

## **Out of the Daubert Fire and into the Fryeing Pan? Self-Validation, Meta-Expertise and the Admissibility of Latent Print Evidence in *Frye* Jurisdictions**

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Increasingly, in contemporary society, regulators, judges, and others charged with technical decision-making are compelled to wrestle with the problem of how to evaluate claims to expertise.<sup>1</sup> Who should count as an expert, and whose voice should be heard when technical decisions must be made? Over the past several decades, it has become clear that there is no easy answer to this problem. We cannot simply adhere to crude credentialism—attributing expertise to those with advanced degrees in the appropriate areas—in part, because all sorts of lay persons—from AIDS patients to sheep farmers—have shown that they can acquire and deploy expert knowledge over even highly technical domains.<sup>2</sup> Indeed, it has been

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<sup>1</sup> . EXPERTISE IN REGULATION AND LAW (Gary Edmond ed. 2004). HARRY COLLINS & ROBERT EVANS, *RETHINKING EXPERTISE* (2007).

<sup>2</sup> . Brian Wynne, *Sheep Farming after Chernobyl: A Case Study in Communicating Scientific Information*, 31 *ENVTL MAGAZINE* 33 (1989); STEVEN

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shown that the attribution of expertise itself can be viewed as sort of strategic, adversarial "game."<sup>3</sup> At the same time, we surely cannot attribute equal expertise to everyone who claims to be an expert.<sup>4</sup> At some point, technical decision makers must face the awkward task of adjudicating expertise—of legitimating some claims to expert knowledge and delegitimizing others. But surely we want such adjudications to be more than arbitrary; surely, we need some sort of rational framework for making such determinations.

Among the technical decision-makers who must adjudicate claims to expertise are trial judges. In most American jurisdictions, trial judges are charged with the task of "gatekeeping," of legitimating some claims to expertise—by allowing the expert to testify at a trial—and delegitimizing others—by barring the expert from testifying. Most American jurisdictions adhere to one of two general approaches to determining the admissibility of expert evidence. The first, which Professor Allen has called "The Deference Model," holds that trial courts should defer to an expert community to determine whether a proffered evidentiary claim is held in high esteem, or "generally accepted."<sup>5</sup> Professor Brewer has usefully called this process "practical epistemic deference."<sup>6</sup> This approach is embodied by the 1923 D.C. Circuit case *Frye v. United States* and by many state cases adopting, and in some cases elaborating on, *Frye*.<sup>7</sup> The second approach, which has most usefully been termed the "reliability-validity standard," directs trial judges to undertake an independent assessment of the relevance and reliability of a proffered evidentiary claim.<sup>8</sup> This approach is embodied by the 1993

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EPSTEIN, *IMPURE SCIENCE: AIDS, ACTIVISM AND THE POLITICS OF KNOWLEDGE* (1996).

<sup>3</sup> . Sheila Jasanoff, *Expert Games in Silicone Gel Breast Implant Litigation*, in *SCIENCE IN COURT* 83 (Freeman & Reece eds., 1998).

<sup>4</sup> . COLLINS & EVANS, *supra* note 1.

<sup>5</sup> . Ronald J. Allen, *Expertise and the Daubert Decision*, 84 J. CRIM. L. & CRIMINOLOGY 1157, 1162 (1994).

<sup>6</sup> . Scott Brewer, *Scientific Expert Testimony and Intellectual Due Process*, 107 Yale L. J. 1535, 1567 (1998) ("the process of deference by legal reasoners to experts").

<sup>7</sup> . *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923); *see also*, e.g., *People v. Kelly*, 549 P.2d 1240, 1249 (Cal. 1976).

<sup>8</sup> . DAVID H. KAYE ET AL., *THE NEW WIGMORE: EXPERT EVIDENCE* 157 (2004).

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United State Supreme Court decision *Daubert v. Merrell Dow Pharmaceuticals* and its progeny cases, *General Electric v. Joiner* and *Kumho Tire v. Carmichael*.<sup>9</sup>

*Frye* was heavily criticized in its time.<sup>10</sup> *Daubert* has generated extensive commentary, too large and varied a literature to summarize here, and some scholars have even advocated a return to the deferential approach of *Frye*.<sup>11</sup> This paper takes no position on the debate between *Frye* and *Daubert*. It does, however, seek to draw our attention back to the deferential approach by further exploring some of the problems in the application of *Frye*, problems that scholars have tended to neglect in the last decade and a half, as attention has shifted to the reliability-validity approach.

The great advantage of the deference approach has been its reliance of the collective wisdom of an institution that commands great epistemic prestige in contemporary society: what is described in *Frye* as the "scientific community." The approach is broadly consistent with much of contemporary society's approach to technical decision making: Want to know whether nuclear power plants are safe, whether tobacco causes cancer, whether human-induced climate change is occurring? Ask an expert. To the extent that most of us prefer to defer to experts about important, highly technical decisions, *Frye* models this process, rather than, as *Daubert* does, turning judges into "amateur scientists," much as making one's own medical decisions turns the layperson into an amateur physician. Essentially, *Frye* seeks to replicate the ideal of peer review in which the consensus judgment of the scientific community should be considered the best, if not the "true," answer to scientific and technical questions. As one evidence scholar noted, "the 'real' issue is whether good

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<sup>9</sup> . *Daubert v. Merrell Dow Pharmaceuticals*, 509 U.S. 579 (U.S. 1993); *General Electric Co. v. Joiner*, 522 U.S. 136 (U.S. 1997); *Kumho Tire v. Carmichael*, 526 U.S. 137 (1999).

<sup>10</sup> . Paul C. Giannelli, *The Admissibility of Novel Scientific Evidence: Frye v. United States, A Half-Century Later*, 80 COLUM. L. REV. 1197 (1980).

<sup>11</sup> . 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. LAW & TECH. 149 (1997).

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scientists consider the evidence reliable at this time."<sup>12</sup>

But how should the deference model function when the expert community from which the proffered expert emanates is not part of what would commonly be recognized as the scientific community? *Frye* has famously been criticized for purportedly allowing astrologers to testify based on the "general acceptance" of their claims within the community of astrologers.<sup>13</sup> In reality, courts have dealt with such problems by identifying what we might call an *appropriate reference community*, a community that *is* commonly recognized as "scientific" whom the contested experts must convince in order for their claims to be deemed "generally accepted." For example, in the *Frye* case itself proponents of the systolic blood pressure test had to convince psychologists and physiologists that the test was valid. Acceptance within the community of operators of the test was insufficient.

But what is to be done when the choice of *appropriate reference community* is not seemingly self-evident,<sup>14</sup> as the psychological community was for the lie detector? What, for example, is the appropriate reference community for astrologers? One might say astronomers, but, in fact, professional astronomers spend very little time evaluating the validity of astrologers' knowledge claims. Instead, the claimed validity of astrology has been evaluated by an eclectic group of individuals from a variety of disciplinary perspectives—psychology, statistics, astronomy—who have undertaken to rigorously empirically evaluate the claims made by astrologers.<sup>15</sup> In those cases in which courts must evaluate the validity of claims for which the appropriate reference community is not obvious, courts have little alternative but to similarly turn to those individuals who

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<sup>12</sup> . Paul S. Milich, *Controversial Science in the Courtroom: Daubert and the Law's Hubris*, 43 EMORY L. J. 913, 924 (1994); see also, Schwartz, *supra* note 11, at 196.

<sup>13</sup> . Michael Saks, *Merlin and Solomon: Lessons from the Law's Formative Encounters with Forensic Identification Science*, 49 HASTINGS L.J. 1069, 1138 (1998).

<sup>14</sup> . I say "seemingly self-evident" because, of course, the choice of psychologists and physiologists as the "relevant scientific communities" could have been contested. See TAL GOLAN, *LAW OF MEN AND LAWS OF NATURE*, 252 (2004).

<sup>15</sup> . For a review, see Guide to Sources, <http://www.rudolfhsmits.nl/uguid1.htm> (last visited Mar. 15, 2008).

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have endeavored to evaluate the validity of those claims from some rational, empirical standpoint. We might call these individuals, who evaluate the validity of claims made by other experts, "meta-experts."

Latent print (or "fingerprint") identification, this article will suggest, is one of those cases for which the only way to find an expert evaluation of the proffered experts' claims is to turn to meta-experts. Since the first admissibility challenge to latent print testimony under *Daubert* in 1999, the admissibility of latent print evidence has generated several legal opinions and a fair amount of legal scholarship.<sup>16</sup> Courts have nearly unanimously found latent print evidence admissible. Legal scholars have, with an equal degree of unanimity, found that latent print evidence fails to satisfy any reasonable application of *Daubert* and *Kumho Tire*.<sup>17</sup> These discussions, however, have overwhelmingly been oriented around the *Daubert* reliability-validity approach. There have been almost no legal opinions, and no scholarship, assessing the admissibility of latent print evidence under the *Frye* deference approach. This appears to be because of the widespread assumption that, whereas latent print individualization evidence's ability to satisfy *Daubert's* "reliability" requirement may be in question, its ability to satisfy *Frye's* "general acceptance" requirement is not. Scholars, litigators, and judges appear to have assumed that the *Frye* general acceptance standard is satisfied by the widespread acceptance of the technique by its numerous practitioners. Some litigators and judges may have also been assumed that latent print evidence would evade *Frye* analysis because it is not novel evidence or because it is not scientific evidence.

All of these assumptions are false. This article argues that, in fact, latent print individualization evidence satisfies neither *Daubert* nor *Frye*. Moreover, it suggests that, contrary to a common assumption among both lawyers and legal scholars, defendants have equally strong, if not stronger, arguments for exclusion of latent print evidence

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<sup>16</sup> . See sources cited *infra* note 241.

<sup>17</sup> . Jennifer L. Mnookin, *The Validity of Latent Fingerprint Identification: Confessions of a Fingerprinting Moderate*, 7 *LAW, PROBABILITY & RISK* 127 (2008).

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under *Frye* as under *Daubert*. Thus, latent print evidence may have escaped the *Daubert* fire, only to end up in the *Fryeing* pan.<sup>18</sup>

This article has several purposes. First, it outlines a conceptual basis for challenging latent print evidence in jurisdictions adhering to *Frye*-like deference approaches to the admissibility of expert evidence. These include some of the largest jurisdictions in the United States.<sup>19</sup> In so doing, the article will further explicate some of the conceptual difficulties that have long been associated with the deference approach, especially how to constitute the "relevant scientific community" and how to measure "general acceptance."<sup>20</sup> In addition, it addresses hitherto underexplored conceptual issues raised by the unusual situation posed by latent print evidence: a non-scientific expert community offering testimony about what purports to be scientific evidence without an obvious scientific community to which a fact-finder can refer for an assessment of the validity of this testimony. The article proposes the notion of the *meta-expert* to describe the *ad hoc* reference community that must be constituted in such a case. In so doing, the article aims to enhance our understanding of the deference approach to admissibility, an approach that is currently understudied and overshadowed by the reliability-validity approach.<sup>21</sup> More broadly, the issue of how to adjudicate competing claims to expertise—how to decide who properly counts as an expert—is currently an issue of great interest and great import in many areas outside of law, concerning technical decision-making of all kinds.<sup>22</sup> It is hoped that this discussion—of one peculiar legal problem of adjudicating expertise—can contribute to that broader discussion as well. Finally, the

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<sup>18</sup> . I am not the first to make this pun. See John F. Decker & Joel Handler, *Voiceprint Identification Evidence—Out of the Frye Pan and Into Admissibility*, 26 AM. U. L. REV. 314 (1977); Deborah Maliver, Note, *Out of the Fryeing Pan and Into Daubert: Trial Judges at the Gate Will Not Spell Relief for Plaintiffs*, 56 U. PITT. L. REV. 245 (1994); David Bernstein, Note, *Out of the Fryeing Pan and Into the Fire: The Expert Witness Problem in Toxic Tort Litigation*, 10 REV. LITIG. 117 (1990); Ned Miltenberg, *Out of the Fire and into the Fryeing Pan Or Back to the Future*, 37 TRIAL 18 (2001).

<sup>19</sup> . See *infra* notes 37–38 and accompanying text.

<sup>20</sup> . Giannelli, *supra*, note 10.

