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2007]

WHERE THE RUBBER MEETS THE ROAD: THINKING ABOUT
EXPERT EVIDENCE AS EXPERT TESTIMONY

SIMON A. COLE*

IT has been more than a decade since the United States Supreme Court first delved into the problem of defining the limits of scientific (or more precisely, expert) evidence within the law in the now famous *Daubert v. Merrell Dow Pharmaceuticals, Inc.* decision and its “progeny.”¹ During this period, what has sometimes been called a “cottage industry” of *Daubert* scholarship has arisen, generating an enormous amount of legal scholarship and discussion.² This attention is far from unwarranted. The use of expert evidence in the law clearly is an enormously important topic, and *Daubert* itself is now considered by evidence scholars, “perhaps the most important evidence case ever decided.”³

This symposium celebrates the publication of the book *No Magic Wand*, the culmination and summation of Professors Caudill and LaRue’s

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1. See *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579 (1993); see also *Kumho Tire v. Carmichael*, 526 U.S. 137 (1999); *Gen. Elec. Co. v. Joiner*, 522 U.S. 136 (1997).

2. See Lawrence S. Pinsky, *The Use of Scientific Peer Review and Colloquia to Assist Judges in the Admissibility Gatekeeping Mandated by Daubert*, 34 HOUS. L. REV. 527, 528 (1997).

3. See Paul Giannelli, *Daubert Revisited*, 41 CRIM. L. BULL. 5 (2005).

substantial contribution to this discussion.⁴ Professors Caudill and LaRue's contributions are of particular interest to me because they attempt to bridge the discussion between two very different literatures: a literature that explores the relationship between science, technology and law from the point of view of the discipline of Science and Technology Studies ("S&TS") and the more familiar (to legal readers) doctrinal legal scholarship on expert evidence. Because S&TS, as a discipline, endeavors to produce empirically accurate accounts of scientific and technological knowledge-making as practices, it might reasonably be expected to have something to contribute to legal discourses that center around the need to appropriately handle expert evidence produced by scientists and technicians. As a scholar who was trained in S&TS and now works in a Law and Society program, Professors Caudill and LaRue's efforts to synthesize these two streams of thought are of particular interest to me. In this paper, therefore, I would like to treat the publication of *No Magic Wand* as an occasion to take stock of the debate over expert evidence with particular emphasis on the potential contribution to this debate that has been, or could be, offered by S&TS. I will argue that the purported debate between legal and social scientific scholars, though still unresolved and perhaps irresolvable, masks large areas of consensus between most scholars. In particular, I will argue that those disagreements that do remain between legal and social scientific scholars are of little consequence in resolving how to best move forward in helping courts resolve what I call "the expert evidence problem."

I. THE EXPERT EVIDENCE PROBLEM & JUDICIAL GATEKEEPING

By way of introduction, it useful to begin by reiterating why expert evidence is considered a problem, and why judicial gatekeeping is considered a solution to that problem. As Professors Denbeaux and Risinger note, the mere fact that expert evidence can be wrong or "bad" is not sufficient to render expert evidence problematic, because that is true of all evidence.⁵ Expert evidence can be combated like all other evidence through mechanisms such as cross-examination, presentation of contrary evidence and exclusion for lack of relevance. Indeed, the *Daubert* opinion contains a passage that would seem to imply that these should be the normal modes of combating expert evidence.⁶ Arguably, it might be inferred that the Court believed litigants should resort to admissibility challenges only in extraordinary cases. Some evidence scholars, for example, have

4. See DAVID S. CAUDILL & LEWIS H. LARUE, *NO MAGIC WAND: THE IDEALIZATION OF SCIENCE IN LAW* (2006).

5. See generally Mark Denbeaux & D. Michael Risinger, *Kumho Tire and Expert Reliability: How the Question You ask Gives the Answer You Get*, 34 SETON HALL L. REV. 15 (2003).

6. See *Daubert*, 509 U.S. at 596 ("Vigorous cross-examination, presentation of contrary evidence, and careful instruction on the burden of proof are the traditional and appropriate means of attacking shaky but admissible evidence.").

now taken the position that *Daubert's* admissibility requirement is not necessary at all and that expert evidence should simply be treated like all other evidence.⁷ Of course, perhaps predictably, that has not been the case; in creating an admissibility requirement (or reinforcing the existing one), the Court created a procedural structure that is now simply treated as yet another weapon in the litigation arsenal.⁸

The courts essentially have two choices in dealing with expert evidence. One option is to take the advice of those scholars cited above, allow all expert evidence into court and rely on that great truth-producing engine,⁹ cross-examination, to sort good evidence from bad. The other is to seek to “manage” expert evidence in some way—that is, to adopt some sort of admissibility procedure, mechanism, threshold, etc., by which the fact-finder is not permitted to hear some expert evidence. As Professors Denbeaux and Risinger cogently note, the Court’s decision to opt for the latter, whether under a *Frye* or a *Daubert* regime, necessarily implies that it has determined that the former solution is inadequate.¹⁰ The reason for this must be that some expert evidence has the potential to be particularly misleading and therefore poisonous to the fact-finder, so poisonous indeed that the fact-finder must be protected from the expert (hence the gatekeeping metaphor). And, the reason for *that* must have something to do with the authority that is accorded “experts” in contemporary society. The Court seems to be reasoning that weak, downright misleading or even false evidence cloaked in the guise of expertise is far more pernicious than weak evidence lacking that imprimatur. Thus, we have admissibility standards so that those proffered witnesses who choose to don the mantle of “expert” may be held to a higher standard than the mere “relevance” required of ordinary witnesses. Under *Daubert*, “relevance” is neatly paired with an equally succinct R concept: “reliability.”

The focus of the expert evidence literature has been primarily on the second R, “reliability.” The Court’s framework generates all sorts of vexing questions, such as how reliability is to be determined and whether judges are competent and consistent at determining reliability. I will not retrace those debates here.

For my purposes, I will note one perhaps unanticipated consequence of admissibility framework. The Court mandated a dichotomous framework for evaluating expert evidence. In the gatekeeping framework, evidence is either admissible or inadmissible. This is plainly inconsistent with the nature of expert evidence itself, which must be arrayed along a continuum of reliability between highly reliable evidence and evidence that is

7. See Richard D. Friedman, *Squeezing Daubert Out of the Picture*, 33 SETON HALL L. REV. 1047 (2003).

8. See Herbert M. Kritzer, *Daubert in the Law Office: Routinizing Procedural Change*, Address at the Annual Meeting of Law and Society Association (July, 2006).

9. See 5 WIGMORE, EVIDENCE § 1367, at 32 (Chadbourne Rev. 1974).

10. See Denbeaux & Risinger, *supra* note 5.

not reliable at all. With admirable economy, Professor Haack summed up my point as follows: “admissibility” is categorical; “reliability” is continuous.¹¹ The dichotomous framework obviously invites the usual problems of drawing bright-line distinctions for phenomena which exist on a continuum. In addition, it presumably undervalues some evidence (highly reliable evidence that is treated as legally equivalent to far less reliable but nonetheless “admissible” evidence, plus pretty good evidence that just fails to meet the admissibility threshold but legally is considered equivalent to “junk” evidence) and overvalues other evidence (marginally reliable evidence that just barely meets the admissibility threshold but is legally equivalent to highly reliable evidence, plus “junk” evidence that has no redeeming value whatsoever, but nonetheless is considered legally equivalent to evidence that has some value, but not quite enough to meet the admissibility threshold).

It is important to note the homology between *Daubert's* admissibility/inadmissibility dichotomy and a closely related dichotomous metaphor that was quite prevalent at the time: Peter Huber’s “junk science”/authentic science metaphor. The relationship between *Daubert* and Huber’s influential popular book on the supposed epidemic of “junk science” in the courts, and his popularization, if not coining, of this term, is complicated, and I will not fully explore it here.¹² The point is that Huber’s propagation of the notion that there was a thing called “junk science” that could easily and reliably be distinguished from “real” science may have subtly reinforced the idea that assigning expert evidence to neat categories of “admissible” and “inadmissible” evidence would be both workable and relatively unproblematic.

Consider, for example, Huber’s characterization of “junk science” as the “mirror image” of authentic science.¹³ Huber’s metaphor could be read in at least two ways. On the one hand, one might read “mirror image” to mean “diametrically opposed,” in which it should be easy to distinguish “junk” from “real” science. On the other hand, the term “mirror image” is also used to denote objects that are indistinguishable, or barely distinguishable, from one another, as in the phrase “mirror image twins.” In this sense, Huber’s metaphor points precisely to the issue that will become the problem: that trial judges will find themselves in a house of mirrors in which every expert appears to be legitimate or, worse, one in which

11. I am grateful to Professor Haack, who made this point at the symposium and who explicates this point clearly elsewhere in her work. See Susan Haack, *Not Cynicism, but Synechism: Lessons from Classical Pragmatism*, 41 *Transactions of the Charles S. Peirce Soc’y* 239, 240 (2005); see also, D. A. Nance, *Reliability and the Admissibility of Experts*, 34 *SETON HALL L. REV.* 191, 200 (2003) (“[S]cientific validity is not an all-or-nothing characteristic; rather, it is a matter of degree.”).

12. For a suggestive account, see generally Gary Edmond & David Mercer, *Litigation Life: Law-Science Knowledge Construction in (Bendectin) Mass Toxic Tort Litigation*, 30 *SOC. STUD. OF SCI.* 265 (2000).

13. See PETER W. HUBER, *GALILEO’S REVENGE: JUNK SCIENCE IN THE COURTROOM* (1991).

the false experts appear trustworthy (because they are “slick” charlatans and con men) and the true experts appear untrustworthy (because as legitimate experts, they are unpolished, bumbling, socially awkward “geeks”).¹⁴ Indeed, it is wise to recall that the reason we developed the admissibility standard in the first place was that we were worried about pseudo-experts who would appear legitimate in the eyes of the jury, not about pseudo-experts who were readily identifiable as such. In fact, the whole idea of the pseudo-expert and all the rich terms used to describe such persons—charlatans, quacks, mountebanks, imposters, frauds, fakes, swindlers and con artists—is that they are untrustworthy experts who appear trustworthy, not untrustworthy experts who are identifiable as such.¹⁵

In this sense *Daubert* would seem to presuppose its own ineffectiveness, in that it asks legal actors to discriminate between trustworthy and untrustworthy experts for the precise reason that it is difficult for jurors to distinguish between them. As a logical matter, *Daubert* can only be saved by assuming that judges, the legal actors *Daubert* asks to draw the distinctions, have a power of discernment that jurors, the legal actors who are presumed unable to draw the distinctions, lack. This assumption has been called into question by recent research, and it probably remains questionable despite Professors Caudill and LaRue’s cogent critique of that research.¹⁶

At this point, one cannot help but mention yet another dichotomous framework that is also closely related to the *Daubert* regime. That, of course, is the philosopher Sir Karl Popper’s theory of falsificationism, through which knowledge claims can be “demarcated” into two categories: science and pseudo-science. As many of the participants in this symposium have already explained, this theory is no longer current among philosophers of science.¹⁷ The association of *Daubert* with Popper comes

14. See Sanja K. Ivkovic & Valerie P. Hans, *Jurors’ Evaluations of Expert Testimony: Judging the Messenger and the Message*, 28 LAW AND SOC. INQUIRY 441, 442 (2003) (“One key assumption underlying the *Daubert* line of cases is that jurors might be duped by a persuasive but untrustworthy expert who testifies about matters that are not based on sound scientific principles or data.”).

15. I cannot resist referring here to the epigraph of one of my articles in which a character in a play by the late August Wilson remarks, “I never would have guessed he was selling fake insurance.” Simon A. Cole, *More Than Zero: Accounting for Error in Latent Fingerprint Identification*, 95 J. CRIM. L. & CRIMINOLOGY 985, 985 (2005).

16. See CAUDILL & LARUE, *supra* note 4, at 55. See generally Veronica Dahir et al., *Judicial Application of Daubert to Psychological Syndrome and Profile Evidence*, 11 PSYCHOL. PUB. POL’Y & L. 62 (2005); Sophia I. Gatowski et al., *Asking the Gatekeepers: A National Survey of Judges on Judging Expert Evidence in a Post-Daubert World*, 25 LAW & HUM. BEHAV. 433 (2001) (finding high percentage of federal judges have poor comprehension of *Daubert* factors and poor scientific literacy).

17. See generally David S. Caudill & Richard E. Redding, *Junk Philosophy of Science? The Paradox of Expertise and Interdisciplinarity in Federal Courts*, 57 WASH. & LEE L. REV. 685 (2000); Gary Edmond & David Mercer, *Conjectures and Exhumations: Citations of History, Philosophy and Sociology of Science in US Federal Courts*, 14 LAW & LIT. 309 (2002) (characterizing *Havvard* as arguing that “longstanding use tri-

from several sources. First, the *Daubert* opinion explicitly cites Popper,¹⁸ a move which has been variously interpreted as a more or less wholehearted endorsement of his philosophy of science. Second, there is a misguided but understandable tendency to equate the demarcation of admissible from inadmissible evidence with the demarcation of science from pseudo-science. Since the popular educated imagination still holds that falsificationism “solved” the demarcation problem, it is natural to assume that Popper holds the key to *Daubert*’s problem.¹⁹

II. FROM SCIENTIFIC EVIDENCE TO EXPERT TESTIMONY

Although both philosophers of science (and other social scientific inquirers about science such as those in the discipline of S&TS) and courts take as their purported object of inquiry something called “science,” for the most part they are dealing with quite different objects. Philosophers of science are generally dealing with “science,” defined as an open-ended mode of inquiry for developing accurate knowledge about the natural world. The scientists about whom philosophers of science and S&TS scholars are seeking to generate plausible accounts are generally engaged in open-ended inquiries seeking to generate original new knowledge about phenomena that are little understood.

