nonmoving party nor an issue of law subject to de novo review).

However—verbal appearances to the contrary—it doesn't follow that scientific truths can be legally constructed. Legal truths are made true by legal decisions; and so, sometimes, are scientific "truths." But it is the character of phenomena and events in the world that scientific propositions describe—*not* legal decisions about evidentiary reliability, and *not* arguments and cross-examination in court—that make true scientific propositions true, and false scientific propositions false.