<sup>21</sup> . *But see*, Schwartz, *supra* note 11.

<sup>22</sup> . See generally, COLLINS & EVANS, *supra* note 1.

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article explores some of the larger implications of the failure of latent print evidence under a deference approach. The inability of latent print evidence to muster "general acceptance" in the "relevant scientific community" not only poses admissibility challenges in both deference and reliability-validity jurisdictions, but also points to a larger issue of a fundamental split between the scientific and legal communities over the necessity of validating expert evidence.

In Part I of this article, I discuss the background to latent print admissibility challenges under *Frye*. In Part II, I explore two important conceptual issues that have long troubled the application of *Frye*: how to constitute the "relevant scientific community" and how to measure "general acceptance." In Part III, I undertake a *Frye* analysis of latent print individualization evidence, whereby I find that latent print individualization is not generally accepted in the relevant scientific community. In Part IV, I explore some of the broader implications of this finding beyond the narrow issue of legal admissibility in *Frye* jurisdictions.

## I. BACKGROUND

### A. LATENT PRINT EVIDENCE UNDER TWO ADMISSIBILITY STANDARDS

The admissibility of latent print individualization evidence has been extensively litigated over the past nine years. Most of this litigation has taken place in jurisdictions that adhere to what is colloquially known as "the *Daubert* standard" for determining the admissibility of expert evidence. This is the admissibility standard that prevails in federal court and is articulated by the Federal Rules of Evidence and the trilogy of Supreme Court cases: *Daubert v. Merrell Dow Pharmaceuticals*,<sup>23</sup> *General Electric v. Joiner*,<sup>24</sup> and *Kumho Tire v. Carmichael*.<sup>25</sup> The *Daubert* trilogy holds that trial judges must ensure that all expert evidence is both relevant and reliable. It further lays out five discretionary<sup>26</sup> criteria to assist judges in assessing

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<sup>23</sup> . 509 U.S. 579 (U.S. 1993).

<sup>24</sup> . 522 U.S. 136 (U.S. 1997).

<sup>25</sup> . 526 U.S. 137 (U.S. 1999).

<sup>26</sup> . See *infra* notes 60-67 and accompanying text.

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reliability: testing, peer review and publication, standards, error rate, and general acceptance in the relevant scientific community.<sup>27</sup> In addition to the federal courts, at least twenty-five states have also adopted the *Daubert* standard.<sup>28</sup> With some qualified exceptions, challenges to latent print evidence under the *Daubert* standard have been unsuccessful.<sup>29</sup>

However, there is a second major standard governing the admissibility of expert evidence in American jurisdictions. This second standard, colloquially known as "the *Frye* rule," is older and derives from the 1923 D.C. Circuit case *Frye v. United States*.<sup>30</sup> *Frye* posits a single test of admissibility of scientific evidence, which is generally summarized by the term "general acceptance." The relevant language from the *Frye* opinion is as follows:

Just when a scientific principle or discovery crosses the line between the experimental and demonstrable stages is difficult to define. Somewhere in this twilight zone the evidential force of the principle must be recognized, and while courts will go a long way in admitting expert testimony deduced from a well-recognized scientific principle or discovery, the thing from which the deduction is made must be sufficiently established to have gained general acceptance in the particular field in which it belongs.<sup>31</sup>

A crucial distinction is that *Frye*, unlike *Daubert*, posits a "deference model" for evaluating scientific evidence.<sup>32</sup> Whereas *Daubert* asks the trial court itself to render a judgment as to whether the proffered evidence is reliable, *Frye* directs the court to defer to the judgment of the "relevant scientific community." *Frye*, as Professor Schwartz has pointed out, assumes that scientists themselves are the best judges of scientific claims.<sup>33</sup> As such, a judge operating under *Frye* is not being asked to form an independent judgment of the reliability of the technique. Instead, the judge is being asked to engage in a

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<sup>27</sup> . Alice B. Lustre, *Post-Daubert Standards for Admissibility of Scientific and Other Expert Evidence in State Courts*, 90 A.L.R.5th 453, 480 (2001).

<sup>28</sup> . *Id.* at 454; see also *infra* notes 37–38 and accompanying text.

<sup>29</sup> . See Part I. C. *infra*.

<sup>30</sup> . 293 F. 1013 (D.C. Cir. 1923).

<sup>31</sup> . *Id.* at 1014.

<sup>32</sup> . Allen, *supra* note 5.

<sup>33</sup> . See



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sort of scientometric<sup>34</sup> exercise in which she attempts to measure the acceptance of the technique among scientists.

This article will endeavor to model the conceptual process of a *Frye* inquiry into the admissibility of expert evidence by latent print examiners. The reader will be put in the position of a trial judge undertaking such an inquiry. Consistent with the philosophy of the deference model, I will deliberately refrain from explaining the nature of fingerprint evidence and the scientific objections to it. Strictly speaking, such matters should be outside the purview of the trial court in a *Frye* jurisdiction. All the trial court really needs to know is whether the "relevant scientific community" accepts the proffered expert's claim or not. Strictly speaking, the judge need not even understand why scientists hold the views they do, as long as the evidence concerning general acceptance is convincing. In this article, I attempt to compel the reader to adopt this mode of reasoning by deliberately withholding technical explanations or objections to fingerprint evidence. There are numerous sources through which the interested reader may learn why scientists and scholars do not accept the claims of latent print examiners.<sup>35</sup>

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<sup>34</sup> . "Scientometrics," a variant of bibliometrics, is a field which, among other things, seeks to measure the influence of particular scientific publications by the amount of citations. See generally Helmut A. Abt, *Do Important Papers Produce High Citation Counts?*, 48 SCIENTOMETRICS 65 (2000).

<sup>35</sup> . See generally 4 MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (David L. Faigman et al. eds., 3rd ed. 2007) [hereinafter MODERN SCIENTIFIC EVIDENCE]; Simon A. Cole, *Is Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents' Discourse*, 28 LAW & POL'Y 109 (2006) [hereinafter Cole, *Fingerprint Identification*]; Robert Epstein, *Fingerprints Meet Daubert: The Myth of Fingerprint "Science" is Revealed*, 75 SO. CAL. L. REV. 605 (2002); Lyn Haber & Ralph Haber, *Scientific Validation of Fingerprint Evidence under Daubert*, 7 LAW, PROBABILITY, & RISK 87 (2008); Tamara F. Lawson, *Can Fingerprints Lie? Re-weighing Fingerprint Evidence in Criminal Jury Trials*, 31 AM. J. CRIM. L. 1 (2003); Jennifer L. Mnookin, *Fingerprint Evidence In An Age of DNA Profiling*, 67 BROOK. L. REV. 13 (2001) [hereinafter Mnookin, *Fingerprint Evidence*]; Katherine Schwinghammer, *Fingerprint Identification: How "The Gold Standard Of Evidence" Could Be Worth Its Weight*, 32 AM. J. CRIM. L. 265 (2005); Sandy L. Zabell, *Fingerprint Evidence*, 13 J.L. & POL'Y 143 (2005). For defenses of latent print identification, see CHRISTOPHE CHAMPOD ET AL., *FINGERPRINTS AND OTHER RIDGE SKIN IMPRESSIONS* (2004); André Moenssens, *Fingerprint Identification: A Valid Reliable "Forensic Science"?*, 18 CRIM. JUST. 31 (2003).

Although *Daubert* is generally perceived as the wave of the future, reports of *Frye*'s demise<sup>36</sup> may be exaggerated. There are still at least twelve states that adhere to some form of the *Frye* rule, and "*Frye* states" still include some of the nation's largest jurisdictions (e.g., California, New York, Florida, Illinois, Pennsylvania).<sup>37</sup> Six additional states have incorporated *Daubert* factors but continue to adhere to *Frye*.<sup>38</sup> Moreover, while many legal scholars have criticized *Daubert*, some have gone so far as to praise the "philosophical insight" of the *Frye* rule and called for its reinstatement in all jurisdictions.<sup>39</sup>

B. THE ASSUMPTION OF ADMISSIBILITY UNDER *Frye*

When criminal defendants began litigating admissibility challenges to latent print evidence in 1999, it was widely assumed that such challenges were only possible under *Daubert*. *Daubert*, it was argued, had opened a door to reconsideration of the admissibility of latent print evidence, a door that had been closed under *Frye*.<sup>40</sup> There were a number of reasons for this assumption, but in this article, I will argue that this assumption (an assumption I shared as well) may have been premature.

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<sup>36</sup> . See DAVID L. FAIGMAN ET AL., SCIENCE IN THE LAW: STANDARDS, STATISTICS AND RESEARCH ISSUES 7-10 (2002) (noting "[t]he Decline of *Frye*" and citing "increasing" use of the *Daubert* standard, but acknowledging that *Frye* "remains the standard by which science is evaluated for courtroom use in many jurisdictions"). See also Richard Friedman, *The Death and Transfiguration of Frye*, 34 JURIMETRICS 133 (1994).

<sup>37</sup> .

<sup>38</sup> . In addition to the federal courts, the *Daubert* jurisdictions are: Alaska, Arkansas, Colorado, Connecticut, Delaware, Idaho, Indiana, Iowa, Kentucky, Louisiana, Maine, Montana, New Mexico, North Carolina, Ohio, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Vermont, West Virginia, Wyoming. The *Frye* jurisdictions are: Arizona, California, the District of Columbia, Florida, Illinois, Kansas, Maryland, Minnesota, Missouri, New York, North Dakota, Pennsylvania, and Washington. States which combine *Frye* and *Daubert* approaches are: Alabama, Hawaii, Massachusetts, Nevada, New Hampshire, and New Jersey. States with their own admissibility rules are: Georgia, Utah, Virginia, and Wisconsin. States that have a split of authority supporting both *Frye* and *Daubert* approaches are: Michigan, Mississippi, and Nebraska. *Id.*

<sup>39</sup> .

<sup>40</sup> . Paul C. Giannelli, *The Supreme Court's "Criminal" Daubert Cases*, 33 SETON HALL L. REV. 1071, 1098 (2003) ("such challenges would not have occurred under *Frye*.").

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First, *Daubert* demands that expert evidence demonstrate not only relevance but also reliability. The demand for a demonstration of reliability was thought to pose greater difficulties than the "general acceptance" requirement for latent print individualization evidence, especially when the criminal defense bar became aware that there were in fact no studies demonstrating the reliability of latent print individualization.<sup>41</sup> By this time, legal scholars had begun to note the apparent irony that *Daubert*, which had explicitly stated that it was intended to loosen the restrictions on expert evidence, in fact, appeared to be a more stringent standard.<sup>42</sup> Professor Saks argued that which standard was more exacting depended on the type of evidence.<sup>43</sup> Some forms of evidence, such as very cutting edge scientific results, might have high reliability but low general acceptance. Such evidence was more likely to be admitted under *Daubert* than *Frye*. Others enjoyed high general acceptance, but had little or no evidence demonstrating reliability. Such evidence was more likely to be admitted under *Frye* than *Daubert*. Professor Saks included latent print evidence (along with much of the rest of the trace evidence forensic sciences)<sup>44</sup> in this category, and his analysis no doubt did much to inform many legal actors' assumptions (including mine) that challenges to latent print individualization evidence were unlikely to be successful in *Frye* jurisdictions.

Consistent with the Saksian view, most legal scholars believed that latent print individualization evidence had difficulties under four of the five *Daubert* reliability criteria. In most cases, however, an exception was made for general acceptance. Some legal scholars conceded that latent print evidence probably satisfied the general acceptance criterion.<sup>45</sup> Some admissibility challenges to latent print evidence, in fact, conceded the general acceptance prong.

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<sup>41</sup> .

<sup>42</sup> . *E.g.*,

<sup>43</sup> . *Id.*

<sup>44</sup> . Trace evidence forensic sciences would include such disciplines as fingerprinting, tool mark identification, bite mark identification, forensic DNA profiling, microscopic hair and fiber comparison, forensic document examination, and footwear analysis. *Id.* at 1094-1127.

<sup>45</sup> . *See generally* However, some attorneys have noted that latent print evidence enjoys acceptance only among practitioners.