Courts, in contrast, are by and large dealing with scientific *evidence*. Generally, the knowledge claims that are advanced in courts are not the cutting edge knowledge claims that occupy both academic scientists and the philosophers and sociologists who try to account for those scientists. There are few court cases about, for example, string theory. Legal knowledge claims tend to be much more mundane applications of existing knowledge claims: How convincing is the evidence that this pathogen causes this disease? This is particularly the case for forensic science. Very little, if any, of what is called “forensic science” consists of the sort of open-ended basic research that is classically the object of philosophy of science. Some small portion of forensic science might consist of efforts to evaluate whether certain new technologies might prove forensically useful. The overwhelming majority of what is called “forensic science,” however, consists of routine applications of existing assays to new materials. This is philosophically equivalent to the routine use of various assays in a large scientific laboratory. Although some in S&TS have valorized this “technical” work and emphasized its importance in constructing what counts as scientific knowledge,²⁰ this is not generally the sort of work that occupies

umphs over the strict application of falsificationism”); Susan Haack, *Trial and Error: The Supreme Court’s Philosophy of Science*, 95 AM. J. PUB. HEALTH 66 (2005).

18. See *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 593 (1993).

19. Indeed, it may be argued that this was precisely what Justice Blackmun was thinking when he cited Popper in *Daubert*.

20. See, e.g., STEVEN SHAPIN, *A SOCIAL HISTORY OF TRUTH*, 355 (1994); Kathleen Jordan & Michael Lynch, *The Mainstreaming of a Molecular Biological Tool: A*

philosophers of science, such as Popper, who try to articulate how “science” may be distinguished from other endeavors.

In short, the *Daubert* Court, faced with the question of whether a particular set of evidential claims were properly considered in assessing whether disease causation could be established, drew on philosophers who were engaged in quite a different project. The philosophers of science that the Court drew on, such as Popper, were engaged in the larger project of how to characterize the broad enterprise known as “science.” In particular, they were interested in those areas of that enterprise in which open-ended inquiry is made and new knowledge is generated. In other words, they were more interested in what went on at Princeton’s Institute for Advanced Study than in what went on at a commercial diagnostic laboratory, even though both may appropriately be viewed as sites where “science” occurs.

At a superficial level, of course, the Court’s use of philosophy of science made sense. The Court was engaged in defining proper scientific evidence, and philosophy of science may have seemed the area of academic inquiry that came closest to addressing that problem. Nevertheless, in fact, the subtle differences between the two endeavors may have been more consequential than the Court anticipated.

One consequence has been on the academic debate about the *Daubert* opinion. If, as I claim, Popper was simply inappropriate for the task with which the Court was faced, this renders largely moot the debate about whether Popper is “correct” or “generally accepted” by philosophers and others who study science. The unfortunate confusion between *Daubert* and Popper’s projects has been responsible for a great deal of needless disagreement among scholars and legal actors alike concerning the problem of the expert witness.

Distinguishing admissible from inadmissible expert evidence is quite a different thing from distinguishing science from pseudo-science. First, not all expert evidence is necessarily science. It is well established that legitimate experts can be technicians or others with “specialized knowledge.” This issue was in some sense resolved by *Kumho Tire*, which clarified the Court’s view that *Daubert*’s relevance and reliability requirement extended to all expert evidence.²¹ Yet, as Professor Edmond points out, that Court did so by fitting an admissibility threshold that had been devised for an idealized model of *science* to all expert evidence, whether it claimed to be “science” or not, something he calls “science through the back door.”²² Second, philosophers of science, like Popper, tend to think about knowledge claims in larger units of analysis than do lawyers and

Case Study of a New Technique, in TECHNOLOGY IN WORKING ORDER: STUDIES OF WORK, INTERACTION, AND TECHNOLOGY 162 (Button ed., 1993).

21. See *Kumho Tire v. Carmichael*, 526 U.S. 137 (1999).

22. See Gary Edmond, *Legal Engineering: Contested Representations of Law, Science (and Non-Science) and Society*, 32 SOC. STUD. OF SCI. 371 (2002).

judges. Philosophers of science tend to frame problems in terms of disciplines, research programs or, at least, lines of research. How do we know that biology produces reliable knowledge? Or molecular biology? Or evolutionary theory?²³ Admissibility determinations tend to focus on smaller units of analysis: How do we know whether this study establishes causation of this pathology? The distinction is important because philosophers are generally thinking about how one can defend the knowledge claims of a broad approach to knowing the world, whereas judges are thinking about how one can defend one very specific knowledge claim.

Finally, admissibility is fundamentally a policy determination, whereas Popper was concerned with defining “science” as a particular, and special, form of inquiry. It might be argued that the parallel is not really between admissibility and science, but between reliability and science. But even then, the parallel does not hold; there are all sorts of highly reliable forms of knowledge that are nonetheless not science.

Simply put, *Daubert* is clearly about the distinction between reliable and unreliable, and thus admissible and inadmissible, evidence. Nowhere does it purport to be about distinguishing science from pseudo-science. To be sure, the opinion helped confuse the issue because it discussed reliability in the context of scientific evidence, and, indeed, there is much slippage in the opinion between describing things as “reliable” and “scientific.” For example:

[I]n order to qualify as “scientific knowledge,” an inference or assertion must be derived by the scientific method. Proposed testimony must be supported by appropriate validation—i.e., “good grounds,” based on what is known. In short, the requirement that an expert’s testimony pertain to “scientific knowledge” establishes a standard of evidentiary reliability.²⁴

In this passage, “scientific” status is equated with both validation and reliability, neither of which necessarily require that a claim be categorized as scientific knowledge or be derived by the scientific method.²⁵ Nor, for that matter, must every claim that we would call “scientific” be validated or even be susceptible to validation. Given this kind of language, it is no wonder that much of the discussion of *Daubert*, from the pages of law journals to the courtrooms, has focused on whether particular knowledge claims are “scientific.” The *Kumho Tire* case might be thought to have remedied some of the confusion; in holding that the *Daubert* gatekeeping re-

23. To be sure, this is not completely true. Popper often discussed specific knowledge claims, such as “all swans are white,” but he also discussed whether entire disciplines, such as psychoanalysis and Marxist history, were scientific.

24. *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 590 (1993).

25. See Edward J. Imwinkelried, *The Meaning of “Appropriate Validation” in Daubert v. Merrell Dow Pharmaceuticals, Inc., Interpreted in Light of the Broader Rationalist Tradition: Not the Narrow Scientific Tradition*, 30 FLA. ST. U. L. REV. 735 (2003).

quirement applied to all expert evidence, including that based on technical or specialized knowledge, the court implicitly clarified that not all expert evidence need be science. But, as Professor Edmond has noted, the effect of *Kumho* seems to have been further confusion, in that Popper's standards for demarcating science from pseudo-science end up being applied to technical evidence.²⁶

Again, one can readily understand how, on a superficial level, the Court may have been gulled into thinking that the demarcation of admissible from inadmissible or reliable from unreliable evidence was the same as the demarcation of science from pseudo-science. And, one can then understand how, saddled with this misapprehension, the Court would view it as appropriate to draw on by far the most famous demarcator of all, Popper. Yet, again, this issue is even more fundamental than the mere fact that Popper was "wrong" or that his view is no longer, if it ever was, widely accepted among professional philosophers. The more fundamental issue is that demarcating science from pseudo-science is very seldom, if ever, a useful exercise in any endeavor, and it is certainly not helpful in solving the problem that faced the *Daubert* Court. As Professor Haack points out, two meanings of the word "scientific," one referring to a distinct mode of inquiry and the other being a honorific meaning something like "good," "objective" or "well intentioned," have become thoroughly confused in modern parlance.²⁷ The Court's task really had to do with the latter; it was necessary to distinguish generally well supported evidence from poorly supported evidence—that is, evidence that, in donning the mantle of expertise, was more likely to mislead than to educate the fact finder—rather than to distinguish "scientific" from "pseudo-scientific" evidence.

In sum, the *Daubert* regime has been characterized by a great deal of slippage, in both legal scholarship and practice, between the quite different issues of whether evidence is "reliable" or "admissible" and whether it is "scientific." One example of this may be found in the area I know best: the admissibility battles over fingerprint evidence, in which legal actors—including, I would readily admit, myself—exhibited considerable slippage between these two issues.²⁸ This slippage confused the record made in these admissibility hearings. Only in the 2004 decision *United States v. Mitchell*, five years after the first hearing, did a court bring clarity to the distinction.²⁹

26. See generally Edmond, *supra* note 22.

27. See Haack, *supra* note 11, at 248.

28. See Michael Lynch & Simon A. Cole, *STS on Trial: Dilemmas of Expertise*, 35 SOC. STUD. OF SCI. 269 (2005); see also MICHAEL LYNCH ET AL., TRUTH MACHINE: THE CONTENTIOUS HISTORY OF DNA FINGERPRINTING (forthcoming).

29. See generally *United States v. Mitchell*, 365 F.3d 215 (3d Cir. 2004); Simon A. Cole, *Does 'Yes' Really Mean Yes? The Attempt to Close Debate on the Admissibility of Fingerprint Testimony*, 45 JURIMETRICS J. 449 (2005).

III. SCIENCE & TECHNOLOGY STUDIES (S&TS) AND *DAUBERT*

The problem of whether and how much to trust purported “experts” is fundamental to my native discipline of S&TS. One of several possible characterizations of the field of S&TS is that it is the study of experts and expert knowledge.³⁰ It so happens that some efforts have recently been made to, in effect, re-articulate the mission of the field of S&TS around this very question of what to do about experts, or more precisely, how to assign attributions of expertise to various actors in resolving social questions that invoke some sort of technical knowledge.³¹ To be sure, this effort has been resisted.³² Yet, the resistance has in part argued that this supposedly necessary re-articulation has already occurred. There is little serious dispute that expertise is a necessary and important object of study for S&TS. Where differences arise is over the nature of the study. Most S&TS scholars take as their object of study the very act of attribution of expertise by social actors (for instance, lawyers and judges). Their interest is documenting, studying, analyzing and revealing the process by which the status of “expert” is attributed or denied to various claimants.³³ Professors Collins and Evans argue that this work is largely complete³⁴ and that its point—that “expertise” is something that is attributed to social actors by other actors, rather than a natural category that exists outside of social interactions—has been made. They argue that it is time to move on to a new project, a normative project which would involve sorting out how to *properly* manage attributions of expertise, given the undeniable fact that such attributions cannot be made “naturally.”³⁵

Although the bulk of *Daubert* discussion has taken place largely within the confines of legal doctrinal scholarship, it is perhaps fair to say that S&TS has made a modest contribution to the discussion. Indeed, one

30. I am not sure where, or if, this is explicitly stated, but the person who crystallized it for me was Professor Michael Aaron Dennis.

31. See generally H.M. Collins & Robert Evans, *The Third Wave of Science Studies: Studies of Expertise and Experience*, 32 SOC. STUD. OF SCI. 235 (2002).

32. See, e.g., Sheila Jasanoff, *Breaking the Waves in Science Studies: Comment on H.M. Collins and Robert Evans*, “*The Third Wave of Science Studies*,” 33 SOC. STUD. OF SCI. 389 (2003); Arie Rip, *Constructing Expertise—in a Third Wave of Science Studies?* 33 SOC. STUD. OF SCI. (2003); Brian Wynne, *Seasick on the Third Wave? Subverting the Hegemony of Proportionalism*, 33 SOC. STUD. OF SCI. (2003).

33. Exemplary in this regard is Sheila Jasanoff, *Expert Games in Silicone Gel Breast Implant Litigation*, in *SCIENCE IN COURT* 83 (Freeman and Reece eds., 1998). Another excellent contributor is none other than CAUDILL & LARUE, *supra* note 4.

34. See Collins & Evans, *supra* note 32. Professors Collins and Evans are careful to note that this sort of work is still worth doing and that they still do it.

35. See *id.* In so doing, Collins and Evans argue against what may or may not be a caricature of the rest of S&TS, which is that most S&TS scholars favor a “leveling” of expertise attributions: essentially, if expertise is not a natural category, everyone should be accorded equal expertise over every technical question. Collins and Evans argue that this is plainly absurd and that some better normative guidelines for defining relevant expertise are necessary. The dispute, in part, is about whether S&TS scholars actually take the position that Collins and Evans ascribe to them.