Nevertheless, this concession was not thought to affect the overall admissibility of latent print evidence. First, proffered expert evidence that met only one of five criteria would seem to be a poor candidate for admissibility. Second, as the Supreme Court noted in *Kumho Tire*, general acceptance alone is usually insufficient to render evidence admissible "where the discipline itself lacks reliability, as, for example, do theories grounded in any so-called generally accepted principles of astrology or necromancy."<sup>46</sup> This reasoning guided, for example, the initial ruling (later vacated) in *United States v. Llera Plaza*, restricting the admissibility of latent print evidence.<sup>47</sup>

Most importantly, it has generally been assumed that an analysis of the state of general acceptance of latent print individualization evidence is something of a "no-brainer."<sup>48</sup> The "relevant scientific community" is latent print examiners. All latent print examiners "accept" latent print evidence. End of argument. Many courts have concluded that latent print evidence easily satisfies the general acceptance requirement, even when evincing skepticism about its ability to satisfy some of the other *Daubert* criteria.<sup>49</sup> I will argue, however, that this is an overly simplistic analysis within the meaning of *Frye*.

The final reason for assuming that *Frye* challenges to latent print evidence were not possible is a phrase in the *Frye* opinion that specifies that it applies to novel scientific evidence.<sup>50</sup> This creates what some evidence scholars have called a non-novelty "loophole" in *Frye*.<sup>51</sup> Forms of expert evidence which either: (1) pre-date *Frye* altogether (as in

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<sup>46</sup> . *Kumho Tire v. Carmichael*, 526 U.S. 137, 151 (1999).

<sup>47</sup> . *United States v. Llera Plaza* (*Llera Plaza I*), 179 F. Supp. 2d 492, 515 (E.D. Pa. 2002), *vacated*, 188 F. Supp. 2d 549 (E.D. Pa. 2002) ("The failure of fingerprint identifications fully to satisfy the first three *Daubert* factors militates against heavy reliance on the general acceptance factor.").

<sup>48</sup> . *United States v. Gary*, 85 F. App'x. 908 (4th Cir. 2004) ("[F]ingerprint analysis is one of those forms of evidence where the reliability of the science and its general acceptance is apparent without a full reexamination of the science.").

<sup>49</sup> . See, e.g., *United States v. Sullivan*, 246 F. Supp. 2d 700, 702 (E.D. Ky. 2003) ("The ACE-V methodology easily satisfies the general acceptance factor of *Daubert*.").

<sup>50</sup> . See *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923).

<sup>51</sup> . *Giannelli*, *supra* note 40, at 1099.

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the case of latent print individualization evidence), or (2) post-date *Frye*, but are not challenged until after they have become familiar enough to the criminal justice system to no longer be regarded as "novel," would not be challengeable under *Frye*. Such forms of expert evidence would not even reach the general acceptance issue. Because *Daubert* explicitly disavowed any novelty requirement,<sup>52</sup> it was widely assumed that challenges were more possible under *Daubert*.<sup>53</sup>

For these reasons, the criminal defense bar and legal scholars alike have assumed that challenging latent print individualization evidence under *Frye* is a hopeless cause, while admissibility challenges under *Daubert* are, at least in principle, plausible. Litigants wishing to challenge the admissibility of latent print individualization evidence in *Frye* jurisdictions have generally adopted the tactic of trying to backdoor *Daubert* by urging courts to consider the *Daubert* factors in making their *Frye* determinations.<sup>54</sup> Such tactics have not met with success.

#### C. DAUBERT CHALLENGES TO LATENT PRINT EVIDENCE

*Daubert* challenges to latent print evidence have not met with success either. Numerous published opinions have ruled on admissibility challenges to latent print evidence in *Daubert* jurisdictions.<sup>55</sup> With some qualified exceptions, these opinions have all ruled latent print individualization evidence admissible. Indeed, even the qualified exceptions generally rule latent print individualization evidence in general admissible, while excluding some specific application of it.<sup>56</sup>

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<sup>52</sup> . *Daubert v. Merrell Dow Pharmaceuticals*, 509 U.S. 579, 592 n.11 (1993) ("Although the *Frye* decision itself focused exclusively on 'novel' scientific techniques, we do not read the requirements of Rule 702 to apply specially or exclusively to unconventional evidence.").

<sup>53</sup> . Giannelli, *supra* note 40, at 1098.

<sup>54</sup> . See, e.g., *People v. Clevenger*, 2003 WL 22872446 (Cal. Ct. App. 2003).

<sup>55</sup> . For a review, see One web site lists more than 40 *Daubert* challenges to latent print evidence, not all of which are published. See Legal Challenges to Fingerprints, [http://www.onin.com/fp/daubert\\_links.html](http://www.onin.com/fp/daubert_links.html) (last visited May 24, 2008).

<sup>56</sup> . Legal Challenges to Fingerprints, [http://www.onin.com/fp/daubert\\_links.html](http://www.onin.com/fp/daubert_links.html) (last visited May 24, 2008).

This lack of success has been at stark odds with the weight of opinion in legal scholarship, nearly all of which concludes that latent print individualization evidence, as currently constituted, does not satisfy the *Daubert* standard for admissibility.<sup>57</sup> Although I believe that latent print individualization evidence must be inadmissible under any reasonable reading of *Daubert*,<sup>58</sup> it now appears that a litigant may, contrary to conventional wisdom, have a better chance of success in a motion to exclude latent print evidence in a *Frye* jurisdiction.

There are several reasons for this. First, at some point the precedential weight of the admissibility rulings will preclude admissibility motions under *Daubert*. Already, in 2004, the Third Circuit Court of Appeals tried not so subtly to put this issue to rest in its opinion upholding the admissibility of latent print individualization evidence.<sup>59</sup> Second, the *Daubert* standard is notoriously vague. Indeed, vagueness is one of the opinion's principal flaws for its many critics.<sup>60</sup> The vagueness of the *Daubert* standard principally lies, first, in the refusal to specify the five "*Daubert* factors" as a "definitive checklist or test." Second, trial court decisions are subject to the abuse of discretion review.<sup>61</sup> These factors combine to create a regime in which trial judges can follow their intuitions with very little risk of being overturned. It is very difficult for a trial court to err under *Daubert* because most decisions with which higher courts may disagree can be explained as either exercises of the trial judge's discretion in framing the *Daubert* inquiry or exercises of the trial judge's discretion in making the

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<sup>57</sup> . *Infra* note 241.

<sup>58</sup> . See Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Ruling from Jennings to Llera Plaza and Back Again*, 41 AM. CRIM. L. REV. 1189, 1196 (2004) [hereinafter Cole, *Grandfathering*].

<sup>59</sup> . *United States v. Mitchell*, 365 F.3d 215, 246 (3d Cir. 2004). ("[A] district court would not abuse its discretion by limiting, in a proper case, the scope of a *Daubert* hearing to novel challenges to the admissibility of latent fingerprint identification evidence—or even dispensing with the hearing altogether if no novel challenge was raised."); Simon A. Cole, *Does 'Yes' Really Mean Yes? The Attempt to Close Debate on the Admissibility of Fingerprint Testimony*, 45 JURIMETRICS 449, 452 (2005) [hereinafter Cole, *Yes*].

<sup>60</sup> . See, e.g., John H. Mansfield, *Scientific Evidence Under Daubert*, 28 ST. MARY'S L.J. 1, 45 (1996).

<sup>61</sup> . *General Electric Co. v. Joiner*, 522 U.S. 136, 136-37 (1997).

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ultimate admissibility determination. It has been suggested that *Daubert* challenges to latent print evidence have failed not because there has been any empirical demonstration of the technique's accuracy or validity, but because latent print evidence benefits from a high degree of what comedian Stephen Colbert has called "truthiness," an instinctual belief that something is true even if no factual basis for that belief exists.<sup>62</sup> In other words, perhaps trial judges believe that latent print evidence is accurate, even if the proponents of the evidence cannot demonstrate it, and, therefore, they are inclined to look for ways to find that the evidence satisfies *Daubert*. If latent print admissibility rulings are indeed outcome oriented, then a vague admissibility standard with a wide range of judicial discretion gives judges more room to follow their instincts. Thus, a vague standard like *Daubert* is not conducive for unpopular litigants seeking a radical change like restricting the admissibility of latent print individualization evidence.<sup>63</sup>

This point is supported by the recent *Daubert* jurisprudence on latent print evidence admissibility. The earliest opinions tended to adopt tortured readings of the *Daubert* factors in order to find that latent print evidence met all the factors with flying colors.<sup>64</sup> More recent decisions, however, tend to find latent print evidence admissible despite what would appear to be shocking lapses in terms of the *Daubert* factors. For example, *United States v. Llera Plaza (Llera Plaza II)* finds latent print

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<sup>62</sup> . Mnookin, *Fingerprint Evidence*, *supra* note 35, at 66 ("It is easy to see why judges are reluctant to exclude fingerprinting: it is a long-used technique, an extremely valuable form of evidence to prosecutors, and one in which the public has enormous faith."); Jacques Steinberg, *Truthiness*, N.Y. TIMES, Dec. 25, 2005.

<sup>63</sup> . The vagueness of *Daubert* has been exacerbated, I would argue, by the five-factor list. The list has drawn both judicial and scholarly attention away from the concept it was meant to elucidate: "reliability." By focusing on the list, which is vague and which the *Daubert* Court had specified was not intended to be "definitive," rather than on the Federal Rules of Evidence (FRE) relevance and reliability requirement, which is neither flexible nor vague, judges and scholars have overstated the flexibility and vagueness of *Daubert*. If *Daubert* rulings are outcome-oriented, the outcome is usually achieved by interpreting the five-factor list, not the FRE reliability requirement.

<sup>64</sup> . Michael J. Saks, *Reliability Standards: Too High, Too Low, or Just Right? The Legal and Scientific Evaluation of Forensic Science (Especially Fingerprint Expert Testimony)*, 33 SETON HALL L. REV. 1167 (2003).

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evidence admissible despite satisfying only the  
standards/error rate and general acceptance prongs.<sup>65</sup>  
*United States v. Sullivan* found it admissible despite being  
"testable, although untested."<sup>66</sup> Finally, *United States v.*  
*Mitchell* found it admissible despite failing the standards  
prong and meeting the testing prong not with true testing,  
but only with "implicit testing."<sup>67</sup> None of these opinions  
have won praise among evidence scholars, and one could  
make a strong argument that they are erroneous  
applications of *Daubert*. But, in the final analysis, it is  
difficult to say that these opinions are absolute violations of  
*Daubert*, rather than the "flexible" interpretations of it that  
that Supreme Court seemed to call for.

The *Frye* general acceptance test, though it too  
contains unresolved ambiguities,<sup>68</sup> is at least less flexible  
and vague than *Daubert*. Indeed, *Frye* has been criticized  
for the supposed rigidity of its "nose counting" test.<sup>69</sup>  
However, as I will argue below, properly conducted, a nose  
counting test actually favors criminal defendants.  
Unpopular litigants with unpopular causes like latent print  
admissibility challenges should want a rigid admissibility  
standard that leaves the court with less discretion.

D. "THE THING FROM WHICH THE DEDUCTION IS MADE": WHAT MUST BE  
"GENERALLY ACCEPTED"?

The first step in analyzing latent print individualization  
under *Frye* is defining what it is that needs to be generally  
accepted. A general acceptance analysis might yield quite  
different results depending on how the claim is formulated.  
To use the original *Frye* case as an example, a court that  
asked whether the ability of a lie detector device to  
accurately detect deception is generally accepted would

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<sup>65</sup> . 188 F. Supp. 2d 549, 576 (E.D. Pa. 2002) (concluding that  
arrangements found sufficiently reliable in England should be found  
reliable in the United States).

<sup>66</sup> . *United States v. Sullivan*, 246 F. Supp. 2d 700, 704 (E.D. Ky.  
2003).

<sup>67</sup> . 365 F.3d at 238; see also Simon A. Cole, 'Implicit Testing': *Can  
Casework Validate Forensic Techniques?*, 46 JURIMETRICS 117, 126 (2006)  
[hereinafter Cole, *Implicit Testing*].

<sup>68</sup> . See, e.g.,

<sup>69</sup> . Joseph D. Hatina, *Shaken Baby Syndrome: Who Are the True  
Experts?*, 46 CLEV. ST. L. REV. 557, 572 n.125 (1998).



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get quite a different result from a court that asked whether the lie detector's utility in exacting confessions was generally accepted.<sup>70</sup> Similarly, a court that asked whether the ability of a lie detector to detect deception some of the time was generally accepted would find quite a different result than a court that asked whether the ability of a lie detector to detect deception with a high degree of accuracy was generally accepted. In the case of latent print evidence, a court might find a very high rate of general acceptance if it asked whether the "relevant scientific community" accepts that latent print identification is a "good" or "useful" thing or that latent print identification is "sometimes" or "often" accurate or "can" be done accurately. Similarly, the proposition that latent print source attributions are to be made accurately from complete sets of ten rolled prints might enjoy an extremely high rate of general acceptance, whereas the proposition that latent print source attributions are made accurately from single partial "latent" prints might command a far lower rate of general acceptance.

How then should "the thing from which the deduction is made" be formulated in a *Frye* challenge to latent print evidence? Given that it is possible to "game" the state of general acceptance through the formulation of the proposition to be accepted, it would seem that the only fair way to proceed is to evaluate the formulation to which the proffered expert witness proposes to testify. In this regard, courts faced with *Frye* challenges to latent print evidence are fortunate because the professional community of latent print examiners is quite explicit about what it is that they claim to be able to do. According to professional guidelines, inculpatory latent print testimony can take only one form: a testimonial claim of "individualization," which is defined as the conclusion that the source of the known print (the defendant) is the only possible source of a latent print, to the exclusion of all other possible sources in the universe.<sup>71</sup>

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<sup>70</sup> . See generally KEN ALDER, *THE LIE DETECTORS: THE HISTORY OF AN AMERICAN OBSESSION* (2007).

<sup>71</sup> . SCIENTIFIC WORKING GROUP ON FRICTION RIDGE ANALYSIS STUDY & TECHNOLOGY, *FRICTION RIDGE EXAMINATION METHODOLOGY FOR LATENT PRINT EXAMINERS 3* (2002), available at [http://www.swgfast.org/Friction\\_Ridge\\_Examination\\_Methodology\\_for\\_Latent\\_Print\\_Examiners\\_1.01.pdf](http://www.swgfast.org/Friction_Ridge_Examination_Methodology_for_Latent_Print_Examiners_1.01.pdf). [hereinafter SCIENTIFIC WORKING GROUP, FRICTION RIDGE EXAMINATION].

This is the strongest possible conclusion that any forensic analyst could offer in regard to the source of a trace, and latent print examiners offer it every time they testify to an inculcation. The ability of latent print analysis to individualize, then, is the "thing" from which the deduction that the defendant is the source of the latent print to exclusion of all other possible sources is made. Therefore, it is the ability of latent print analysis to individualize that, under *Frye*, needs to be generally accepted in the "relevant scientific community."<sup>72</sup>

It is often suggested, however, that it is the underlying "premises" of the technique, not the accuracy of the technique itself, which must be generally accepted. A common tactic in both admissibility hearings and in latent print examiners' own literature has been to advance evidence supporting the "premises" of latent print individualization instead of evidence supporting the accuracy of the technique itself.<sup>73</sup> Specifically, in admissibility hearings, the government has spent a great deal of time demonstrating the "uniqueness" and "permanence" of friction ridge skin (the anatomical structure of which finger, palm, and sole prints are impressions), rather than the accuracy of latent print individualization. Could it be that "the thing from which the deduction is made" is the uniqueness of all human friction ridge skin? It cannot. The conclusion that a single area of friction ridge skin is the only possible source of a particular latent print is not a logical deduction from the proposition that all friction ridge skin is unique. Just because the skin is unique, it does not follow that an analytic process is sufficiently diagnostic to always identify the true source of an impression of that unique skin.

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<sup>72</sup> . *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923). Some readers of this article have questioned why I have not formulated "the thing" more generously in a way that would command higher general acceptance. I must admit to being somewhat perplexed by this suggestion, given that the latent print community has the full capacity to formulate its claim, however it so chooses. It is certainly true that there would probably be a high rate of general acceptance if the claim were formulated as follows: "latent print analysis can correctly attribute source for complete sets of prints." But this is not how the latent print community and those who proffer latent print examiners as expert witnesses have formulated the claim.

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Moreover, as a matter of common sense, legal scholars and at least one court<sup>74</sup> have noted that it would defy common sense to allow acceptance of the underlying "premises" of the technique to be sufficient for admissibility; surely the issue in a *Frye* inquiry is not merely whether the premises of the technique are generally accepted but also whether or not it is generally accepted that the technique itself does what it claims to be able to do.<sup>75</sup>

## II. CONCEPTUAL DIFFICULTIES IN THE APPLICATIONS OF *FRYE*

The *Frye* rule has come under frequent criticism over the years. Scholars have argued that applying the *Frye* rule is not nearly as clear-cut as it might appear at first glance. Two difficulties applying *Frye*, in particular, have generated concern. One is how the "relevant scientific community" is defined. The second is how "general acceptance" is measured.<sup>76</sup>

### A. CONSTITUTING THE "RELEVANT SCIENTIFIC COMMUNITY"

Critics of *Frye* have pointed out that the "relevant scientific community" is not always obvious.<sup>77</sup> For example, situations may arise in which one specialist community "accepts" a particular principle or technique, while another

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<sup>74</sup> . State v. Velasco, 799 P.2d 821, 827 (Ariz. 1990) (en banc) ("The question is not whether the scientific community has concluded that the scientific principle or process is absolutely perfect, but whether the principle or process is generally accepted to be capable of doing what it purports to do.").

<sup>75</sup> . On the distinction between the validity of a technique and the theory behind it, see *See also* ("For testimony to be sufficiently probative to warrant admission . . . the fundamental theory and the existence of a valid procedure for taking the necessary measurements and drawing the appropriate inferences needs to be established."). The uniqueness of all human friction ridge skin may logically count as a "premise" of latent print individualization—it is a necessary but not sufficient condition of the claim of individualization—but I would question its status as a "theory." The claim of uniqueness does not purport to explain how or why analyses by latent print examiners result in individualization, it merely proposes that the targets of their analyses are "unique."

<sup>76</sup> . DAVID L. FAIGMAN ET AL., SCIENCE IN THE LAW: STANDARDS, STATISTICS AND RESEARCH ISSUES 8 (2002).

<sup>77</sup> .

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specialist community is more skeptical. Such "cases of extra-disciplinary competition of credentialed experts" may take several forms.<sup>78</sup> In some cases, acceptance may be greater in one discipline than in another. In other cases, the disagreement may pit a practitioner community against a community of scientists.<sup>79</sup>

#### 1. Practitioners Only

Can a technique satisfy the *Frye* rule if it is accepted by practitioners, but not by the broader scientific community? The *Frye* case itself, as well as subsequent cases concerning polygraph evidence, were practitioner-only cases. Practitioners of lie detector tests "accepted" them as valid, whereas the broader scientific community, defined variously as psychologists, physiologists, or neurologists, was more skeptical. This was also the case in voice spectrography cases, in which practitioners of the technique accepted it as valid, whereas the scientific community, consisting of audiologists, acousticians, speech scientists, acoustical engineers, anatomists, electrical engineers, linguists, phoneticists, physicists, physiologists, psychologists, and statisticians, was more skeptical.<sup>80</sup> In such cases, if the "relevant scientific community" was defined as the practitioner community, the technique appeared to be generally accepted, but if the "relevant scientific community" was defined as the broader scientific community, it may not have been.<sup>81</sup> Thus, as Professors Faigman, Kaye, Saks, and Sanders have pointed out, how the "relevant scientific community" is defined determined the outcome of the *Frye* inquiry in every voice spectrography case.<sup>82</sup>

By and large, however, there is little disagreement on the issue of whether practitioners alone can constitute the "relevant scientific community." Courts have generally

<sup>78</sup> . Brewer, *supra* note 6, at 1633.

<sup>79</sup> . Christopher Slobogin, *Dangerousness and Expertise*, 133 U. PA. L. REV. 97 139 (1984).

<sup>80</sup> . DAVID L. FAIGMAN ET AL., *SCIENCE IN THE LAW: FORENSIC SCIENCE ISSUES* 296 n.4 (2002) [hereinafter, FAIGMAN, FORENSIC SCIENCE ISSUES].

<sup>81</sup> . *Id.* at 297.

<sup>82</sup> . *Id.*; Giannelli, *supra* note 10, at 1214 (noting that "general acceptance of the polygraph is almost assured if the opinions of [polygraph] examiners are considered").

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found that practitioner-only acceptance cannot satisfy the *Frye* rule. In *Frye* itself, the systolic blood pressure test failed because it was not generally accepted "among physiological and psychological authorities," rather than, say, being admitted because it was accepted by William Moulton Marston, its developer, and his disciples.<sup>83</sup>

Maryland's case adopting the *Frye* rule, *Reed v. State*, chided the trial court for restricting the "relevant scientific community" to "the group actually engaged in the use of this technique and in the experimentation with this technique."<sup>84</sup> The court wrote:

[W]e find that the trial court's formulation is inconsistent with the proper standard of acceptance necessary for admissibility. The circumstances of the instant case suggest no basis for "restricting the relevant field of experts" to those who have performed voiceprint experiments, and eliminating from consideration the opinions of those scientists in the fields of speech and hearing, as well as related fields, who, by training and education, are competent to make professional judgments concerning experiments undertaken by others. The purpose of the *Frye* test is defeated by an approach which allows a court to ignore the informed opinions of a substantial segment of the scientific community which stands in opposition to the process in question.<sup>85</sup>

The Arizona Supreme Court agreed.<sup>86</sup>

The Alaska courts have not only included non-practitioners in the "relevant scientific community," but have even excluded practitioners. In *Contreras v. State*, regarding hypnosis, the Supreme Court of Alaska wrote:

We define the relevant scientific community as the academic, scientific, and medical or health-care professions which have studied and/or utilized hypnosis for clinical, therapeutic, research and investigative applications. It does not include those whose involvement with hypnosis is strictly limited to that of practitioner, technician or "operator" . . . . We exclude technicians from the group because *Frye* requires scientific, not merely technical, judgments to be made.<sup>87</sup>

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<sup>83</sup> . *Frye v. United States*, 293 F. 1013, 1014 (D.C. Cir. 1923).

<sup>84</sup> . *Reed v. State*, 391 A.2d 364, 377 (Md. 1978).

<sup>85</sup> . *Id.*

<sup>86</sup> . *State ex rel. Collins v. Superior Court*, 644 P.2d 1266, 1285 (Ariz. 1982) ("This requirement is not satisfied with testimony from a single expert or group of experts who personally believe the challenged procedure is accepted or is reliable.").

<sup>87</sup> . *Contreras v. State*, 718 P.2d 129, 135 (Alaska 1986); see also *Trout-Clark v. State*, No. A-4666, 1993 WL 13157037, \*4 (Alaska App. Oct.

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The courts' rationale for evincing skepticism concerning techniques that are accepted only by practitioners appears to have been motivated principally by two concerns. First, practitioners tend to be materially interested in the validity of the technique. That is, they tend to stand to benefit financially if the technique is legitimated by a favorable admissibility ruling in the courts. Therefore, such individuals' "acceptance" of the technique should be taken with a grain of salt. For example, a Florida District Court of Appeal excluded polygraph evidence because "[t]he only testimony was from two people who earn a living by giving polygraph tests."<sup>88</sup> Similarly, the Supreme Court of Michigan stated

While one would not want an expert witness without experience or background in the technical field, one would want, where the task was to demonstrate general scientific acceptability, an acknowledgment of the value of the device and the techniques by disinterested scientists whose livelihood was not intimately connected with it.<sup>89</sup>

In a later case, the court stated:

To allow general scientific acceptance to be established on the testimony alone of witnesses whose livelihood is intimately connected with a new technique would eliminate the safeguard of scientific community approval implicit in the general scientific acceptance test. Scientific community approval is absent where those who have developed and whose reputation and livelihood

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20, 1993) (excluding horizontal gaze nystagmus evidence because testimony of practitioner did not suffice for general acceptance in relevant scientific community); *Halley v. State*, No. A3463, 1991 WL 11650674, \*3 (Alaska App. Jan. 16, 1991) (excluding preliminary breath test evidence because testimony of practitioner did not suffice for general acceptance in relevant scientific community); *Haakanson v. State*, 760 P.2d 1030, 1034 (Alaska App. 1988) (finding polygraph evidence failed to satisfy *Frye* because polygraph examiner, in contrast to defendant's expert, a Psychology Professor, was not member of the relevant scientific community). Alaska has since adopted *Daubert*. See *State v. Coon*, 974 P.2d 386, 402 (Alaska 1999).