S&TS scholar was cited in the *Daubert* opinion, although one can fairly debate whether it was in fact a “sincere” citation or an “opportunistic” one (that is, a cynical or ignorant use of an authority to support a proposition that did not actually accord with the author’s intent).³⁶ There are several reasons for S&TS interest in expert evidence problems. First, the practical problem facing the *Daubert* Court—how to evaluate the trustworthiness of expert evidence—is a problem over which both scholars allied with the field of S&TS³⁷ and more conventional philosophers of science might be considered to have some disciplinary “jurisdiction,” although, as I will argue below, there are also strands within both fields that tend toward *dis-*

36. See *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 593 (1993); see also Edmond & Mercer, *Conjectures*, *supra* note 17 (arguing that legal citations to philosophy of science endorse philosophical views generally considered outmoded among philosophers and that there are few legal citations to sociology of science); Gary Edmond & David Mercer, *The Invisible Branch: The Authority of Science Studies in Expert Evidence Jurisprudence*, in *EXPERTISE IN REGULATION AND LAW* 197 (Edmond ed., 2004) (arguing that this citation, as well as others to S&TS, all use S&TS authorities to support points not intended by authors and generally inconsistent with S&TS). These articles incidentally predate the citation of the author’s work in several cases, at least some of which make points consistent with my intent (*United States v. Crisp*, 324 F.3d. 261 (4th Cir. 2003) (Michael, J., dissenting)) and perhaps even with generally held S&TS views. See generally *United States v. Kincade*, 379 F.3d 813, 874 (9th Cir. 2004) (Kozinski, J., dissenting). S&TS scholars, as well as historians of science at least loosely associated with S&TS also signed *amicus curiae* briefs that were filed in *Daubert* but not cited in the text of the opinion. See Brief of Amici Curiae of Physicians, Scientists and Historians of Science in Support of Petitioners, *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 1992 WL 12006437 (1992); Brief of Amici Curiae Daryl E. Chubin, Ph.D. Edward J. Hackett, Ph.D. David Michael Ozonoff, M.D., M.P.H. Richard W. Clapp, Sc.D., M.P.H., in Support of Petitioners, *Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 1992 WL 12006332 (1993).

37. Defining the field of S&TS is problematic. A literal definition would hold that S&TS would include anyone who studies science and technology from any scholarly disciplinary perspective. Moreover, S&TS is widely understood to be an interdisciplinary endeavor and to be an outgrowth of the social science, the humanities or both. At the same time, however, it is undeniable that the term “S&TS” has come to be associated with something more specific that would be closer to a particular intellectual movement or school of thought than to a generic term for any study of the institutions of science and technology from any discipline or perspective. This movement is generally associated with the sociology of scientific knowledge, or even more specifically with “the strong programme in the sociology of scientific knowledge,” and it is to this movement that scholars often refer when they talk about S&TS or “science studies.” See Haack, *supra* note 17. As I have noted elsewhere, this ambiguity can be problematic, especially when S&TS *itself* becomes a subject of *Daubert* inquiry. In such circumstances, S&TS cannot possibly be defined as an intellectual movement (to do so would be akin to defining “chemistry” intellectually, i.e. as people who believe in certain chemical theories, rather than as people who study the chemical makeup of matter). But, there is sufficient disagreement among scholars who study science that, if S&TS is defined broadly and generically, it becomes very difficult to make general statements about the field’s view on anything, including such fundamental issues as the nature of science itself. Especially pronounced and best known in this regard is the disagreement between many philosophers of science and many sociologists of science over generally “realist” or “relativist” views of scientific knowledge. See *id.*

interest in this problem. Second, social and institutional actors' attempts to define the limits of scientific knowledge are inherently interesting to S&TS scholars—they are data to us. Thus, the law's attempt to limn scientific knowledge has become a topic of study, albeit a relatively small one, within S&TS.³⁸ The final reason, perhaps the least significant motivation, is that some scholars may believe that the S&TS knowledge itself—that is, social scientific knowledge about science, technology and expertise—might actually be useful to legal actors faced with expert evidence problems. It is this last point that, at least in part, motivates Professors Caudill and LaRue's book *No Magic Wand*, as well as my own work in this area.

Much of the early S&TS *Daubert* scholarship considered whether *Daubert* "got science right." Did *Daubert*, in other words, evince an accurate understanding of the enterprise of scientific (and expert) knowledge-making, or at least an understanding informed by the latest and best social science research on the issue? The answer, explicated in perhaps the greatest detail in the collected works of the teams of Professor Caudill and his collaborators, Professors Redding and LaRue, and Professors Edmond and Mercer, was "no."³⁹ Of course, one need not have been an S&TS scholar to find *Daubert's* understanding of science lacking. For instance, while the thrust of Professors Haack and Schwartz's critiques are quite different, both found the explicit and implicit philosophy of science contained in *Daubert* to be inconsistent, incoherent and outmoded.⁴⁰ Professors Caudill and Redding, in famously⁴¹ calling *Daubert* "junk philosophy of science,"⁴² were making a different point. They faulted *Daubert* not only for using outmoded philosophy of science, but also for ignoring the contributions of history, sociology and anthropology of science—in a word, S&TS—to the discussion of expert evidence.

Most evidence scholars have tended to accept *Daubert's* supposed philosophy of science at face value.⁴³ Whether they did so because that was the philosophy of science to which they adhered or simply because that was the *Daubert* Court's philosophy of science was not always entirely clear.

38. The best known work in this regard is undoubtedly SHEILA JASANOFF, *SCIENCE AT THE BAR: LAW, SCIENCE, AND TECHNOLOGY IN AMERICA* (1995). Another excellent more recent study is TAL GOLAN, *LAWS OF MEN AND LAWS OF NATURE* (2004). The book under discussion in this symposium, is, of course, another contribution to this literature, although in contrast to the other works it is less situated within S&TS than it is an attempt to bridge S&TS and legal literatures. See CAUDILL & LARUE, *supra* note 4.

39. See, e.g., CAUDILL & LARUE, *supra* note 4; Edmond & Mercer, *Conjectures*, *supra* note 17.

40. See generally Haack, *supra* note 17; Adina Schwartz, *A "Dogma of Empiricism" Revisited: Daubert v. Merrell Dow Pharmaceuticals, Inc. and the Need to Resurrect the Philosophical Insight of Frye v. United States*, 10 HARV. J.L. & TECH. 149 (1997).

41. It is no insult for Professors Caudill and Redding to acknowledge that this is perhaps an overstatement. What I mean is "famously" within the small community of scholars who think about scientific evidence and law.

42. Caudill & Redding, *supra* note 17.

43. See Edmond & Mercer, *The Invisible Branch*, *supra* note 36.

It should be clear now, based on the discussion in Part II, that this debate to some extent misses the point. It does not matter whether Popper, or *Daubert*, “got science right” because *Daubert* is not about science. It is about the reliability and admissibility of evidence. The Supreme Court was not really doing philosophy of science, at least not in the sense that Popper was. It was, to be sure, confusing, and perhaps even pretentious, of them to cite philosophy of science, but, nonetheless, it would probably be more true to say they were not doing philosophy of science than to say they were doing it badly.

It should be noted that, at the same time, the best known S&TS legal scholar, Professor Jasanoff, offered a more generous reading of *Daubert*. Jasanoff was perhaps more willing to read the contributions of S&TS between the lines of the *Daubert* text. *Daubert*’s notorious vagueness made it possible for one to, in a sense, make facts on the ground by claiming room for S&TS in the *Daubert* opinion.⁴⁴ Professor Haack’s conclusion is not all that different in that, after amply deconstructing *Daubert*’s philosophy of science, she concludes that its philosophical inconsistency does not matter all that much and should not distract us from more important issues at hand.⁴⁵

In sum, while legal scholars, philosophers of science and S&TS can find much to quibble with, both in *Daubert* and amongst themselves, none of them seems to make a particularly strong argument that these quibbles matter all that much for the central practical matter at hand: the problem of expert evidence. Most scholars, from Professor Jasanoff to Professor Haack, advocate a generally pragmatist approach to this problem.⁴⁶ Professors Caudill and LaRue’s book, *No Magic Wand*, is indeed remarkably pragmatist in orientation.⁴⁷

IV. THE “*DAUBERT* FACTORS”

Another area in which both S&TS and philosophy of science have made contributions is in criticizing the so-called “*Daubert* factors,” the now notorious four-part “checklist” (that the Court warns judges not to treat as a checklist)⁴⁸ which purports to assist courts in determining whether proffered evidence is “reliable.” S&TS scholars and philosophers of science have criticized the factors for being internally inconsistent, overly Popperian and unworkable. My view, as articulated above, is that the argument that the factors constitute poor philosophy of science largely misses the point because the trial judge’s task is not the philosophers’. Yet, this still

44. See JASANOFF, *supra* note 38.

45. See Haack, *supra* note 17.

46. See generally *id.*; Sheila Jasanoff, *Just Evidence: The Limits of Science in the Legal Process*, 34 J.L. MED. & ETHICS 328 (2006).

47. See generally CAUDILL & LARUE, *supra* note 4.

48. See *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 593 (U.S. 1993) (“[W]e do not presume to set out a definitive checklist or test.”).

does not excuse the factors. I think it has become clear with the passage of time that the principal harm of the factors is that they have come to overwhelm the concept they were supposed to elucidate: reliability.⁴⁹ Rather than being read as they were written, as an illustration of one possible way to make a reliability inquiry, the *Daubert* factors have come to be read as a four-pronged test of *admissibility* (not reliability) and have encouraged actors to almost forget about the notion of reliability altogether. That this would occur was perhaps inevitable: the Court was at best naïve if it believed that the *Daubert* factors would not become a four-pronged test. But, in any case, I have come to believe that the Court might have been wiser to state that admissibility required a two-prong test—relevance and reliability—and left it at that.

V. FIT

I have tried to argue that many of the disagreements that characterize *Daubert* scholarship are more apparent than real. In this section, I will focus on one concept that, in my view, unites rather than divides scholars who think about scientific evidence. That concept has been characterized in many different ways, but it was succinctly characterized by the *Daubert* Court as “fit.”⁵⁰ By “fit,” I mean whether the expert testimonial claim is appropriately supported by evidence. The issue is not whether a given expert can muster evidence supporting her ability to make some, legally admissible, testimonial claim, but rather, whether she can muster evidence supporting her ability to make precisely the testimonial claim that she *will* give.

An examination of the *Daubert* literature, both the legal literature and the literature related to S&TS or philosophy of science, shows that a wide variety of scholars who generally would be thought to disagree about many issues share a common view that “fit” is matter of crucial importance. Scholars as diverse as Professor Imwinkelried,⁵¹ Professor Black,⁵² Profes-

49. A quick glance at the legal literature shows that scholars’ use of Justice Rehnquist’s characterization of the four prongs as dicta has decreased as *Daubert* became more familiar and accepted with the passage of time. *See id.* at 600 (Rehnquist, C.J., dissenting).

50. *See id.* at 600; *United States v. Downing*, 753 F.2d 1224, 1242 (3d Cir. 1985).

51. *See* Edward J. Imwinkelried, *The Relativity of Reliability*, 34 SETON HALL L. REV. 269, 277 (2004) (“The degree of allowable definiteness of the expert’s final opinion should vary with the reliability foundation laid by the expert’s proponent.”).

52. *See generally* Bert Black, *Focus on Science, Not Checklists*, 39 TRIAL 26 (Dec. 2003) (“Focus on the specifics of an expert’s opinion, not just the field of expertise.”).

sor Nance,⁵³ Professor Berger,⁵⁴ Professor Beecher-Monas,⁵⁵ Professor Faigman⁵⁶ and Professors Gross and Mnookin⁵⁷ all appear to agree on the crucial importance of fit. Furthermore, it is difficult to find scholars who argue the opposite position—that fit should not matter.⁵⁸

Perhaps the most thought-through view is that of Professor Friedman, who coins the useful term “over-claiming” to refer to instances in which expert witnesses exaggerate the probative value of the knowledge claims.⁵⁹ Professor Friedman notes that the *Daubert* binary admissibility framework is of little use in controlling this problem. Its sole remedy, the “axe-wielding demarcationist”⁶⁰ one of exclusion of evidence, will generally appear to judges to be too draconian a sanction for a little over-claiming, especially when they have already, in an admissibility ruling, committed themselves to the position that the evidence is reliable and therefore admissible. Moreover, Professor Friedman usefully considers the logical

53. See Nance, *supra* note 11, at 253. Nance notes that:

The use of a dichotomous concept of reliability, whether or not founded on deference to a non-legal discipline, simply poses too many problems. These problems, in turn, encourage an epistemically invasive use of admissibility to monitor the weight of the evidence. It is time to turn away from that kind of thinking and try another tack, one that draws on more manageable comparative reliability assessments that build on a gradational concept of reliability.

Id.

54. See Margaret A. Berger, *Expert Testimony in Criminal Proceedings: Questions Daubert Does Not Answer*, 33 SETON HALL L. REV. 1125, 1140 (2003) (“Admissibility and sufficiency determinations rest on more than satisfaction of a reliability component; they require careful attention to what the evidence proves and how the trier of fact will use it.”).

55. See Erica Beecher-Monas, *A Ray of Light for Judges Blinded by Science: Triers of Science and Intellectual Due Process*, 33 GA. L. REV. 1047, 1062 (1999). (“[S]cientific reliability is not an all-or-nothing proposition, but rather depends on the application of the evidence and the acceptable risk of error.”).

56. See David L. Faigman, *Expert Evidence in Flatland: The Geometry of a World Without Scientific Culture*, 34 SETON HALL L. REV. 255, 258 (2004) (“[D]isclaim[ing] any intention to transform reliability into a dichotomous variable. . .”).

57. See generally Samuel Gross & Jennifer L. Mnookin, *Expert Information and Expert Evidence: A Preliminary Taxonomy*, 34 SETON HALL L. REV. 141 (2004) (“As important as an examination of method, however, and much less noted, is another dimension: the degree of certainty that the expert posits in what she offers.”).

58. Professor Kaye and co-authors argue persuasively that “fit” is generally implied in any appropriate deployment of the notion of validity. They concede, however, that “*Daubert*’s reference to ‘fit’ serves as an important reminder that the ‘scientific validity’ of a technique or instrument depends on the use to which it is put.” DAVID H. KAYE ET AL., *THE NEW WIGMORE: EXPERT EVIDENCE*, §6.3.1 (2004).

59. See Friedman, *supra* note 7. Friedman explains:

Perhaps, though the problem is that the expert witness over-claimed the significance of the serological result. If that is the problem, then the solution would not be to exclude evidence of the underlying serological phenomenon, but to try to prevent the over-claiming, or at least to ensure that the jury is not misled by it. It does not seem to me that the *Daubert* framework is of much help here.

60. Haack, *supra* note 11, at 248.

extent of the emphasis on fit by taking the counterintuitive position that evidence that is wrong more often than it is right can nonetheless have probative value (and, therefore, should be admissible), provided only that the fact finder is properly informed of the evidence's reliability.⁶¹

Despite all these pronouncements, the paeans to "fit" are almost homilies; scholars do not say much about fit, other than that they are in favor of it. This is perhaps not surprising as a consequence of the issues discussed earlier; most scholars view admissibility demarcation as homologous with the demarcation of science from pseudo-science and, therefore, focus on demarcation rather than assessment of fit. What I want to propose here is a shift of focus away from the overemphasized issue of demarcation and toward the neglected issue of fit. In some sense, I view "fit" as another overlooked gem in the *Daubert* Trilogy, much as Professors Denbeaux and Risinger have sought to elevate *Kumho Tire's* "task at hand" requirement to a central place in the evaluation of expert evidence.⁶²

There is, of course, some resemblance between "fit" and the "task at hand." Both concern an appropriate relationship between the evidentiary claim and what Professors Denbeaux and Risinger usefully call the "warrants" for that claim. However, Professors Denbeaux and Risinger's focus is primarily on the validity of the procedures that produce evidence. My focus here is further downstream; it is on how that evidence is used in the courtroom. Even valid procedures can produce invalid testimony, if the testimony does not fit the warrants that support that testimony. Much scientific evidence scholarship, including my own, has focused on the need to validate forms of evidence that are used in court.⁶³ I am becoming convinced, however, that it is in testimony that "the rubber meets to road" for the problem of the expert in law. The problem with experts for law is not so much what the evidence says, but what the expert says to the fact finder.

Hitherto, scholars and courts have largely focused on the validity of evidence, seen in a dichotomous admissibility framework. Valid evidence is admissible, invalid evidence inadmissible. What happens after admissibility has largely been given short shrift, and experts are permitted to say

61. See Friedman, *supra* note 7. Professor Friedman acknowledges that this argument also requires the assumption that it can shown or assumed that the reliability of the evidence can be properly conveyed to the fact-finder and that the fact-finder can appropriately integrate it with other evidence. As a factual matter, of course, both of these are highly suspect assumptions, as Professor Friedman acknowledges.

62. See generally Denbeaux & Risinger, *supra* note 5; Imwinkelried, *supra* note 26.

63. See DAVID L. FAIGMAN ET AL., MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (2d ed. 2002); Edward J. Imwinkelried, *Coming to Grips with Scientific Research in Daubert's "Brave New World": The Courts' Need to Appreciate the Evidentiary Differences between Validity and Proficiency Studies*, 61 BROOK. L. REV. 1247 (1995); Imwinkelried, *The Meaning of "Appropriate Validation"*, *supra* note 25; Simon A. Cole, *Is Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents' Discourse*, 28 J.L. & POL'Y 109 (2006).

more or less anything about evidence deemed admissible, even if what they say overstates (or understates) the probative value of the evidence. I suggest that courts and scholars need to spend a lot more time thinking about expert testimony and perhaps a little less time thinking about scientific evidence.

VI. BRINGING THE STRANDS TOGETHER

What I want to argue, in sum, is that courts (and scholars) should not be demarcating at all, but rather assessing fit. I would hypothesize that more damage is done by “over-claiming”—by expert testimony that exaggerates its own probative value—than by evidence that is deemed admissible when it should not be. Gone would be the binary distinction between admissible and inadmissible, reliable and unreliable evidence. In its stead would be an evaluation of where particular evidentiary claims stand along a continuum of trustworthiness. Most crucially, that evaluation would control not whether the evidence is heard, as in a *Daubert*-like binary admissibility framework, but rather the nature of the testimonial claim that is heard. In other words, judges would carefully calibrate the strength of the testimonial claim to the strength of the warrants that support that claim. My claim is that this is precisely what is missed in a binary admissibility framework in which judges assume that their work is done once they have ruled proffered evidence admissible or inadmissible. I suggest that in this framework, weak evidence that is ruled admissible probably routinely over-claims. Meanwhile, as Professor Friedman notes, fact finders may be deprived of weak evidence that nonetheless might have probative value as long as it is restricted from over-claiming.

VII. FORENSIC EVIDENCE: A CASE STUDY IN OVER-CLAIMING

In this section, I will report on a provisional attempt to apply this over-claiming framework to a problematic area of scientific evidence: forensic science or forensic evidence. The choice of forensic evidence does not merely reflect the fact that it is the area of scientific evidence with which I am most familiar. Over-claiming has already been identified as a particular problem with forensic evidence by a number of scholars (though they do not necessarily use that term). Over-claiming has long been identified as a problem for microscopic hair comparison.⁶⁴ The National Research Council’s (NRC) report on Comparative Bullet Lead Analysis (CBLA) revealed rampant over-claiming over the course of the nearly

64. See Berger, *supra* note 54, at 1132 (“We know from cases and news accounts that hair examiners often embellish their testimony with probability estimates based on their personal experience for which there is no empirical basis.”); Friedman, *supra* note 7, at 1063; see also generally Peter Neufeld, *The (Near) Irrelevance of Daubert to Criminal Justice and Some Suggestions for Reform*, 95 AM. J. PUB. HEALTH S107 (2005); Clive A. Stafford Smith & Patrick D. Goodman, *Forensic Hair Comparison Analysis: Nineteenth Century Science or Twentieth Century Snake Oil?*, 27 COLUM. HUM. RTS. L. REV. 227 (1996).

forty-year history of FBI testimony concerning results of that assay, including the completely unfounded claim that two analytically indistinguishable bullets “must have come from the same box or from another box that would have been made by the same company on the same day.”⁶⁵ Indeed, much of the scholarly attention devoted to CBLA and the NRC report has focused on the very interesting statistical issues raised by CBLA evidence and the FBI’s and NRC’s alleged failure to deal with them adequately.⁶⁶ Nevertheless, in some sense these sexy statistical issues⁶⁷ have induced many scholars to miss the forest for the trees. Setting the issues of statistical inference aside, the CBLA episode can be read as a simple story of over-claiming: the testimonial use of claims well beyond what empirical evidence did, or even could, support. And, indeed, it is in the *testimony* where the damage was done. The NRC report documents sworn government witnesses testifying that a bullet recovered from a crime scene “must have come from the same box” as a bullet found in the defendant’s possession “or from another box that would have been made by the same company on the same day,” a claim that is unsustainable no matter what one thinks about the value of CBLA.⁶⁸

Latent print identification is perhaps the only discipline for which over-claiming is *institutionalized*. Because professional guidelines mandate that the *only* inclusionary conclusion latent print examiners may offer in their sworn testimony is that of “individualization” (the claim that the defendant or suspect’s skin is the source of the latent or crime-scene print to the exclusion of all other possible sources in the universe),⁶⁹ and such

65. National Research Council, *Weighing Bullet Lead Evidence*, Report in Brief (2004).

66. See generally Michael O. Finkelstein & Bruce Levin, *Compositional Analysis of Bullet Lead as Forensic Evidence*, 13 J.L. & POL’Y 119 (2005); Edward J. Imwinkelried & William A. Tobin, *Comparative Bullet Lead Analysis (CBLA) Evidence: Valid Inference or Ipse Dixit?*, 28 OKLA. CITY U. L. REV. 43 (2003); David H. Kaye, *Bullet-proofing the NRC Bullet Lead Report: Should Science Committees Make Legal Findings?*, 46 JURIMETRICS J. 91 (2005); William C. Thompson, *Analyzing the Relevance and Admissibility of Bullet Lead Evidence: Did the NRC Report Miss the Target?*, 46 JURIMETRICS J. 65 (2005); see also Alicia L. Carriquiry, *Good Riddance: Further Arguing Against CABL as a Forensic Tool*, 19 CHANCE (2006); Clifford Spiegelman & Karen Kafadar, *Data Methods, Materials, and Scientific Responsibility*, 19 CHANCE (2006); Simon A. Cole et al., *A Retail Sampling Approach to Assess Impact of Geographic Concentrations on Probative Value of Comparative Bullet Lead Analysis*, 4 LAW, PROBABILITY AND RISK 199 (2005).

67. If such a concept is even possible.

68. National Research Council, *Weighing Bullet Lead Evidence*, Report in Brief (2004).

69. See Scientific Working Group on Friction Ridge Analysis Study and Technology, *Standards for Conclusions*, J. Forensic Identification (2004), available at http://www.swgfast.org/Standards_for_Conclusions_ver_1_0.pdf; Scientific Working Group on Friction Ridge Analysis Study and Technology, *Friction Ridge Examination Methodology for Latent Print Examiners* (2002), available at http://www.swgfast.org/Friction_Ridge_Examination_Methodology_for_Latent_Print_Examiners_1_01.pdf.

claims are obviously unsustainable, *all* inclusionary latent print testimony is over-claimed.⁷⁰

Whether latent print evidence is the only forensic discipline that enjoys institutionalized over-claiming is currently the subject of a vociferous debate among students of tool mark analysis.⁷¹ It is clear that tool mark examiners sometimes testify that the correspondence between a crime scene tool mark and a test tool mark somehow warrants them to testify the object that made the test tool mark *is* the object that made the crime scene mark. Further, they claim that all other tools in the universe—even the next consecutive tool produced on the assembly line—can be eliminated as potential sources of the crime scene mark. Courts have noted that this is an “extraordinary” claim in its epistemological strength and probative value, and they have noted that it is particularly extraordinary given the paucity of evidence supporting it.⁷² The ultimate resolution of this debate, which concerns precise interpretations of the wording of the Association of Firearms and Toolmark Examiners’ “Theory of Identification,” is less important for our purposes than the mere fact that the issue remains ambiguous. The very fact that there is debate over the nature of properly framed tool mark testimony illustrates not only that another professional group may be engaging in systematic over-claiming in our courts, but also the casualness with which courts, scholars and practitioners approach matters of testimonial nuance in their eagerness to focus on *Daubert*’s dichotomous admissible/inadmissible distinction.

Forensic odontologists have also been accused of over-claiming. In one case, the witness testified, not only that a crime scene bite mark was consistent with the defendant’s dentition, but also that no other dentition among the 3.5 million inhabitants of the Detroit metropolitan area would also be found consistent with this particular bite mark.⁷³ As the Sixth Cir-

70. For further discussion, see William C. Thompson & Simon A. Cole, *Psychological Aspects of Forensic Identification Evidence*, in *PSYCHOLOGICAL TESTIMONY FOR THE COURTS* (Costanzo et al. eds., 2006); Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Ruling from Jennings to Llera Plaza and Back Again*, 41 *AM. CRIM. L. REV.* 1189, 1223 (2004). It is curious that Professor Friedman, from whom I derive this notion of over-claiming, failed to recognize the dangers of institutionalized over-claiming in latent print identification when he claimed: “If the treatment of fingerprint evidence does not square with *Daubert*, as some . . . have suggested . . . I believe the problem lies with *Daubert* rather than with the way fingerprint evidence has been treated for decades.” Friedman, *supra* note 7, at n.26. As for Professor Friedman’s claim that “[f]ingerprint identification . . . seems to be a trivial source of mistaken identification,” based on Innocence Project data, that issue is treated extensively in Cole. See Cole, *supra* note 15, at 1025.

71. See generally Adina Schwartz, *A Systemic Challenge to the Reliability and Admissibility of Firearms and Toolmark Identification*, 6 *COLUM. SCI. & TECH. L. REV.* 1 (2005); Ronald Nichols, *The Scientific Foundation of Firearms and Tool Mark Identification—A Response to Recent Challenges*, California Ass’n of Criminalists News 8 (2nd Quarter, 2006).

72. *United States v. Green*, 405 F.Supp.2d 104, 107 (D. Mass. 2005); see also *Ramirez v. State*, 810 So. 2d 836 (Fla. 2001).