<sup>88</sup> . *State v. Thompkins*, 891 So.2d 1151, 1152 (Fla. Dist. Ct. 2005).

<sup>89</sup> . *People v. Barbara*, 255 N.W.2d 171, 180 (Mich. 1977); see also *People v. Coy*, 669 N.W.2d 831, 838 (Mich. Ct. App. 2003) (per curiam) ("When demonstrating that there is general scientific recognition of novel scientific techniques or principles, it is necessary to present the testimony of disinterested and impartial experts whose livelihood is not intimately connected with the technique at issue."); *Collins*, 644 P.2d at 1285 ("Acceptance must be by those experts who are relatively disinterested and impartial and whose livelihood, therefore, is not intimately connected with approval of the technique.").

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depends on use of the new technique alone certify, *in effect self-certify*, the validity of the technique . . . . If this Court were to adopt the view that the testimony of persons who have developed and whose reputation and livelihood depends on the use of a new technique alone supports admissibility, then the views of the developer and his disciples would be substituted for the scrutiny of the marketplace of general scientific opinion and the substance of the *Frye* test would be eliminated.<sup>90</sup>

Courts have also recognized, however, that even beside financial interest, practitioners are vulnerable to developing a personal stake in the validity of a technique. Having spent a great deal of their professional time on developing, learning, disseminating, or advocating the technique, practitioners may find it very difficult to simply concede that the technique is not valid, no matter what the empirical evidence. As the Florida court went on to say about polygraph evidence, "*Frye* requires more than the testimony of an expert who has a personal stake in the theory or is prone to an institutional bias."<sup>91</sup> In *People v. Kelly*, the Supreme Court of California viewed the testimony of a leading practitioner of voice spectrography with caution because "he has virtually built his career on the reliability of the technique."<sup>92</sup> A California appellate court in an earlier case went further, arguing, as had the Alaska Supreme Court, to exclude practitioners from the "relevant scientific community," stating that in deciding whether "a technique or process is generally accepted in the scientific community, self-serving opinions should not be received . . ."<sup>93</sup>

About the idea of allowing practitioners to constitute the "relevant scientific community," the Eighth Circuit Court of Appeals said this: "[s]ome commentators have posited the argument that the polygraph need only attain general acceptance among the polygraph operators themselves to satisfy the test for admissibility . . . . This position must be rejected."<sup>94</sup> Instead, the court suggested that courts might turn to the mainstream scientific community: "Experts in neurology, psychiatry and physiology may offer needed

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<sup>90</sup> . *People v. Young*, 391 N.W.2d 270, 276 n.24 (Mich. 1986) (emphasis added).

<sup>91</sup> . *Thompkins*, 891 So.2d at 1152.

<sup>92</sup> . *People v. Kelly*, 549 P.2d 1240, 1249 (Cal. 1976).

<sup>93</sup> . *People v. King*, 72 Cal. Rptr. 478, 491 (Cal. Ct. App. 1968).

<sup>94</sup> . *United States v. Alexander*, 526 F.2d 161, 164 n.6 (8th Cir. 1975).

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enlightenment upon the basic premises of polygraphy."<sup>95</sup>  
The Third Circuit also noted disapprovingly "some courts, when they wish to admit evidence, are able to limit the impact of *Frye* by narrowing the "relevant scientific community" to those experts who customarily employ the technique at issue."<sup>96</sup> Similarly, the Florida Supreme Court noted, "[i]n applying the *Frye* criteria, general scientific recognition requires the testimony of impartial experts or scientists. It is this independent and impartial proof of general scientific acceptability that provides the necessary *Frye* foundation."<sup>97</sup>

Significantly, although some courts have functionally narrowed the "relevant scientific community," most commonly in cases upholding the admissibility of voice spectrography, in almost none of those cases have courts defended or even articulated limiting the "relevant scientific community" to practitioners as a *principle*.<sup>98</sup> One exception is the Minnesota Supreme Court, which disagreed with the Michigan Supreme Court's insistence on relying on relatively disinterested experts.<sup>99</sup> There are also some cases in which evidence is deemed admissible when it "has obtained general acceptance in only one branch of science," such as techniques that are accepted only in forensic chemistry, but not in chemistry generally.<sup>100</sup> Such situations are quite different from the situation for latent prints, in which the technique is only accepted, not by a "branch of science," but by practitioners of the technique,

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<sup>95</sup> . *Id.*

<sup>96</sup> . United States v. Downing, 753 F.2d 1224, 1236 (3d Cir. 1985).

<sup>97</sup> . Ramirez v. State, 810 So. 2d 836, 851 (Fla. 2001).

<sup>98</sup> . See, e.g., Hodo v. Superior Court, Riverside County, 106 Cal. Rptr. 547 *passim* (Cal. Ct. App. 1973); Commonwealth v. Lykus, 327 N.E.2d 671 *passim* (Mass. 1975); People v. Bein, 453 N.Y.S.2d 343 *passim* (N.Y. Sup. Ct. 1982); United States v. Maivia, 728 F. Supp. 1471 *passim* (D. Haw. 1990).

<sup>99</sup> . State v. Fenney, 448 N.W.2d 54, 60 (Minn. 1989) ("The *Young* decision is flawed from the Minnesota perspective because of the court's requirement that witnesses qualified to testify as members of the relevant scientific community must be 'disinterested and impartial' experts whose 'livelihood [is] not intimately connected with the new technique.' . . . Minnesota's interpretation of *Frye* requires 'experts in its field' and has no such narrow requirement of disinterestedness.").

<sup>100</sup> . See Robinson v. State, 425 A.2d 211, 220 (Md. Ct. Spec. App. 1981).



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the vast majority of whom do not have scientific training.<sup>101</sup>

To be sure, courts have acknowledged that a balance must be struck between finding experts who are knowledgeable with the technique and those who are disinterested. A completely disinterested expert may lack knowledge.<sup>102</sup> Therefore, courts have required only relative disinterest.<sup>103</sup> The point here is merely that it is well established that courts have reason for skepticism when general acceptance emanates *only* from practitioners with a degree of both financial and emotional interest in the

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<sup>101</sup> . Latent print examiners themselves do not appear to be scientists, at least in the conventional sense of being trained in, and acquiring advanced degrees in, science. Historically, latent print examiners have been drawn from the ranks of law enforcement officers and identification bureau clerks, and scientific training was not a credential required to analyze latent prints. David L. Grieve, *The Identification Process: Traditions in Training*, 40 J. FORENSIC IDENTIFICATION 195 *passim* (1990). Even today, *after* the profession instituted formal educational requirements—perhaps in response to criticisms of the lack of training standards exposed in early *Daubert* challenges—SWGFAST guidelines only *recommend* a bachelor's degree in science. Work experience can still compensate for the lack of such a degree, according to SWGFAST. Since the SWGFAST guidelines are not binding on law enforcement agencies and are of recent origin, many practicing latent print examiners may fall short of even these modest scientific credentials.

Even if latent print examiners did all possess SWGFAST's recommended credential, the B.S. degree, it is not clear that they would properly be considered scientists. Most working scientists would probably want to see an advanced degree in science before calling an individual a "scientist." Although, I am aware of a handful of individual latent print examiners who possess such degree, it would seem that they are a small minority in the profession. One study found that only 3% of crime laboratory directors require a M.S. degree for a position as a firearms, document, or fingerprint examiner, and only 2% require a Ph.D. Kenneth G. Furton et al., *What Educational Background Do Crime Laboratory Directors Require from Applicants*, 44 J. FORENSIC SCI. 128, 131 (1999). Moreover, it appears likely that these credential requirements may reflect crime laboratory directors' aspirations more than the true credentials available to them in the labor market. It is difficult to imagine a crime laboratory requiring a Ph.D. for a position in firearms, document, or fingerprint examination, and it is equally difficult to imagine them successfully filling the position with such an individual. I am not aware of any latent print examiner who has this credential, though I am aware of one individual who is seeking a doctoral degree in forensic science (Glenn Langenburg of the Minnesota Bureau of Criminal Apprehension. Trial Transcript, State v. Columbus, No. 04082599 (Minn. Hennepin County Dist. Ct. May 18, 2006).).

To be sure, it is possible to argue that latent print examiners are credential-less scientists. We would not necessarily want to take the position that an individual or group of individuals who are clearly doing

perpetuation of the technique.

Evidence scholars also agree that practitioner communities alone cannot satisfy the general acceptance requirement. Professor Black notes that such definitions of the relevant scientific community would "allow[] a group that advocates a technique or method to self-validate it simply by declaring acceptance."<sup>104</sup> This would also allow self-validation by astrologers, cults, and what Professor Schwartz colorfully calls "mutual admiration societ[ies]."<sup>105</sup> Professor Schwartz notes that if the "relevant scientific community" consists solely of individuals whose "professional reputations and commercial interests . . . depend on validation of the technique, general acceptance may be a foregone conclusion . . . ."<sup>106</sup> Specifically with

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scientific work, but lack formal advanced degrees, could not be considered scientists. For example, there are talented amateur astronomers who make what are considered by professional astronomers to be genuine contributions to astronomical knowledge. For these exceptional individuals, their lack of a doctoral degree in astronomy does not negate their contribution to the corpus of scientific knowledge. Some latent print examiners have invoked this argument, claiming that a scientist is one who analyzes and compares. Simon A. Cole, *What Counts for Identity? The Historical Origins of the Methodology of Latent Fingerprint Identification*, 12 *SCI. CONTEXT* 139, 144 (1999). Whatever the merits of this argument for latent print examiners, it does not help the court performing a *Frye* inquiry. The inquiry would still be hampered by the absence of a relevant scientific community, which would legitimate latent print examiners' scientific claims in the way that professional astronomers legitimate the claims of talented amateur astronomers.

It might perhaps be argued that latent print examiners constitute a credential-less scientific community that lacks affirmation from a conventionally credentialed scientific community. The trouble is, it is not clear how a court would distinguish such a community from, say, a community of like-minded practitioners united by financial interest in the perpetuation of their technique and the deluded belief that the technique "works." I am not suggesting here that latent print examiners are one type of community or the other; merely that a proper *Frye* inquiry requires a heuristic for distinguishing one from the other.

<sup>102</sup> . This is an old problem in the sociology of knowledge. See *generally*, JÜRGEN HABERMAS, *KNOWLEDGE AND HUMAN INTERESTS* (1971).

<sup>103</sup> . *People v. Young*, 391 N.W.2d 270, 275 (Mich. 1986) ("A certain degree of 'interest' must be tolerated if scientists familiar with the theory and practice of a new technique are to testify at all.").

<sup>104</sup> . Bert Black, *A Unified Theory of Scientific Evidence*, 56 *FORDHAM L. REV.* 595, 633 (1988).

<sup>105</sup> .

<sup>106</sup> . *Id.* at 207; see also Jay P. Kesan, *A Critical Examination of the Post-Daubert Scientific Evidence Landscape*, 52 *FOOD & DRUG L.J.* 225, 240

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regard to latent print individualization, Professor Mnookin notes, "[w]hen there is challenge to the fundamental reliability of a technique through which the practitioners make their living, there is good reason to be especially dubious about 'general acceptance'" in that community.<sup>107</sup>

As three evidence scholars note in a prominent treatise:

[A] practitioner-only rule could leave an entire field largely immune from appropriate criticism. The practice of handwriting analysis, for example, is conducted by those who believe in it. The only plausible experts who can testify critically on the reliability of handwriting analysis are analysts who have developed second thoughts, the few academics who have conducted experimental studies of handwriting analysis, or the potentially greater number of academics who have studied the literature on the validity of handwriting analysis.<sup>108</sup>

Instead, they suggest, "[a] requirement of acceptance among 'disinterested scientists' helps ensure that the community in which acceptance is determined consists of more than a handful of devotees of the theory or technique in question."<sup>109</sup> Elsewhere, they note:

Constricting the scientific community to forensic scientists is not an adequate solution. As a formal matter, it resolves the problem of applying the general acceptance test to "forensic-only" evidence, but this limited acceptance does not necessarily demonstrate that the scientific theories or techniques can be relied on in court. . . . [I]f crime laboratories adopt a method before it has been adequately validated, this version of "general acceptance" will not detect the gap in the scientific foundation for the expert testimony.<sup>110</sup>

## 2. Breadth

"Cases of extra-disciplinary competition of credentialed experts" may also arise when a specialist community "accepts" a principle or technique, but the broader

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(1997) ("[T]he technique always will be deemed reliable and valid if the inquiry is limited to practitioners of the technique.").