73. See *Ege v. Yukins*, 485 F.3d 364 (2007).

cuit Court of Appeals, and the state court before it, noted, this testimony exaggerated the probative value of the evidence not merely because the witness had not actually observed the dentition of those 3.5 million individuals, but also because the witness lacked any data from which to even make responsible estimate of the rarity of the observed features of the bite mark in a population of that size.⁷⁴

Indeed, some scholars have noted that the *weakest* areas of forensic science appear to be the most prone to over-claiming, something they view as “ironic” that might equally be viewed as entirely unsurprising and perhaps even as a predictable outcome of the *Daubert* framework.⁷⁵ And yet, even a discipline with sufficient data to support very precisely formulated testimonial claims and forensic DNA profiling is not immune to over-claiming. For several years now, forensic scientists have debated the propriety of rounding very low random match probabilities into testimonial claims of “source attribution”—that the defendant “is” the source of the crime-scene DNA sample.⁷⁶

A. *A Preliminary Empirical Study of Over-claiming*

The evidence concerning over-claiming in the previous section is generally anecdotal or indirect. Arguments concerning over-claiming are typically supported by reference to notorious cases, and such cases may not necessarily be common or representative. Another method of documenting over-claiming is to examine policy statements, promulgated standards or other documents generated by professional organizations or other standard-setting bodies. My argument concerning institutionalized over-claiming by latent print examiners and perhaps tool mark examiners can be supported by reference to such documents.⁷⁷

74. *Id.*

75. See John I. Thornton & Joseph L. Peterson, *The General Assumptions and Rationale of Forensic Identification*, in SCIENCE IN THE LAW: FORENSIC SCIENCE ISSUES 1, 25 (Faigman, et al. eds., 2002) (“It is ironic that those areas of forensic science that have real underlying data offer more modest statements of individualization, while those limited to subjective or impressionistic data make the strongest statements, sometimes of absolute certainty.”).

76. See generally Bruce Budowle, *Source Attribution of a Forensic DNA Profile*, 2 FORENSIC SCIENCE COMMUNICATIONS (2000); John Buckleton, *Population Genetic Models*, in FORENSIC DNA EVIDENCE INTERPRETATION 65 (Buckleton et al. eds., 2005); Bruce S. Weir, *DNA Match and Profile Probabilities: Comment on Budowle et al.* (2000); Fund and Hu (2000), 3 FORENSIC SCIENCE COMMUNICATIONS (2001).

77. See Nichols, *supra* note 71; Scientific Working Group on Friction Ridge Analysis Study and Technology, *Standards for Conclusions* (2003). Among latent print examiners, I can also show that institutionalized over-claiming does not just exist at the center of the latent print community; the principle has trickled down to the local level. For example, the New Hampshire State Police Laboratory latent print protocol states that testimony should be given as follows: “The latent impression developed on exhibit ____ has been identified as the fingerprint impression of _____.” New Hampshire State Police Forensic Laboratory, *Latent Print Examination: Interpretation and Reporting of Conclusions*, IDU-001-03 (2005). The report form of another laboratory contained the preprinted words: “After examination of

There are, of course, good reasons for using such documents as indicators of the nature of testimonial claims used by the witnesses who belong to the relevant disciplines and/or organizations. Although the standards promulgated by these bodies are not typically “binding” in any strict sense, they presumably do have some persuasive authority with those expert witnesses who either belong to the relevant organizations or consider themselves members of the relevant disciplines.⁷⁸

American trial practice, however, is a vast and disparate body of activity that is incompletely and sporadically reported. Therefore, there is little way of knowing to what extent the pronouncements of professional organizations and self-appointed standard-setting bodies constrain the testimony of actual expert witnesses in American courtrooms. And, this, after all, is the heart of the matter: what witnesses say to juries every day all over the country clad in the mantle of expertise. Once again, in my view, the essence of *forensic* science is not merely what is done but, at least as importantly, what is said.

Despite the importance of the issue, determining what expert witnesses say in American courtrooms is far from an easy matter. No organization systematically, or even unsystematically, collects such data. The law’s historical convention of treating appellate opinions, rather than trial transcripts, as the site at which law “happens” has led to a situation in which—the far from minor manner of publication, unpublication and depublication aside⁷⁹—transparent, fully searchable records are kept of appellate decisions, but almost no searchable databases of trial transcripts exist. It is true that some proprietary trial transcript databases do exist, and it so happens that some of these focus on scientific evidence. But these proprietary databases tend to focus on the scientific evidence that is used in civil, not criminal, litigation.⁸⁰

this evidence the latent prints obtained from _____ and the _____ were made by one and the same person.” Los Angeles Police Department, Scientific Investigation Division, Latent Print Section, *Forensic Print Comparison Report* (1990), on file with the author.

78. Some guidelines may have even more authority than this. For example, resolutions of the International Association for Identification echo the point made by SWGFAST above, banning conclusions less than “individualization.” International Association for Identification, *Resolution V*, 30 IDENTIFICATION NEWS 3 (1980); International Association for Identification, *Resolution VII*, 29 IDENTIFICATION NEWS 1 (1979). The IAI’s somewhat different language, banning “probabilistic” identifications, essentially amounts to the same thing, but it also carries with it an implicit threat of professional sanction, such as expulsion from the IAI. Although the IAI has expelled members for other reasons, I am not aware of any case of expelling a member for expressing a probabilistic conclusion, even when such a conclusion has been reported in a published legal opinion. See *Michigan v. Ballard*, 2003 Mich. App. LEXIS 547 (Mich. Ct. App. Feb. 28, 2003); see also *Latent Print Certification Actions*, 55 J. FORENSIC IDENTIFICATION 658 (2005) (explaining reasons why other IAI members were expelled).

79. See, e.g., Penelope Pether, *Inequitable Injunctions: The Scandal of Private Judging in the U.S. Courts*, 56 STAN. L. REV. 1435 (2004).

80. See, e.g., Daubert Tracker, <http://www.dauberttracker.com>.

In order to gain a more precise understanding of the semantic nature of forensic expert testimony, I have begun collecting trial transcripts of criminal cases in which forensic expert evidence is presented. I began with latent print evidence, the area with which I have the most familiarity, but I am actively collecting transcripts in other areas as well. The analysis presented here derives from a set of thirty-four latent print transcripts, which comprise a non-representative opportunistic sample. I acquired some transcripts through my work as a scholar interested in latent print evidence, as a consultant to criminal defendants in cases involving latent print evidence, and as an expert witness on the validity or lack thereof of latent print evidence in such cases. In addition I also received some unsolicited transcripts directly from convicted inmates who were aware of my work on latent print evidence. Fifteen of the thirty-four transcripts were gathered in this manner. I acquired additional transcripts by soliciting transcripts on defense attorney listservs and by directly emailing attorneys of my acquaintance who were interested in forensic evidence issues. Seventeen of the thirty-four transcripts were gathered by this method. Finally, two trial transcripts involving fingerprint evidence from high profile trials—*United States v. McVeigh* and *United States v. Nichols*—were publicly available on the internet (*see* Appendix for details on the transcripts).

Any form of spoken, sworn testimony concerning an inclusionary conclusion drawn from latent print evidence was considered suitable for inclusion. The intent was to understand what probative conclusions expert witnesses uttered in sworn testimony. Thus, the transcripts derive from trials, preliminary hearings and depositions. Affidavits, such as the notorious “100% identification” affidavit used to wrongly arrest attorney Brandon Mayfield in the Madrid bombing,⁸¹ were not included. The thirty-four transcripts derive from twelve states and from federal court. One transcript derives from a foreign source, the state of Western Australia. The transcripts derive from criminal cases involving a variety of charges, ranging from burglary to mass murder (e.g., *United States v. McVeigh*), from obscure to extremely high-profile cases (*McVeigh* again). The dates the transcripts were generated range from 1986 to 2006. The mean year is 2000.

Obviously, the transcripts analyzed here derive from an opportunistic sample, and no claims can be made concerning their representativeness. Methods of gathering a more representative sample can be imagined, but they would be quite unwieldy.⁸² The important question, however, is

81. For further discussion, see Cole, *supra* note 15; Simon A. Cole, *Brandon Mayfield, Suspect*, in *SUSPECT* 172 (Knechtel ed., 2005); Simon A. Cole, *The Prevalence and Potential Causes of Wrongful Conviction by Fingerprint*, 37 *GOLDEN GATE U. L. REV.* 39 (2006); William C. Thompson & Simon A. Cole, *Lessons from the Brandon Mayfield Case*, 29 *THE CHAMPION* 42 (Apr. 2005); Steven T. Wax & Christopher J. Schatz, *A Multitude of Errors: The Brandon Mayfield Case*, 28 *THE CHAMPION* 6 (Sept./Oct., 2004).

82. It would be necessary to assemble a set of representative jurisdictions, define a representative time period, and examine all criminal transcripts for the pres-

whether the transcripts analyzed here are likely to overstate or understate the quality of latent print testimony as it exists in the universe *not* captured by these transcripts. Because my argument is generally critical of the current testimonial practice concerning latent print evidence—in that I claim there is routine, institutionalized over-claiming—we need only worry about the possibility that this data understates the quality of testimony. If this data overstates the quality of testimony, and if I have erred, I have erred in favor of my adversary.

The “quality” of expert testimony is not an obviously measurable thing. Essentially, what we would be concerned about is that the testimony found in this data set is somehow “worse”—less responsible, less defensible—than the typical testimony given during the comparable time period in comparable jurisdictions. There are several reasons to conclude that this data set is, if anything, likely to overstate the “quality,” however that is measured, of latent print expert testimony. First, the transcripts are relatively recent in origin. The mean year of generation is 2000. Latent print testimony has been offered in U.S. courts for almost a century.⁸³ Although defendants challenged the evidence when it was first introduced, by the 1920s the admissibility and presumed reliability were widely sanctioned by the courts.⁸⁴ Over the course of the twentieth century, latent print examiners have remarked on defense attorneys’ reluctance to challenge evidence.⁸⁵ In 1999, however, a criminal defendant challenged the admissibility of latent print expert evidence under *Daubert v. Merrell Dow Pharmaceuticals, Inc.*⁸⁶ This challenge stimulated more admissibility challenges, and these provoked a fair amount of notice within the latent print community⁸⁷ and some consternation about whether the technique might be ruled inadmissible under *Daubert*.⁸⁸ There was even a brief scare when one court did limit the admissibility of latent print evidence before reversing itself.⁸⁹ Thus, if ever within nearly a century of expert testimony, la-

ence of testimony concerning latent print evidence. The “yield” of such a method would probably be quite low and thus require examining a large number of transcripts. This would be quite laborious given that searchable databases of trial transcripts are rare.

83. See *People v. Jennings*, 96 N.E. 1077 (Ill. 1911).

84. See SIMON A. COLE, *SUSPECT IDENTITIES: A HISTORY OF FINGERPRINTING AND CRIMINAL IDENTIFICATION* 206 (2001).

85. See 6 *FINGER PRINT AND IDENTIFICATION MAGAZINE* 2 (1924); Andre A. Moenssens, *Testifying as a Fingerprint Witness*, 54 *FINGER PRINT AND IDENTIFICATION MAGAZINE* 3 (1972).

86. See generally *United States v. Mitchell*, 365 F.3d 215 (3d Cir. 2004); Robert Epstein, *Fingerprints Meet Daubert: The Myth of Fingerprint “Science” is Revealed*, 75 S. CAL. L. REV. 605 (2002).

87. See, e.g., David L. Grieve, *Rocking the Cradle*, 49 J. FORENSIC IDENTIFICATION 719 (1999).

88. See, e.g., John P. Nielson, *Are You Dead? Take this Test and Find Out*, 53 J. FORENSIC IDENTIFICATION 1 (2003).

89. See *United States v. Llera Plaza*, 188 F. Supp. 2d 549 (E.D. Pa. 2002); *United States v. Llera Plaza*, 179 F. Supp. 2d 492 (E.D. Pa. 2002).

tent print examiners were going to be careful about how they phrased their conclusions, it would have been after 1999. Because half of the transcripts date from 2000 or after, we should expect them to represent relatively carefully phrased testimony.

Second, the transcripts derive from relatively serious crimes. Of the eighteen transcripts for which the charge can be determined, half derive from homicide cases. Homicide cases represent around one percent of felony cases prosecuted nationwide.⁹⁰ Thus, the data set oversamples serious cases. All other things being equal, the quality of both legal representation and expert testimony is likely to be higher the more serious the case. Similarly, with two of the thirty-four cases representing very high-profile cases, the data set surely oversamples high-profile cases. Again, all other things being equal, the quality of both representation and testimony should be relatively high in high-profile cases. Although some forensic scientists have claimed that latent print analysis is more error-prone in high-profile cases,⁹¹ this claim has not been extended to sloppy phrasing of testimony.⁹² Finally, with six out of thirty-four cases, the federal jurisdiction is also surely overrepresented in the data set. Again, all other things being equal, the quality of both representation and expert testimony may be expected to be generally higher in the federal jurisdiction. This is especially true in the area of forensic evidence, where the expert witnesses very likely come from such vaunted organizations as the FBI, the Secret Service, the ATF, the Postal Police, Homeland Security and the IRS.

Third, the level of experience of the examiners testifying was not remarkably low. Of the cases in which the level of the examiner's experience was stated in the transcript, the average number of years of experience was fifteen. The latent print community generally uses experience as a rough measure of ability,⁹³ given the absence of any other measure (other than certification), although the facile equation of ability with

90. See Bureau of Justice Statistics, U.S. Dep't of Justice, Criminal Case Processing Statistics, available at <http://www.ojp.usdoj.gov/bjs/cases.htm> (last updated Sept. 28, 2004).

91. See generally Robert B. Stacey, *A Report on the Erroneous Fingerprint Individualization in the Madrid Train Bombing Case*, 54 J. FORENSIC IDENTIFICATION 706 (2004).

92. Moreover, the claim is unsupported. See generally Cole, *supra* note 81; Thompson & Cole, *supra* note 81.

93. Jon S. Byrd, *Confirmation Bias, Ethics, and Mistakes in Forensics*, 56 J. FORENSIC IDENTIFICATION 511, 520 (2006) ("[T]he training and experience of the examiner becomes the vital element in the identification process.") (emphasis added); Pat A. Wertheim, *The Connection: Faulty Forensics* (NPR radio broadcast, June 10, 2004), available at http://www.theconnection.org/shows/2004/06/20040610_b_main.asp (questionably interpreting *Llera Plaza II* as holding, "[t]he reliability of the examiner through training, experience, and testing is the key to the reliability of the evidence they [sic] present in court."); see also *Llera Plaza*, 188 F.Supp. 2d at 549. One of the ways in which this is manifested is through the frequent reference to "training and experience" as warrant for the expert's claim.

experience has been questioned by some.⁹⁴ In general, however, it does not seem plausible to claim that the examiners represented in this data set under-represent the level of experience of latent print examiners testifying in the U.S. in recent years.

Finally, the method of collection itself is a rough indicator of generally high quality of justice. First, the trial transcript is in the possession of an attorney, meaning that someone is appealing the conviction. Second, the possessor of that transcript either: (1) subscribes to a listserv, itself an indicator of a relatively engaged attorney, and took the opportunity to respond to an academic researcher's request for information without any prospect of compensation; or (2) engaged the services of the author as a consultant, indicating a relatively high level of engagement in the litigation. These may be considered rough measures of a case that is enjoying a reasonably high level of representation, not cases from the backwoods⁹⁵ using latent print examiners out of touch with current practice. For all of these reasons, it would seem safe to conclude that the transcripts do not represent atypically "poor" expert testimony.

In analyzing the transcripts, I focused on what I call the "source attribution moment," the moment at which the expert connected the latent print to the defendant or the moment at which the expert identified the defendant as the source of the latent print. This is the moment when the latent print expert does his or her work: telling the jury that the defendant is the source of an incriminating print. Generally speaking, this moment should occur on direct examination, and for consistency purposes, I only analyzed direct testimony even though cross examination yielded some highly incriminatory statements in some cases.

Using a process of open coding,⁹⁶ I read through the "source attribution moments" in several iterations to allow the data to suggest categories, rather than imposing my own categories upon the data. This process yielded three broad categories of testimony capable of encompassing all thirty-four testimonies. A few testimonies fit in more than one category. Note, however, that two categories would have been sufficient to encompass all but three of the testimonies. The third category ("Identity") should thereby be regarded as less significant than the other two.

B. *About Identification Testimony*

Latent print (or "fingerprint") evidence is perhaps best characterized as "source attribution" evidence.⁹⁷ The probative value of such evidence typically lies in the identification of a particular individual as the source of

94. See Pat A. Wertheim, *The Ability Equation*, 46 J. FORENSIC IDENTIFICATION 149 (1996).

95. Or, say, Philadelphia.

96. See ANSELM L. STRAUSS, *QUALITATIVE ANALYSIS FOR SOCIAL SCIENTISTS* (1987).

97. See KEITH INMAN & NORAH RUDIN, *PRINCIPLES AND PRACTICE OF CRIMINALISTICS: THE PROFESSION OF FORENSIC SCIENCE* 137 (2001); WILLIAM C. THOMPSON &

trace evidence found at a crime scene. This is done through a finding of consistency between that crime scene trace and a reference sample known to come from that individual (typically because it was taken from the individual while held in custody). For the past century or so, such evidence has been offered in court for such forms of trace evidence as fingerprints, bite marks, serology, hair and handwriting.⁹⁸ During this past century, there appears to have been relatively little systematic thought about how testimony concerning such evidence should be given. There does not appear to have been any organized control over forensic expert testimony. Nor did the courts appear to exercise much control, save the occasional sanction when a witness went too far, such as by straying into the province of the jury.⁹⁹ Forensic expert witnesses testified in a variety of formulations, most typically stating that the crime scene trace “matched” the defendant.

This situation changed with the introduction of forensic DNA profiling. As DNA typing developed, expert testimony evolved into what has been called a “two-stage” form of testimony.¹⁰⁰ The first stage was a statement of consistency. Presumably such statements should have also included the criteria by which traces were judged on consistency, an accounting of any inconsistencies, an explanation of why it was still permissible to find the traces consistent and some measure of the “amount” of consistency. The second was an estimate of rarity. Such statements sought to convey the significance, or the probative value, of the finding of consistency to the fact-finder by estimating the rarity of the features found consistent in stage (1) in a given population. To use an obvious example, an eyewitness sighting of a perpetrator fleeing in a Rolls Royce has a higher probative value than the sighting of a perpetrator fleeing in a Toyota because Rolls Royces are more rare. This general framework for presenting forensic evidence to the fact finder commanded widespread agreement and was endorsed by two NRC panels,¹⁰¹ although there were, and still are, fierce disputes over the details.¹⁰²

SIMON A. COLE, *Psychological Aspects of Forensic Identification Evidence*, in *PSYCHOLOGICAL TESTIMONY FOR THE COURTS* (Costanzo et al. eds., 2006).

98. For an overview, see generally *MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY* (David L. Faigman et al. eds., 2d ed. 2002); see also, Clive A. Stafford Smith & Patrick D. Goodman, *Forensic Hair Comparison Analysis: Nineteenth Century Science or Twentieth Century Snake Oil?*, 27 *COLUM. HUM. RTS. L. REV.* 227 (1996) (analyzing hair evidence).

99. See, e.g., *Stacy v. State*, 292 P. 885 (Ok. 1930).

100. See Colin Aitken, *Presentation at the Sackler Colloquium on Forensic Science: The Nexus of Science and the Law* (Nat'l Acad. of Sciences, Nov. 17, 2005), available at <http://progressive.playstream.com/nakfi/progressive/sackler/forensics/aitken/aitken.html>.

101. See NAT'L RESEARCH COUNCIL, *THE EVALUATION OF FORENSIC DNA EVIDENCE* (Nat'l Acad. Press 1996).

102. See JAY ARONSON, *GENETIC WITNESS: SCIENCE, LAW AND CONTROVERSY IN THE DEVELOPMENT OF DNA PROFILING*; see also MICHAEL LYNCH ET AL., *TRUTH MA-*

The dominance of the two-step model has led to a climate in which it has become difficult to understand why anyone would want to present source attribution evidence in any other way. Nonetheless, those disciplines that historically predate the development of forensic DNA profiling remain committed to their traditional methods of characterizing evidence. Primarily, this is because they lack the data with which to calculate the estimates necessary to complete the second step.¹⁰³ Even so, the implicit contrast with DNA profiling has led to some discomfort with the match language in the traditional source attributions disciplines. Bite mark examiners have been the most proactive in this regard, seeking to develop a sort of linguistic scale of certainty along which judgments of consistency could be calibrated.¹⁰⁴ The obvious question prompted when disciplines that testify about “matches” are contrasted with forensic DNA profiling is: what is meant by a “match”? Is a “match” an assertion that the reference sample is consistent with trace, *and nothing else could be consistent*? Or, is it an assertion that the reference sample is consistent, and some other number of objects in the world might also be consistent? Typically, this ambiguity inherent in the word “match” is not resolved in expert testimony (or even necessarily in the witnesses’ mind).¹⁰⁵ Of course, as evidence scholars have noted, testimony about “matches” is unhelpful to fact-finders who are unable to assess the probative value of the evidence without knowing the rarity of the “match.”¹⁰⁶

A statement such as “the latent print is not inconsistent with the known print of the suspect” is, in my view, a scientifically defensible statement to make about a latent print analysis. It is not false on its face; it does not imply the existence of studies, data and conclusions that do not exist (as does the statement, “[t]he latent print is consistent with the known print of the suspect, and no other known print could be found consistent”). Nevertheless, even this more scientifically defensible statement serves the fact-finder poorly. The fact-finder still does not know how many individuals in the relevant population can produce known prints that would be deemed “not inconsistent” with the crime scene trace and, therefore, cannot calculate the probative value of the evidence. The prob-

CHINE: THE CONTENTIOUS HISTORY OF DNA FINGERPRINTING (Univ. of Chic. Press, forthcoming).

103. For a discussion on latent prints, see generally CHRISTOPHE CHAMPOD ET AL., *FINGERPRINTS AND OTHER RIDGE SKIN IMPRESSIONS* (2004); Christophe Champod & Ian W. Evett, *A Probabilistic Approach to Fingerprint Evidence*, 51 J. FORENSIC IDENTIFICATION 101 (2001).

104. See C. Michael Bowers, *Identification from Bitemarks: Scientific Issues*, in SCIENCE IN THE LAW: FORENSIC SCIENCE ISSUES 244 (Faigman et al. eds., 2002).

105. See INMAN & RUDIN, *supra* note 97.

106. See generally Jennifer L. Mnookin, *Fingerprint Evidence In An Age of DNA Profiling*, 67 BROOK. L. REV. 13 (2001); Judith A. McKenna et al., *Reference Guide on Forensic DNA Evidence*, in REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 273, 297 (1st ed. 1994).

lem with the evidence under *Daubert* thus becomes not its reliability, but its relevance.¹⁰⁷

These issues notwithstanding, given that latent print examiners have no data or methods with which to assess the rarity of the features that they find consistent in their analyses, in my view the only defensible testimony latent print examiners could give would be statements like the following:

“I was not able to exclude the defendant as the source of the latent print.”

“It is my opinion that the latent print and the known print may have derived from a common source.”

“Using a process whose accuracy is not known, I reached a conclusion that the latent print and the known print derived from a common source.”

“The consistencies between the latent print and the known print show that potential donors of the latent print are the defendant and an unknown number of other individuals.”

These are not the sorts of statements I found in my analyses of actual transcripts. The most carefully and precisely worded conclusion was without question the following one:

“The latent print that appears on this lift, which is Government’s Exhibit 44, was made by the same individual whose inked fingerprint appears in the left ring finger block of the fingerprint card, Government Exhibit Number 60, which bears the name Latrell Lanthrom Gilchrist”.¹⁰⁸

C. *Categories of Testimony*

1. *Process Statements*

Eighteen testimonies fit into a category I will call “Process statements” because the witness characterized the evidence not in terms of probative value, but as the outcome of process. Typically, the witnesses have told the jury that they “identified” or “matched” the latent print “to” the defendant. An example of such a “Process statement” is the following: “This impression here, No. 3 upper, right-hand corner, No. 18, was identified to the ink impression of Victor Reyes’ left palm in this area here.”¹⁰⁹

In some ways, the “Process” category belies much of the discussion we have had up to this point. Process statements are not really source attribution moments in that the expert never actually states that defendant is the source of the crime scene trace. Nor do Process statements misstate the

107. I am grateful to Professor Jennifer Mnookin for making this point.

108. *United States v. Gilchrist*, 204 Fed. Appx. 258 (2006).

109. *See Florida v. Reyes*, Tr. Trans. at 49.

probative value of the evidence; indeed, what is remarkable is that such statements would seem, on their face, to have very little probative value or perhaps no probative value at all. What does it mean for an expert witness to say they “identified” a trace “to” the defendant? Or to have “matched” the crime scene trace to a known sample from the defendant? It has often been noted that the claim of a “match” is in itself meaningless, if not accompanied by any further information about how unusual is such match.¹¹⁰ In addition, it is necessary to know the accuracy of this “matching” or “identification” process. Absent such knowledge, the jury has no way of assessing the probative value of a “match” or “identification.”¹¹¹ Any statistically or logically informed evaluation of a Process statement—which, of course, is not something we can expect from juries—would prompt the questions: What is this Process by which the defendant was “identified” (or “matched”)? How accurate is it? How discriminating? The Process statements do not say, and, therefore, the jury should not be able to infer any probative value from them at all. Consider: it would be one thing to “identify” a bloodstain “to” a defendant by visual analysis (the defendant has a fresh wound), another to “identify” the bloodstain “to” the defendant by serological analysis (same ABO blood type) and still another to “identify” it “to” the defendant by DNA profiling. The probative values of each “matching” process are wildly divergent, yet none of that divergence is accounted for or communicated to the jury if all three processes are characterized by nothing more than Process statements.

Of course, Process statements are not valueless, at least when they concern latent print evidence. Common sense and numerous cases attest that juries will find defendants guilty beyond a reasonable doubt based solely on latent print evidence.¹¹² Something must be bestowing probative value on the evidence that is not discernible merely by reading the expert’s testimony on its face.

110. For a discussion of “matches,” see *supra* note 106 and accompanying text.

111. See *United States v. Green*, 405 F.Supp.2d 104, 119 (D. Mass. 2005) (noting that expert evidence may be excluded “because the factfinder has no information about the likelihood of error in the opinions, and thus cannot adjust the weight to be given to the evidence.”)