<sup>107</sup> . Mnookin, *Fingerprint Evidence*, *supra* note 35, at 63.

<sup>108</sup> . Everything in this passage applies equally well to latent print evidence, with the exception that the number of academics who have conducted experimental studies may be even smaller.

<sup>109</sup> . *Id.* at 180. I would suggest that there is no good reason to think that the principle would not still apply even if, as in the case of latent print evidence, the devotees number more than a "handful."

<sup>110</sup> . *Id.* at 443.

community is less convinced.<sup>111</sup> Sociologists of science have shown that it is not uncommon for a small community close to a particular problem to have a different consensus view than the broader disciplinary community more conceptually distant from a problem.<sup>112</sup> For example, the state of "general acceptance" of certain scientific knowledge claims would be quite different among physicists who work with gravity-wave detectors than among physicists in general. Both groups are undoubtedly "scientific communities," and they may be equally well credentialed. But the state of general acceptance would be quite different depending on how narrowly or broadly the "relevant scientific community" is defined.

We might call this "the problem of breadth." How broadly should the "relevant scientific community" be defined? In the above example, is the "relevant scientific community" for claims about gravity waves, gravity-wave physicists, experimental physicists, all physicists, or even all scientists? Conceptually, the problem of breadth is a difficult problem. Narrow definitions of community have the virtue of capturing a community in which most members will have a high degree of familiarity with and knowledge about the claim in question. But such communities will also have the vice of consisting of members who are more likely to have an entrenched or vested interest, whether financial or emotional, in the claims in question. Likewise, a broad community will have the virtue of a community of individuals with little vested interest in the problem. But such a community may not have the depth of familiarity as the narrower community.

The courts, however, have not found the problem of breadth all that conceptually difficult. Instead, virtually all courts have articulated a preference construing the "relevant scientific community" broadly, rather than narrowly. The courts' rationale appears to be implicitly based on the idea, commonly espoused by sociologists and philosophers of science, that unfettered criticism is necessary to produce robust knowledge.<sup>113</sup> The courts

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<sup>111</sup> . Brewer, *supra* note 6, at 1633.

<sup>112</sup> . H. M. Collins, *Certainty and the Public Understanding of Science: Science on Television*, 17 SOC. STUD. SCI. 689, 692 (1987).

<sup>113</sup> . ROBERT K. MERTON, SOCIAL THEORY AND SOCIAL STRUCTURE: TOWARD THE

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appear to recognize that breadth is necessary to generate what the Florida Supreme Court described as "the kind of searching, critical review that is the *sine qua non* of scientific acceptance."<sup>114</sup> For example, California's case adopting the *Frye* rule, *People v. Kelly*, noted, "[i]deally, resolution of the general acceptance issue would require consideration of the views of a typical cross-section of the scientific community, including representatives, if there are such, of those who oppose or question the new technique."<sup>115</sup>

The Supreme Judicial Court of Massachusetts, in a similar fashion, asserted that the "relevant scientific community must be defined broadly enough to include a sufficiently broad sample of scientists so that the possibility of disagreement exists."<sup>116</sup> The court cautioned trial judges not to "define the 'relevant scientific community' so narrowly that the expert's opinion will inevitably be considered generally accepted."<sup>117</sup> In *People v. Watson*, the Appellate Court of Illinois agreed with the trial court opinion "that too narrow a definition of the pertinent scientific community would render the *Frye* standard meaningless and ineffective."<sup>118</sup> The Appellate Court added, "[w]e have found overwhelming support for this view in the decisions of other courts which have confronted this issue."<sup>119</sup> In

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CODIFICATION OF THEORY AND RESEARCH (1949); KARL POPPER, *CONJECTURES AND REFUTATIONS* 29 (1965).

<sup>114</sup> . *Ramirez v. State*, 810 So. 2d 836, 850 (Fla. 2001).

<sup>115</sup> . *People v. Kelly*, 549 P.2d 1240, 1248 (Cal. 1976).

<sup>116</sup> . *Canavan's Case*, 733 N.E.2d 1042, 1050 n.6 (Mass. 2000); see also *Bernardoni v. Industrial Com'n*, 840 N.E.2d 300, 311 (Ill. App. Ct. 2005) ("A court must not define the relevant field of experts so narrowly that the expert's opinion inevitably will be considered generally accepted. If the community is defined to include only those experts who subscribe to the same beliefs as the testifying expert, the opinion always will be admissible. The community of experts must include a sufficiently broad sample of experts so that the possibility of disagreement exists.")

<sup>117</sup> . *Canavan's Case*, 733 N.E.2d at 1050 n.6.

<sup>118</sup> . *People v. Watson*, 629 N.E.2d 634, 641 (Ill. App. Ct. 1994).

<sup>119</sup> . *Id.* (citing *State v. Bible*, 858 P.2d 1152 (Ariz. 1993); *People v. Barney*, 10 Cal. Rptr. 2d 731 (Cal. Ct. App. 1992); *People v. Pizarro*, 12 Cal. Rptr. 2d 436 (Cal. Ct. App. 1992); *Fishback v. People*, 851 P.2d 884 (Colo. 1993); *Lipscomb*, 574 N.E.2d 1345 (Ill. App. Ct. 1991); *Commonwealth v. Lanigan*, 413 Mass. 154, 596 N.E.2d 311 (1992); *State v. Vandebogart*, 616 A.2d 483 (N.H. 1992); *People v. Mohit*, 579 N.Y.S.2d 990 (N.Y. Co. Ct. 1992); *United States v. Yee*, 134 F.R.D. 161 (N.D. Ohio 1991)).

*United States v. Porter*, the District of Columbia Court of Appeals deemed "somewhat astonishing" the government's proposal that the trial judge "severely restrict the categories of scientists whose views he should consider in assessing general acceptance."<sup>120</sup> The court voiced agreement with the trial judge's conclusion that "[i]t simply is not creditable to argue . . . that general acceptance may be premised simply on the opinion of forensic scientists."<sup>121</sup>

Perhaps most significant is the absence of any opinions in which courts take the opposite view—that the "relevant scientific community" *should* be narrowly defined. There are two major categories of exceptions to the trend toward broad construal of the *Frye* test: voice spectrography and deoxyribonucleic acid (DNA).<sup>122</sup> In both categories, there are numerous cases in which courts have upheld the admissibility of evidence by narrowly construing the "relevant scientific community." In the DNA cases, the government typically urged courts to define the "relevant scientific community" as those who practice the technique in a forensic context; whereas defendants typically argued that the "relevant scientific community" should include scientists who used DNA profiling techniques in academic research. The government generally would argue that the "relevant scientific community" consisted of those individuals who performed actual forensic work, whereas defendants would argue that researchers who used DNA profiling techniques in their line of work were well equipped to evaluate the use of the same techniques in forensics. Both expert communities were scientists, but one derived its authority from its experience in the trenches doing forensic work, whereas the other derived its authority from more traditional markers of academic prestige. In addition, issues arose in forensic applications, for which academic research provided no relevant experience. For example, the

<sup>120</sup> . *United States v. Porter*, 618 A.2d 629, 634 (D.C. 1992).

<sup>121</sup> . *Id.*

<sup>122</sup> . An oft-cited "exception" to the principle of breadth, *People v. Williams*, 164 Cal. App. 2d Supp. 858 (Cal. App. Dep't Super. Ct. 1958), is not really an exception. In *Williams*, the broader community was simply ignorant of the test under consideration. That differs from the case of latent print individual-ization, in which members of the broader community are aware of, and do not accept, the claim of the validity of latent print individualization.

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issue of calculating the probative value of a DNA "match" was one that arose in the forensic context, but not in the academic research context; academic experts would first need to educate themselves in order to comment on forensic applications of the technique. The crucial issue was whether the court circumscribed the "relevant scientific community" narrowly, as those who practice *forensic* DNA profiling, or more broadly, as those who generally practice DNA profiling techniques. This decision ultimately determined the outcome of the *Frye* inquiry.<sup>123</sup>

In voice spectrography cases, Professors Faigman et al. have shown that the scope of the "relevant scientific community" determined the outcome of *Frye* rulings; all courts that construed *Frye* broadly and used no other test excluded the evidence, while all courts that construed it narrowly admitted it.<sup>124</sup> However, although the courts construed the "relevant scientific community" narrowly in some cases, in none of them did the court defend narrowness as a principle.<sup>125</sup> This stands in marked contrast to the voice spectrography cases in which the "relevant scientific community" was construed broadly. In these cases, the courts were able to eloquently articulate the virtues of breadth.<sup>126</sup> The conclusion perhaps is that narrowness conveys virtues of outcome, but not of principle.

Legal scholars also support the principle of breadth. One legal commentator has recommended, "[w]here only proponents of a technique appear, the court should sua

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<sup>123</sup> . See generally Saul Halfon, *Collecting, Testing and Convincing: Forensic DNA Experts in the Courts*, 28 Soc. Stud. Sci. 801 (1998); Schwartz, *supra* note 33. Interestingly, in the earliest cases, it was the government that construed the relevant scientific community broadly, bringing in high-powered academic scientists like Kenneth Kidd of Yale University and Richard Roberts. Only when criminal defendants began recruiting equally high-powered scientists from the academic community, like Richard Lewontin of Harvard University and Eric Lander of MIT, did the government seek to narrow the definition of the community. See JAY D. ARONSON, *GENETIC WITNESS: SCIENCE, LAW, AND CONTROVERSY IN THE MAKING OF DNA PROFILING* (2007).

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<sup>125</sup> . See *Hodo v. Superior Court, Riverside County*, 30 Cal. App. 3d 778 (Cal. Ct. App. 1973); *United States v. Maivia*, 728 F. Supp. 1471 (D. Haw. 1990); *Commonwealth v. Lykus*, 327 N.E.2d 671 (Mass. 1975); *People v. Bein*, 453 N.Y.S.2d 343 (N.Y. Sup. Ct. 1982).

<sup>126</sup> . See, e.g., *Reed v. State*, 391 A.2d 364 (Md. 1978).

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sponte take the responsibility of inquiring not just whether the experts believe the scientific community is generally in agreement, but whether they are in fact aware of any opposing sentiment in the relevant scientific community."<sup>127</sup>

#### B. MEASURING "GENERAL ACCEPTANCE"

Even if the "relevant scientific community" is defined, a second major ambiguity in the *Frye* rule remains. How should "general acceptance" be measured within that community? Must there be unanimous acceptance within the "relevant scientific community"? Will a simple majority do, or should some sort of supermajority be required? Should all voices be weighted equally, or should some sort of differential weighting be applied? If the latter, how should weight be accorded? By academic prestige? By familiarity with the specific question at hand? Or, should the opposite principle hold? Perhaps the greater the individual's professional distance from the question at hand, the *greater* weight their opinion should be afforded, on the reasoning that they have the least interest in the outcome of the *Frye* analysis.

Although these are thorny questions, courts have not had that much difficulty working out some general practical parameters. For example, numerous courts have taken the trouble to refute the red herring that general acceptance is understood as *unanimous* acceptance in the "relevant scientific community."<sup>128</sup> But how much general acceptance is required and how it should be measured remain open questions. Some courts criticize what they call "nose counting" or "head counting," a sort of crude counting of implicitly expressed "votes" in the "relevant scientific

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<sup>127</sup> . Comment, *The Voiceprint Dilemma: Should Voices be Seen and not Heard?*, 35 MD. L. REV. 267, 293 (1975); see also James P. Flannery, Kara Howe, & Blanca Dominguez, *Frye, Daubert, Donaldson, and Junk Science: The Admissibility of Novel Scientific Evidence in Illinois*, CBA REC., May 2004, at 30, 37 (stating that "narrowing the pertinent field too much would render the *Frye* test meaningless and ineffective").