112. See, e.g., *Taylor v. Stainer*, 31 F.3d 907 (9th Cir. 1994); *People v. Riser*, 305 P.2d 1 (Cal. 1956); *People v. Adamson*, 165 P.2d 3 (Cal. 1946); *People v. Ramirez*, 113 Cal. App. 204, 205-06 (Cal. Ct. App. 1931); *People v. Atwood*, 223 Cal. App.2d 316 (Cal. Dist. Ct. App. 1963); *People v. Ang*, 204 Cal. App.2d 553 (Cal. Dist. Ct. App. 1962); *People v. Wise*, 199 Cal. App.2d 57, 60 (Cal. Dist. Ct. App. 1962); *People v. Beem*, 192 Cal. App.2d 207 (Cal. Dist. Ct. App. 1961) (fingerprints on dusty suitcase piled on top of show window and beneath hole cut in ceiling where entry was made); *People v. Massey*, 196 Cal. App.2d 230, 232 (Cal. Dist. Ct. App. 1961) (fingerprint on inside of open bedroom window through which burglar fled); *People v. Rodis*, 145 Cal. App.2d 44, 45-46 (Cal. Dist. Ct. App. 1956) (fingerprints on outside of window that was nine-feet above ground and through which entry was made); *Grice v. State*, 151 S.W.2d 211 (Tex. Crim. App. 1941). *But see Mikes v. Borg*, 947 F.2d 353 (9th Cir. 1991); *Ballard v. State*, 923 So.2d 475 (Fla. 2006).

It is not difficult to imagine what this “something” might be. The most likely explanation is that it is the word “fingerprint” that does the expert’s work of conveying to the jury the idea that this is an identification process with very high probative value. Fingerprinting enjoys a very high level of popular acceptance and enjoys a mythos that makes it nearly synonymous with infallibility.¹¹³ In other words, it is not necessary for the expert to state the probative value of this “identification” or “matching” process; the jury does that for the expert based on the “cultural assumption” that a latent print “match” or “identification” is an absolute fact and the result of an “infallible” process. This saves the witness from having to misstate the probative value of the evidence, or even from having to state it all. The latter benefit is especially useful for latent print evidence where, many scholars have argued, one of the fundamental problems is that expert witnesses have no way of generating a defensible estimate of the probative value of the evidence.¹¹⁴

2. *Source Attribution Statements*

Sixteen testimonies contained what I call “source attribution statements.” These are flat assertions that the defendant “made” the print, that the print “is” the print of the defendant or that the defendant “could be the only source” of the print. The model testimony cited above is an example of such a statement.¹¹⁵ More typical is the following:

The No. 1 latent fingerprint was the No. 10 finger of Terry Patterson, which is the left little finger. The No. 2 latent fingerprint is Terry Patterson’s No. 9 finger, which is the left ring finger; and the No. 3 latent is the No. 8 finger, which is the left middle finger of Terry Patterson.¹¹⁶

These statements are more problematic than Process statements for several reasons. First, they represent the problem of over-claiming that we have been discussing here. It has become axiomatic among forensic scien-

113. See generally *State v. Quintana*, 103 P.3d 168 (Utah Ct. App. 2004) (Thorne, J., concurring) (“In essence, we have adopted a cultural assumption that a government representative’s assertion that a defendant’s fingerprint was found at a crime scene is an infallible fact, and not merely the examiner’s opinion.”). This “cultural assumption” is, of course, intensified by latent print examiners’ penchant for declaring the technique infallible and for claiming that it has a zero error rate, a topic explored in excessively exhaustive detail in other work. See generally Simon A. Cole, *More Than Zero: Accounting for Error in Latent Fingerprint Identification*, 95 J. CRIM. L. & CRIMINOLOGY 985 (2005).

114. See generally THOMPSON & COLE, *supra* note 97; David M. Siegel et al., *The Reliability of Latent Print Individualization: Brief of Amici Curiae submitted on Behalf of Scientists and Scholars by The New England Innocence Project*, Commonwealth v. Patterson, 42 CRIM. L. BULL. 21 (2006).

115. See generally *United States v. Gilchrist*, 204 Fed. Appx. 258 (2006).

116. *Commonwealth v. Patterson*, 840 N.E.2d 12 (Mass. 2005). Trial trans. at 5-78, on file with the author.

tists that all forensic evidence is inherently probabilistic.¹¹⁷ It has been cogently argued that latent print analysis must also be inherently probabilistic.¹¹⁸ To claim that the defendant “made” the latent print is to essentially claim that the analyst has reached a level of probability so high that the possibility of the alternative hypothesis is no longer even worth mentioning to the fact-finder. Whether this is wise policy in any instance has been questioned,¹¹⁹ but it is particularly problematic when this purportedly astronomically high probability has not even been *calculated* but merely subjectively inferred based on “experience.”¹²⁰

Second, the statement that the defendant “made” the print would seem not to be a scientific statement. It does not state the results of some instrumental, or even subjective, laboratory process. It does not even confine itself to the trace evidence. It is not a statement about the evidence that the expert presumably analyzed. It is statement about an event that occurred at the time of the crime, that the expert has presumably inferred from the evidence. But why should an expert be permitted to make such an inference? Shouldn’t experts be expected to confine their statements to what they can infer directly from the evidence before them? In this case, that would be something closer to “the latent print and the defendant’s known print are not inconsistent with originating from a common source” than “the defendant made the print.” Of course, the law of expert testimony is generally thought to control expert testimony in this precise way. That the experts in these transcripts appear to have evaded that restriction without comment or sanction, I would suggest, supports my argument that the courts’ attention is usually directed toward something other than *control* (as opposed to admissibility) of expert testimony.

In such testimonies, experts have gone beyond the bounds of what they know from their expert analysis into the realm of what they *infer* from that analysis. The problem with such testimony is not merely that it invades the province of the jury; it is that it is not science and not derived solely from expert knowledge.¹²¹

3. Identity Statements

As noted, the third category of statements is less quantitatively significant than the first two. Only three testimonies could not be accounted for using the first two categories. These testimonies used what I call an “Iden-

117. See Aitken, *supra* note 100.

118. See CHAMPOD, *supra* note 103; Champod & Evett, *supra* note 103.

119. See *Population Genetic Models*, in FORENSIC DNA EVIDENCE INTERPRETATION 65 (John Buckleton et al. eds., 2005).

120. See THOMPSON & COLE, *supra* note 97.

121. See Nance, *supra* note 11, at 243 (stating “for the expert to give an opinion on an ultimate issue, the expert must implicitly weigh other evidence in a case, including evidence that goes beyond any expert’s asserted expertise”); see also KAYE, *supra* note 58, at § 1.3.1. My formulation of this argument has also benefited from discussions with Professor William C. Thompson.

tity statement” at the source attribution moment. One additional testimony used an Identity statement in conjunction with both other types of statements. The expert witness states that the latent print and the reference print were “identical” or “one and the same.” For example: “Finger No. 6 on the submitted fingerprint card was one and the same with the latent print that I developed on the Item 9 piece of paper.”¹²²

This category of statements is curious. To begin with, the testimony is false. It is axiomatic in latent print examiners’ and forensic scientists’ own literature that not only are no two impressions from different fingers “identical,” but no two impressions even of the same finger are “identical.”¹²³ The correctly formulated testimony of a latent print expert witness should concern the possibility that the latent print and the known print derive from a common source, not the possible sameness of the two prints. The statements are also, of course, false as matters of logic and semantics. The two prints are not “one and the same”; they are two different prints. Arguably, they cannot be “identical” either, without some sort of agreed upon definition of how much variance is permissible for two prints to still be deemed “identical.”¹²⁴

Even if Identity statements are taken at face value, as with the Process statements, they leave unsaid information that would be important for a statistically or logically informed evaluation of the statement. Even if two prints are “identical,” and something meaningful is meant by “identical,” it would still be necessary to know how many other individuals might also be capable of producing prints deemed “identical” to one of these under whatever parameters are being used to define “identity.”

Of course, as with Process statements, the persuasive value of Identity statements is presumably not diminished by the fact that they make little sense on their face. As with Process statements, the poor wording and logic of Identity statements is presumably compensated for by the fact that everyone—including, of course, jurors—knows what a latent print expert witness means when he or she says that the latent print and the known print are “one and the same.” The expert does not mean that the prints are exactly the same; he or she means that they appear generally the same—the same “within tolerance” is the current parlance¹²⁵—and that the expert has inferred from this appearance of sameness that they must

122. See *Jackson v. Commonwealth*, 590 S.E.2d 520 (Va. 2004), Tr. Trans., at 832.

123. See generally Wikipedia, *Fingerprint*, <http://en.wikipedia.org/wiki/Fingerprint> (last visited Apr. 2, 2007) (explaining flexibility of friction ridge skin means that no two finger or palm prints are ever exactly alike (never identical in every detail), even two impressions recorded immediately after each other); see also INMAN & RUDIN, *supra* note 97, at 133.

124. See generally CHAMPOD, *supra* note 103.

125. See, e.g., Thomas J. Ferriola, *Scientific Principles of Friction Ridge Analysis and Applying Daubert to Latent Print Identification*, available at <http://www.clpex.com/Articles/ScientificPrinciplesbyTomFerriola.htm>. The ill-defined nature of the notion of “tolerance” is one of the fundamental problems with latent print individualization.

come from a common source and ruled out the possibility that they come from different sources.

D. *Other Attributes of Testimony*

1. *Bolstering*

What is surprising about many of these testimonies is how little probative value they seem to convey. Most of them fail to actually address the issue of the probative significance of the evidence. In eight of the thirty-four cases, however, the perceived probative value of the evidence was bolstered by hyperbolic statements that, though not necessarily bearing any logical relation to the probative value of the evidence, nonetheless presumably served to heighten the fact-finder's sense of the "infallibility" of latent print analysis. Latent print expert witnesses told jurors that they were "positive," that they were "absolutely certain," that they had "no doubt" that the match was "to the elimination of all other fingers on the planet" and that "once a fingerprint or a palm print has been identified to an individual it cannot belong to anybody else except that individual." Some of these statements, such as the experts' self-characterizations as being "positive" or "certain," may exploit jurors' (and perhaps experts') confusion between confidence and accuracy. Others, however, such as the claim to have eliminated all other fingers on the planet, have no basis.¹²⁶

2. *Quantifications of Certainty*

As noted, latent print examiners do not tend to attempt to quantify the probative value of their conclusions of consistency. This is in part because no data exists from which to make a responsible estimate of that value. In addition, however, latent print examiners are banned by professional guidelines from attaching probabilistic estimates to their conclusions.¹²⁷ An exception to the ban on quantification, however, is made for the proportion "100%." This is because latent print examiners are permitted to conclude that an individual is the only possible source of a latent print in the universe.¹²⁸ They are not, however, permitted to conclude that an individual *might be* the source of a latent print (the only statistically defensible conclusion). Obviously, under such conditions, the use of the

126. See CHAMPOD, *supra* note 101; THOMPSON & COLE, *supra* note 97; Sandy L. Zabell, *Fingerprint Evidence*, 13 J.L. & POL'Y 143 (2005); Interpol European Expert Group on Fingerprint Identification, *Method for Fingerprint Identification* (2007), available at <http://www.interpol.int/public/Forensic/fingerprints/WorkingParties/IEEGFI/ieegfi.asp#>.

127. See generally Simon A. Cole, "Implicit Testing": Can Casework Validate Forensic Techniques?, 46 JURIMETRICS J. 117 (2006); Int'l Assoc. for Identification, *Resolution V*, 30 IDENTIFICATION NEWS 3 (1980); Int'l Assoc. for Identification, *Resolution VII*, 29 IDENTIFICATION NEWS 1 (1979).

128. See Scientific Working Group on Friction Ridge Analysis Study and Technology, *Standards for Conclusions*, J. FORENSIC IDENTIFICATION (2004), available at http://www.swgfast.org/Standards_for_Conclusions_ver_1_0.pdf.

proportion “100%” cannot be considered quantification in any meaningful sense because it is the product of a policy decision rather than any sort of reasoning or calculation. The fact-finder, however, is unlikely to be aware of these background conditions and might interpret the use of the proportion “100%” to be the outcome of some sort of chain of calculations based on meaningful data and information. Four of the thirty-four testimonies quantified the witness’s certainty as “100%,” in this manner.

E. *Conclusions from the Transcript Data*

What is most surprising about the sloppiness of these formulations is not so much that they are prejudicial to the defendant, but instead, how little they offer to the government. Latent print expert witnesses’ testimonial statements are remarkably lax about giving prosecutors what they theoretically need in terms of probative value. It would appear that a good deal of latent print testimony actually given in U.S. courtrooms is not particularly probative, but is nevertheless extremely persuasive. What, then, accounts for the vaunted power of latent print evidence?

Again, the answer seems obvious. The power of the testimony derives from the talismanic power of the word “fingerprint,” rather than from any articulation of the probative value of the evidence.¹²⁹ Indeed, one almost suspects that latent expert witnesses could say the word “fingerprint” and then essentially say anything that indicated that their conclusion was incriminating, rather than exculpatory, and the jury would afford the testimony enormous probative value.¹³⁰ This suggests that the remedy I propose in this article—greater judicial control over testimonial claims, rather than over admissibility—may be insufficient in the case of latent print testimony because the cultural mythos is so strong and so deep that even judicial control over testimony may be incapable of overcoming it. It is for this precise reason that Judge Thorne concluded that latent print evidence required a jury instruction to overcome the “cultural assumption” of its “infallibility.”¹³¹

A deeper way of thinking about this issue is suggested by Professor Wells’s seminal psychological research on juror evaluation of statistical evi-

129. This might suggest a reason that latent print practitioners have historically been so unconcerned with devising methods of calculating the probative value of latent print evidence. In a sense, they have mapped an alternative route to evidentiary power; rather than DNA’s route through precisely quantified calculations of probative value, latent print evidence achieved evidentiary power through cultural mythos. See generally Cole, *supra* note 81.