<sup>128</sup> . See, e.g., *Kaelbel Wholesale, Inc. v. Soderstrom*, 785 So. 2d 539, 546 (Fla. Dist. Ct. App. 2001); *In re K.T.*, 836 N.E.2d 769 (Ill. App. Ct. 2005); *In re Commitment of Bushong*, 815 N.E.2d 103 (Ill. App. Ct. 2004); *People v. Dalcollo*, 669 N.E.2d 378, 387 (Ill. App. Ct. 1996); *Clemons v. State*, 896 A.2d 1059 (Md. 2006); *Barmeyer v. Montana Power Co.*, 657 P.2d 594 (Mont. 1983); *State v. Whitley*, 821 A.2d 1086 (N.H. 2003).



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community."<sup>129</sup> For some courts, nose counting is problematic because it entails weighing all opinions equally, rather than affording greater weight to the more qualified.<sup>130</sup> Others argue that waiting for a sufficient "nose count" will delay acceptance of cutting edge science.<sup>131</sup> Some courts have criticized "nose counting" in the process of arguing that a forgiving relevancy test is preferable to the *Frye* test.<sup>132</sup> Others have criticized it in the process of arguing for their own idiosyncratic admissibility standards.<sup>133</sup> Still other have criticized "nose counting" in the process of calling for a *Daubert*-like reliability inquiry to replace the *Frye* test.<sup>134</sup>

Still, some courts and scholars have defended "nose counting."<sup>135</sup> Of all the arguments against "nose counting" surveyed above, only the first does not entail the rejection of *Frye* itself. Therefore, to a court that *does* subscribe to *Frye*, the only relevant criticism is the first: the *Leahy* court's caution that not all votes should necessarily be counted equally. This would require some sort of weighting solution. The *Leahy* court argues that weighing should be operationalized by insisting that the court "must consider the quality, as well as quantity, of the evidence supporting or opposing a new scientific technique."<sup>136</sup> This notion of "quality" appears to be something akin to scientific credentials or even prestige. In *Leahy* it was used to require

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<sup>129</sup> . *People v. Leahy*, 882 P.2d at 336-37 (Cal. 1994); *People v. Marlow*, 41 Cal. Rptr. 2d 5, 31 (Cal. Ct. App. 1995); *Kaelbel Wholesale, Inc.*, 785 So.2d at 546; *Brim v. State*, 695 So. 2d 268, 272 (Fla. 1997).

<sup>130</sup> . *Leahy*, 882 P.2d at 336-37; *Marlow*, 41 Cal. Rptr. 2d at 31; *Kaelbel Wholesale, Inc.*, 785 So. 2d at 546; *Brim*, 695 So. 2d at 272.

<sup>131</sup> . *State v. Alberico*, 861 P.2d 192, 201 (N.M. 1993).

<sup>132</sup> . *Andrews v. State*, 533 So. 2d 841 (Fla. App. 5 Dist. 1988); *Taylor v. State*, 889 P.2d 319 (Okla. Cr. 1995); *Springfield v. State*, 860 P.2d 435 (Wyo. 1993); *State v. Williams*, 446 N.E.2d 444, 448 (Ohio 1983). In the case of *Andrews*, the call for a "relevancy" test would appear to be overruled by Florida Supreme Court cases endorsing the *Frye* test.

<sup>133</sup> . *Harper v. State*, 292 S.E.2d 389, 395 (Ga. 1982).

<sup>134</sup> . *United States v. Downing*, 753 F.2d 1224, 1238 (3d Cir. 1985).

<sup>135</sup> . *Jones v. United States*, 548 A.2d 35, 42 (D.C. 1988) (affirming "the focus is primarily on counting scientists' votes, rather than on verifying the soundness of a scientific conclusion,"); , at 222 ("Instead of evaluating various scientists' opinions, a court is only to count numbers of scientists within a relevant community who do or do not accept a theory or technique.")

<sup>136</sup> . *People v. Leahy*, 882 P.2d 321, 336-37 (Cal. 1994).

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more than the testimony of police practitioners to deem the test at issue to be valid.<sup>137</sup> This is not really a criticism of nose counting, but merely a honing of it. Indeed, it is hard to escape the conclusion that *Frye* necessarily requires some sort of polling of the scientific community if any sort of general acceptance is to be ascertained. In any case, none of these concerns will pose any difficulty for a general acceptance analysis of latent print individualization since both crude nose counting and weighted prestige counts will yield the same result.

### III. ANALYSIS OF LATENT PRINT EVIDENCE UNDER *FRYE*

It is established that for most *Frye*-subscribing courts, the "relevant scientific community" should be construed broadly and should not consist solely of practitioners, if at all. It is also established that *Frye* necessarily entails some sort of polling of the views of that "relevant scientific community"—call it nose counting if you wish. How do these principles apply to latent print evidence? The first question is: Who constitutes the "relevant scientific community" for the claim of latent print individualization?

#### A. CONSTITUTING THE "RELEVANT SCIENTIFIC COMMUNITY"

Historically, it has been assumed that latent print examiners constitute the "relevant scientific community," and latent print individualization enjoys high, perhaps even unanimous, "acceptance" in this community. However, as discussed above, for other forms of evidence, courts have generally held inadmissible types of evidence accepted only among practitioners of the technique. Admittedly, latent print practitioners are more numerous than polygraphers or voice spectrographers. However, as one court stated, "Mere numerical majority support or opposition by persons minimally qualified to state an authoritative opinion is of little value."<sup>138</sup>

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<sup>137</sup> . *Id.*

<sup>138</sup> . *Id.*; see also *People v. Marlow*, 41 Cal. Rptr. 2d 5, 31 (Cal. Ct. App. 1995) ("[T]he trial court (and the appellate court on de novo review) must not simply count heads but must look to the quality as well as the quantity of evidence supporting or opposing a new scientific method.").

1. Why Latent Print Practitioners Cannot Constitute the "Relevant Scientific Community"

How can latent print examiners be viewed as "minimally qualified to state an authoritative opinion"<sup>139</sup> on the validity of latent print individualization? This statement may seem counterintuitive to some, but it can be easily understood by considering the difference between practicing a technique and assessing the validity of that technique. The question before the court in a *Frye* proceeding is whether the "proposition" has passed from the "experimental" to the "demonstrable stage."<sup>140</sup> In other words: has the correctness of the proposition been demonstrated? Knowing whether latent print examiners can in fact do what they claim to be able to do requires performing what is generally called a "validation study." A validation study measures the rate at which latent print examiners achieve accurate results.<sup>141</sup> Validation is a common process in the sciences by which the ability of a test or assay to achieve accurate results is measured. Scientists in a wide variety of disciplines are trained to assess whether instruments of various types are valid. It is important to note that practicing a technique does not constitute validating it. Indeed, one can practice a technique without even being aware of whether or not it is valid. Moreover, no amount of day-to-day practice can inform the practitioner of the validity of the technique. A practitioner cannot "experience" validity. Validity must be measured, usually through a study.

Latent print examiners, however, normally undergo no such training. Latent print examiners are trained to analyze latent prints. They are not trained to conduct validation studies, or to perform literature reviews in order to assess whether validation studies have been conducted. Most latent print examiners have little scientific education and cannot reasonably be expected to understand validation, conduct such a study, or to assess the quality of a

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<sup>139</sup> . Leahy, 882 P.2d at 337.

<sup>140</sup> . *Id.* at 340.

<sup>141</sup> . 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, 759-60 (2003).

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purported validation study. To be sure, this situation is changing with the entry of more young trainees with scientific training into the profession, and there are a few latent print examiners who are very familiar with validation studies.<sup>142</sup> Nevertheless, even these exceptions do not make the community a good arbiter of whether latent print individualization has been validated. Because their business is analyzing prints and not conducting or assessing validation studies, the fact that thousands of latent print examiners accept latent print individualization is of little value.<sup>143</sup> Evidence scholars suggest that individuals indifferent to validation cannot properly constitute the "relevant scientific community"—"The emphasis should be on *scientists* . . . If the general acceptance standard is to fulfill its objectives, the theory and technology that generate the evidence must be familiar to a community of experts who rarely embrace methods that have not been rigorously validated."<sup>144</sup>

Professor Jonakait's characterization of electrophoresis, a more technical form of forensic evidence, is equally, if not more applicable to latent print evidence:

A forensic procedure becomes widespread not because all the people using it have made independent evaluations about reliability. . . . If the new technique appears to work, then the methods are taught to others. Since few labs can afford to employ only highly trained scientists, often those learning the procedures are not scientists, but technicians. Thus, most of those who use the new tests have not verified the test's reliability, and few would have the training to conduct such research in any event. The users of the procedure, then, trust that the procedure is reliable, not because they have verified that fact but because the developers of the procedure say that it is reliable. Widespread use of electrophoretic tests in forensic labs thus does not indicate anything more about reliability than that a handful of people have

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<sup>142</sup> . Such as Glenn Langenburg of the Minnesota Bureau of Criminal Apprehension, who has a degree in chemistry and experience in the chemical industry including validation studies. See Trial Transcript, *State v. Columbus*, No. 04082599 (Minn. Hennepin County Dist. Ct. May 18, 2006) (on file with the author).

<sup>143</sup> . See ("[A] technician's testimony should never suffice to establish the validity of a novel technique."). There is no logical reason that Professor Giannelli's statement should be limited to "novel" techniques, and he may have only used it because of the *Frye's* novelty requirement. On the novelty requirement, see *infra* text accompanying notes 255–268.

<sup>144</sup> .

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attested to their reliability.<sup>145</sup>

In fact, latent print practitioners would be the *subjects* of properly conducted validation studies of the latent print individualization. It is *their* accuracy that would be measured. Allowing latent print examiners to constitute the "relevant scientific community" would be to confuse the confidence of a practitioner in the validity of her own practice with validity as assessed by an outside observer. Latent print examiners' confidence in the validity of their own practice is not meaningful because latent print examiners do not receive valid feedback in going about the course of their work. They are not regularly told whether they have reached correct results for the simple reason that in casework the correct results are not known to anyone. At best, a latent print examiner might sometimes receive feedback from a peer who disagrees with their conclusion. But if, for example, an error were corroborated rather than detected by the peer, neither individual would necessarily receive valid feedback about the error.<sup>146</sup> Because of this lack of valid feedback, the examiners are in no position to assess the accuracy of their own practice. Indeed, if asked to assess accuracy, they are very likely to confuse their own confidence with actual validity. Much the same point has been made about polygraph examiners: "[P]olygraph examiners are perhaps the group whose opinions concerning the technique are, paradoxically, of the least value."<sup>147</sup> If relevant scientific communities were construed merely as practitioners, then astrologers would constitute the "relevant scientific community" that "accepts" astrology as valid and wine tasters would constitute the "relevant scientific community" for assessing the ability of wine tasters to accurately identify vintages and types of wine.

It is clear then, that if the question is the validity of the

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<sup>145</sup> . Randolph Jonakait, *Will Blood Tell? Genetic Markers in Criminal Cases*, 31 EMORY L. J. 833, 860-61 (1982).

<sup>146</sup> . For examples of actual cases in which errors were corroborated, rather than detected, see Simon A. Cole, *More Than Zero: Accounting for Error in Latent Fingerprint Identification*, 95 J. CRIM. L. & CRIMINOLOGY 985, 1023-1025 (2005).

<sup>147</sup> . William G. Iacono & David Lykken, *The Scientific Status of Research on Polygraph Techniques: The Case Against Polygraph Tests*, in SCIENCE IN THE LAW: FORENSIC SCIENCE ISSUES 609, 618 (Faigman et al. eds., 2002).