130. My argument about the power of the word “fingerprint” is supported by the tendency of expert witnesses testifying about *other* disciplines to seek to utter the word “fingerprint” whenever possible: “DNA fingerprinting,” “brain fingerprinting,” “unique marking just like a fingerprint,” etc. See LYNCH, *supra* note 102.

131. State v. Quintana, 103 P.3d 168 (Utah Ct. App. 2004) (Thorne, J., concurring).

dence.¹³² Wells sought to understand what has now become known as the “Wells effect,” juror reluctance to convict or award civil damages based on a “naked statistic,” a statistical inference unsupported by any other evidence.¹³³ The Wells effect has been illustrated by reference to the following two statistically equivalent statements:

EXPERT WITNESS A. Based on a blood test that is 99.8% accurate, I conclude that the defendant is the father.

EXPERT WITNESS B. Based on a blood test, there is a 99.8% probability that the defendant is the father.

Wells found that jurors were much more likely to assign paternity when presented with Expert A than when presented with Expert B. Wells suggests that difference between the two statements has to do with the way in which the expert witness vouches for the conclusion. Although the probability is the same in both cases, Expert A has in some sense staked something on the conclusion in a way that Expert B has not. Wells suggests that this generates a bond between the evidence and the ultimate fact for Expert A that is absent for Expert B: If the defendant turns out *not* to be the father, then Expert A was wrong—having said something false—but Expert B was *not* wrong. After all, Expert B clearly told the jury that there was a chance, albeit a small one, that the defendant was not the father. Were the defendant wrongly found liable for paternity, Expert B certainly could not be held morally responsible. Expert A, however, would seem more morally culpable. In short, Expert B shifts the moral responsibility for making a difficult decision—whether to convict or award civil damages based on nothing more than statistical evidence, even if the probability of error is quite low—to the jury. Jurors, Wells suggests, do not appreciate expert witnesses shifting that responsibility to them—they feel it belongs with the expert. Therefore, jurors punish Expert B accordingly by affording Expert B’s testimony less weight even though its probative value is statistically equivalent to that given by Expert A. Wells calls this a “birectional test of good evidence”; “good” evidence, in jurors’ view, is that evidence whose truth or falsity is tightly bound to the truth or falsity of the ultimate fact. “Good” evidence must become false when the ultimate fact that it indicates is proven to be false.¹³⁴ Thus, Expert A’s testimony survives the bidirectional test of good evidence, but Expert B’s does not.

How does the Wells effect map onto fingerprint evidence? Professor Wells himself pointed to fingerprint evidence as an example of the sort of evidence that might pass the bidirectional test. In making this argument,

132. See generally Gary L. Wells, *Naked Statistical Evidence of Liability: Is Statistical Probability Enough?*, 62 J. OF PERSONALITY AND SOC. PSYCHOL. 739 (1992).

133. This is, of course, the famous “blue bus” problem, beloved of evidence professors. See, e.g., Laurence H. Tribe, *Trial by Mathematics: Precision and Ritual in the Legal Process*, 84 HARV. L. REV. 1329 (1971).

134. The close resemblance between this concept and Popper’s notion of falsificationism, *supra* Part I, is too apparent to pass without remark.

Wells used a formulation that would correspond to the Source Attribution Statements I have previously described.¹³⁵ Indeed, his argument would certainly seem to hold for those statements. In Source Attribution Statements, the expert witness flatly states that the defendant is the source of the print. Even the preamble about the accuracy of the test used in Wells's simulation is gone. The latent print expert witness is fully morally accountable if the testimony turns out to be mistaken.¹³⁶ Indeed, some latent print examiners have located the reliability of the technique in precisely this notion of moral accountability.¹³⁷

But what of the Process Statements? Assessing whether or not these statements meet the bidirectional test is more difficult mainly because they are so devoid of probative value; they are not on their face incriminating statements, and thus, it is difficult for them to meet bidirectionality. It is notable, however, that eight of the seventeen Process statements use the first person (e.g., "I identified all three fingerprints with Mr. Terry Nichols.")¹³⁸ Perhaps by the use of the first person, the witness takes moral responsibility in the manner described by Professor Wells.

VIII. CONCLUSION

I have argued that in controlling the problem of expert evidence, judges and legal scholars need to shift their focus from the admissibility of evidence to control of testimony. I have suggested that the dangers to naïve fact-finders rests not so much with hearing the evidence at all, but rather with over-claiming that is often, and in some cases routinely, attached to expert testimony. I have attempted to illustrate this point with some modest empirical data on what I believe to be the typical practice of expert testimony for one of the best trusted forms of forensic evidence in the U.S. over the last two decades.

I believe that this data supports my argument, but it is illuminating in other ways as well. First, it demonstrates the wide variability of testimony

135. See Wells, *supra* note 132, at 749 ("Fingerprint experts, when allowed to state conclusions (e.g. 'I conclude that the prints lifted from the glass are those of the defendant'), are likely to pass this bidirectional test of good evidence.").

136. This does not mean, of course, that some latent print examiners who do make mistakes are not capable of shifting blame, uttering things like "the system failed me" or "I made an honest mistake." See generally Simon A. Cole, *More Than Zero: Accounting for Error in Latent Fingerprint Identification*, 95 J. CRIM. L. & CRIMINOLOGY 985 (2005). In marked contrast, was the behavior of some, but not all of the examiners in the Mayfield misattribution who readily accepted blame for the error. See generally Simon A. Cole, *The Prevalence and Potential Causes of Wrongful Conviction by Fingerprint Evidence*, 37 GOLDEN GATE U. L. REV. 39 (2006).

137. See Rhonda Boston, 147 THE WEEKLY DETAIL (June 7, 2004), <http://www.clpex.com/Articles/TheDetail/100-199/TheDetail147.htm> ("The latent print examiner needs to realize that each and every time he or she makes a call on an identification/individualization they put themselves and their credentials on the line.").

138. See Court TV, *The Oklahoma City Bombing Trial Transcripts: Terry Nichols* (Nov.14,1997), available at <http://www.courtstv.com/archive/casefiles/Oklahoma/nichtranscri-pts/1114am.html>.

that can be attached to what is essentially the same evidence in U.S. courtrooms. Such casualness concerning how testimony is phrased illustrates my argument that courts and scholars have focused too much on admissibility of evidence and too little on the nuances of testimony. Testimony should not be this haphazard. There is no way to exercise judicial control over testimony until the testimony itself is stabilized and standardized. Latent print examiners, for example, are restricted to only three possible *conclusions*, but have almost no restrictions on their *testimony*.¹³⁹ For routine procedures like latent print analysis, testimony should be professionally controlled at least to the extent that conclusions are professionally controlled.¹⁴⁰ Once professional bodies articulate what testimony *they* think is defensible, the question of whether such testimony is indeed defensible can then be litigated. This would be sort of pragmatic approach to a particular problem with the use of scientific and technical evidence in the courts for which *No Magic Wand* so resoundingly calls.

139. The exception, perhaps, is the ban on probabilistic statements. For a discussion of the exception, see *supra* note 125 and accompanying text.

140. See Scientific Working Group on Friction Ridge Analysis Study and Technology, *Standards for Conclusions*, J. FORENSIC IDENTIFICATION (2004), available at http://www.swgfast.org/Standards_for_Conclusions_ver_1_0.pdf; see also *The Theory of Identification* in Ronald Nichols, *The Scientific Foundation of Firearms and Tool Mark Identification—A Response to Recent Challenges*, CALIF. ASSOC. OF CRIMINALISTS NEWS 8 (2ndQuarter,2006), <http://www.firearmsid.com/Feature%20Articles/nichols060915/AS%20Response%20110805.pdf>.

Appendix: List of Cases Uses in Latent Print Testimony Transcript Analysis

No.	Case Name	Jurisdiction	Case No.	Year	Charge	Outcome	Name of LPE	Agency	Years experience	Process Statement	Source Attribution Statement	Identity Statement	Bolstering	Quantification
1.	State v. Fred Van Dyken	Missoula Co., Montana	6877	1986	Murder	Guilty verdict	Philip Conover	Retired (former FBI, Montana State Department of Justice	33		X			
2.	State v. Kenneth Saffold	Palm Beach Co., Florida	89-8675CFA02	1989	Burglary	Guilty verdict	Mullins			X			X	
3.	Commonwealth v. Stephan Cowans	Suffolk Co., Massachusetts	SUCR 1997-11231	1997	Attempted Murder	Guilty verdict	Dennis LeBlanc	Boston Police Department	8		X			
4.	State v. Freddie Roberson	California		1997			Robert A. Williams	County Sheriff's Department	26	X				
5.	State v. Jason Young	California		1998			Robert A. Williams	Department	26	X			X	
6.	State v. Victor Reyes	Broward Co., Florida	96-027365	2001	Murder	Acquitted	Robert Hobbrook	Broward County Crime Laboratory	16	X			X	
7.	State v. Vance Brooks	2nd Municipal District, Cook Co., Illinois	00 CR 11801	2002	Armed robbery	Guilty verdict	Mary Beth Thomas	Illinois State Police	7		X		X	X
8.	United States v. Gilchrist	Superior Ct. of D.C.	F-2158-01	2003			Randall Fitzwater	FBI	34		X			
9.	United States v. Johnson	Superior Ct. of D.C.	F-7618-01	2003			William Higginbotham	Metropolitan Police Department (D.C.)	14		X			X
10.	United States v. Baxter	Superior Ct. of D.C.	F-5928-01	2003			Tyritia Thomas			X				
11.	Regina v. Tregurtha	D. Western Australia		2003	Kidnapping	Guilty verdict	Graeme Arbery	West Australian Police Service	19	X				
12.	State v. Beuke	Ohio		1983			Michael Powers	Hamilton County Sheriff's Depart.	0					X

No.	Case Name	Jurisdiction	Case No.	Year	Charge	Outcome	Name of LPE	Agency	Years experience	Process Statement	Source Attribution Statement	Identity Statement	Bolstering	Quantification
13.	Commonwealth v. Jackson	Williamsburg, Virginia	CR02-012455	2002			Jean Mower	Virginia Division of Forensic Science	1.5			X		
14.	California v. Guerra	California Court of Appeal, 2 nd Appellate District	SA 0045990				Gilbert Aguilar	Los Angeles Police Department			X			
15.	State v. Despio	Cir. Ct. 11 th Jud. Cir., Miami-Dade City, Florida	F-03-0852				Lyubomir Nikolov	Miami-Dade Police Department		X				
16.	State v. Bates	14 th JD, Manchester, Tennessee	21291	1987			Larry Edward Hall	Tennessee Bureau of Investigation	15	X				
17.	State v. Hedlund (A) McKinney (B)	Mesa, Arizona	CR-91-90926	1992			Glenda Kay Hardy	Arizona Department of Public Safety	6.5	X				
18.		Arizona					Robert Tavernaro	Arizona Department of Public Safety	12	X				
19.	State v. Armstrong	Florida	F012033070	2006	Burglary	Acquittal	Michael Collier	Miami Dade Police Department	31	X	X			
20.	State v. Johnston	Florida			Murder	Convicted	Thomas L. Jones	Florida Department of Law Enforcement	31	X	X			
21.	State v. Vinson	Virginia	97-1789-98-1107	1998	Murder	Convicted	Robert B. Clark	Potomouth Police Department		X				
22.	People v. James	California	2225473	2006	Burglary	Plea	Ronen Shouldice	San Francisco Police Department	10		X			
23.	People v. Jackson	California	2211286	2005	Burglary	Plea	Ronen Shouldice	San Francisco Police Department	10	X				

No.	Case Name	Jurisdiction	Case No.	Year	Charge	Outcome	Name of LPE	Agency	Years experience	Process Statement	Source Attribution Statement	Identity Statement	Bolstering	Quantification
24.	State v. Stevenson	Missouri	CR305-1267F J-2	2005	Attempted murder	Convicted	Pamela Maecke Johnson, David Warren	Southeast Missouri Regional Crime Laboratory					X	X
25.	People v. Johnson	California	2226148	2006	Burglary, rape	In process	Ronen Shouidice	San Francisco Police Department	10		X			
26.	United States v. Price	D.C.	F-3012-03	2004		Convicted. Under appeal	Diane C. Downing	D.C. Metropolitan Police Department	30	X				
27.	People v. Sital	New York	220 A.D. 2d 784	1993	Murder	Convicted. Appeal denied.	Charles Schinkel	New York Police Department	11.5			X		
28.	People v. Westerfield	California		2002	Murder	Convicted	Jeffrey Graham, Pat Wertheim	San Diego Police Department (Graham)	5.5		X		X	
29.	Commonwealth v. Patterson	Massachusetts	93-11744	1995	Murder	Convicted. Reversed on appeal. Pled.	Robert Foilb	Boston Police Department	4		X			
30.	United States v. Terry Nichols	U.S.	96-CR-68	1997	Murder	Convicted.	Louis Hupp	FBI	22	X				
31.	United States v. Timothy McVeigh	U.S.	96-CR-68	1997	Murder	Convicted	Louis Hupp	FBI	22	X	X			
32.	Commonwealth v. Ali	Massachusetts	45892	1994		Convicted	Winfred Everette	FBI	31		X			
33.	People v. Knight	California	BA322651	2007			Kimberly Love	Los Angeles Police Department	13	X	X	X		
34.	People v. Cowan	California		1994	Murder	Convicted	Martin Collins	California Department of Justice			X		X	
Averages/Totals									15	18	16	4	8	4