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technique, the proper scientific community is those equipped to assess validity, not those equipped to *practice* the technique. Indeed, as several courts have noted, the very purpose of the *Frye* rule is to "ensure[] that the persons most qualified to assess scientific validity of a technique have the determinative voice."<sup>148</sup> The Pennsylvania Supreme Court aptly described the virtue of *Frye* in the following manner: it "require[es] judges to pay deference to the conclusions of those who are in the best position to evaluate the merits of scientific theory and technique when ruling on the admissibility of scientific proof . . . ."<sup>149</sup> This, the court argues, "is the better way of insuring that only reliable expert scientific evidence is admitted at trial."<sup>150</sup>

In addition, courts have expressed concern about practitioners' interests, financial and institutional, and these concerns apply with force to latent print examiners. If latent print individualization evidence were not generally accepted, it would no longer be admissible in court. Most latent print examiners would probably be out of a job. But latent print examiners' emotional investment is probably even greater than their pecuniary interest. The extraordinary claims of "infallibility"<sup>151</sup> or "total reliability"<sup>152</sup> that today still surrounds latent print individualization, as distinct from all other areas of forensic science, make the idea that the technique lacks validation particularly difficult for practitioners to accept. To accept scientists' arguments that latent print individualization lacks validation may imply that latent print examiners had been perpetrating a fraud, or at least an exaggeration. It is not unreasonable to think that a latent print examiner who had devoted her career to this practice would have great difficulty taking such a step. This difficulty may be demonstrated by the rather visceral

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<sup>148</sup> . Goeb v. Tharaldson, 615 N.W.2d 800, 813 (Minn. 2000); Grady v. Frito-Lay, Inc., 789 A.2d 735, 743 (Pa. Super. Ct. 2001) (overturned on other grounds).

<sup>149</sup> . Grady v. Frito-Lay, Inc., 839 A.2d at 1045.

<sup>150</sup> . *Id.*

<sup>151</sup> . FEDERAL BUREAU OF INVESTIGATION, THE SCIENCE OF FINGERPRINTS: CLASSIFICATION AND USES, at iv (1985).

<sup>152</sup> . Scientific Working Group on Friction Ridge Analysis Study and Technology, Press Kit, May 18, 2004, [http://www.swgfast.org/swgfast\\_press\\_kit\\_may04.html](http://www.swgfast.org/swgfast_press_kit_may04.html).

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reaction in the latent print community to scientists' argument that the technique lacks validation.<sup>153</sup>

The California Supreme Court wrote that to establish reliability and general acceptance, "[t]he witness must have academic and professional credentials which equip him to understand both the scientific principles involved and any differences of view on their reliability. He must also be 'impartial,' that is, not so personally invested in establishing the technique's acceptance that he might not be objective about disagreements within the relevant scientific community."<sup>154</sup> Latent print examiners violate both criteria; they fail to understand the scientific principles that have led to differences of view about the reliability of latent print individualization. This is evidenced by their consistent mustering of irrelevant arguments concerning the uniqueness of friction ridge skin, or the use of latent prints in casework in response to questions about the reliability of latent print individualization.<sup>155</sup> And, as already noted, they are not impartial.

#### B. MEASURING "GENERAL ACCEPTANCE"

How is a court to assess the views of the "relevant scientific community" once it determines who makes up that community? One tempting possibility would be to somehow survey that community.<sup>156</sup> There is actually some precedent for such an approach. Several surveys have been conducted explicitly for the purpose of assessing the general acceptance of the polygraph and social framework testimony about eyewitness identification.<sup>157</sup> Is there such a

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<sup>153</sup> . See, e.g., ANDRÉ MOENSSENS, *THE RELIABILITY OF FINGERPRINT IDENTIFICATION: A CASE REPORT* (2002), available at <http://www.forensic-evidence.com/site/ID/pollak2002.html>; David L. Grieve, *Rocking the Cradle*, 49 J. FORENSIC IDENTIFICATION 719 (1999).

<sup>154</sup> . *People v. Brown*, 40 Cal. 3d 512, 530 (Cal. 1985).

<sup>155</sup> . See Cole, *Fingerprint Identification*, *supra* note 35.

<sup>156</sup> .

<sup>157</sup> . Not surprisingly, proponents and opponents of polygraphy were able to archive diametrically opposed results in their surveys based on the way in which they constructed the relevant scientific community. ALDER, *supra* note 70, at 256; ; Saul M. Kassin et al., *The "General Acceptance" of Psychological Research on Eyewitness Testimony*, 44 AM. PSYCHOLOGIST 1089, 1096 (1989) (arguing that "for assessing the consensus of opinion on various eyewitness findings" the survey method "is the only plausible method and is far better than other means of establishing

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survey assessing the state of general acceptance of latent  
print individualization?

### 1. Formal Surveys

As it turns out, one such survey has been conducted for latent print individualization.<sup>158</sup> The survey was conducted by the Federal Bureau of Investigation (FBI) in preparation for the first *Daubert* challenge to latent print evidence in 1999.<sup>159</sup> The cover letter makes clear that the survey was meant, at least in part, to address the general acceptance prong of *Daubert*.<sup>160</sup> The survey did not directly ask whether the respondents generally accept latent print individualization, but it did ask, in Question #A7, "Does your agency accept the fundamental principles of uniqueness and permanence as scientific basis [sic] for using fingerprints as a means of individualization?"<sup>161</sup> All respondents who completed this part of the survey (forty-nine respondents) responded "yes" to this survey item.<sup>162</sup> The survey was presented as evidence of general acceptance in the *Mitchell Daubert* hearing.<sup>163</sup> The survey question should be interpreted as acceptance of the premises that make latent print individualization plausible but instead has been confused with acceptance that latent print individualization actually works. However, even if we construe the question as asking about general acceptance of the validity of latent print individualization, rather than to its premises, should the fact that one hundred percent of respondents answered "yes" to that question indicate general acceptance in the "relevant scientific community"?

The answer is probably not. In addition to the problem of indirectly focusing on acceptance of premises rather than on the process itself, the survey suffers from methodological flaws. For example, the survey might be

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general acceptance"). Saul M. Kassin et al., *On the "General Acceptance" of Eyewitness Testimony Research: A New Survey of the Experts*, 56 AM. PSYCHOLOGIST 405 (2001).

<sup>158</sup> . FBI LABORATORY, SURVEY OF LAW ENFORCEMENT AGENCY'S [sic] FINGERPRINT OPERATIONS IN SUPPORT OF A DAUBERT HEARING (1999).

<sup>159</sup> . United States v. Mitchell, 365 F.3d 215, 219 (3d Cir. 2004).

<sup>160</sup> . See *infra* note 164 and accompanying text.

<sup>161</sup> . FBI LABORATORY, *supra* note 158.

<sup>162</sup> . *Id.*

<sup>163</sup> . *Mitchell*, 365 F.3d at 241.



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biased because it included a cover letter that began as follows:

The FBI needs your immediate help! The FBI Laboratory is preparing for a *Daubert Hearing* [*sic*] on the scientific basis for fingerprints as a means of identification. The Laboratory's Forensic Analysis Section, Latent Print Unit, is coordinating this matter and supporting the Assistant United States Attorney in collecting data needed to establish this scientific basis and its universal acceptance. The overall strategy must specifically address the two fundamental principles (uniqueness and permanence) for using fingerprints to individualize. The availability of the requested information will not only provide supportive documentation but will also fulfill one of the other *Daubert elements*, i.e., that the scientific basis is widely accepted.<sup>164</sup>

The letter went on:

The time sensitive nature of these requests cannot be expressed strongly enough, nor can the importance of your cooperation. The potential impact of the Federal court not being convinced of the scientific basis for fingerprints providing individuality has far-reaching and potentially negative ramifications to everyone in law enforcement. The FBI wishes to present the strongest data available in an effort to insure success in this legal matter and your cooperation is a key component in achieving this success.<sup>165</sup>

This stimulus does not appear to be consistent with the fundamental principles of survey research. The letter makes the purpose of the study clear, the desired response clear, and threatens dire social consequences ("potentially negative ramifications") if the desired response is not provided. The letter purports that these consequences not only threaten the respondents, but innocent bystanders as well—"everyone in law enforcement"—should every respondent not give the desired response.<sup>166</sup> A further biasing effect may have been exerted by the fact that, on the crucial Question #7, respondents were asked to provide "an explanation as an attachment" if they answered "no," but not if they answered "yes." This asymmetry creates a disincentive to answer "no." These methodological

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<sup>164</sup> . FBI LABORATORY, *supra* note 158.

<sup>165</sup> . *Id.*

<sup>166</sup> . DON A. DILLMAN, *MAIL AND INTERNET SURVEYS: THE TAILORED DESIGN METHOD* 161 (2nd ed. 2007) (stating that it is inappropriate to give an "obviously biased" explanation of why the survey is being conducted that gives "the impression that the sponsor wants responses from people who have opinions that are highly supportive" of one particular position in a survey cover letter).

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problems make the mutual accusations of methodological  
flaws in the polygraph surveys look like nitpicking.<sup>167</sup>

But even these problems pale in comparison to the issue of the selection of respondents. The survey was sent to the fingerprint units of fifty-three law enforcement agencies (the fifty state police agencies, plus the police agencies of the District of Columbia, Canada, and the United Kingdom).<sup>168</sup> As a method of polling the "relevant scientific community," the selection of recipients clearly leaves something to be desired. The pool of recipients is limited to latent print examiners. As discussed *supra*, limiting the "relevant scientific community" to practitioners allows a practitioner group to "self-validate" and, as the Court noted in *Kumho Tire*, would fail to bar even astrology or necromancy from meeting the general acceptance test.<sup>169</sup> It is noteworthy that in the case of surveys designed to measure the state of general acceptance of the polygraph, even though various scientists waged a strenuous battle about how the respondent pool should be delineated, *neither* side advocated that the respondent pool be limited to practitioners (that is, polygraph operators) and exclude scientists.<sup>170</sup> The selection problem in the FBI survey makes the mutual accusations of selection bias in the polygraph debate look minimal. Similarly, the designers of the social framework testimony surveys worried about the breadth of their sample even though they did not limit their sample to "practitioners" (that is, those who give social framework expert testimony in court).<sup>171</sup>

Limiting the recipient pool to practitioners would be bad enough, but the FBI further limited the pool to current employees of law enforcement agencies. Although most practicing latent print examiners, no matter where they are employed, probably "accept" latent print individualization, those who might have doubts are more likely to express those doubts when they are no longer employed in law enforcement.<sup>172</sup> The cover letter's insinuation that a

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<sup>167</sup> . See *supra* note 157.

<sup>168</sup> . *Mitchell*, 365 F.3d at 223.

<sup>169</sup> . *Kumho Tire v. Carmichael*, 526 U.S. 137, 151 (1999).

<sup>170</sup> . See sources cited *supra* note 157.

<sup>171</sup> . Kassin, et al., *supra* note 157, at 414.

<sup>172</sup> . This point is anecdotally illustrated by the example of Mark Acree,

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negative response to the "acceptance" question would have "far-reaching and potentially negative ramifications to everyone in law enforcement"—that is, all the survey respondents' employers and colleagues—only further undermines the trustworthiness of a survey of current of law enforcement employees.

Not surprisingly, given these methodological flaws, the survey has never been published or submitted to formal peer review. Again, the contrast with the polygraph surveys is telling. The polygraph surveys (criticized for lack of scientific peer review<sup>173</sup>) came closer to proper scientific publication than the FBI survey, because the polygraph surveys were, respectively, published in a non-refereed journal and submitted as a successful master's thesis in psychology.<sup>174</sup>

One would think that courts would be concerned by so poor a survey that so clearly seeks to limit the relevant community to practitioners. But, to the contrary, courts have accepted this survey without any qualms. In *Mitchell*, the case in which the survey was first introduced, the Third Circuit ruled that latent print individualization clearly met the general acceptance prong of *Daubert* because of "the results of the FBI's survey of state agencies."<sup>175</sup> In response to *Mitchell's* argument that law enforcement latent print examiners did not constitute the "relevant scientific community," the court drew on *Kumho Tire* to argue that "the scientific/nonscientific distinction is irrelevant."<sup>176</sup> But *Kumho Tire* renders the scientific/ nonscientific distinction irrelevant for purposes of applying *Daubert*. That is, *Kumho* applied *Daubert* to all expert evidence. There is nothing in *Kumho Tire* that justifies the *exclusion* of scientists from the general acceptance analysis. Moreover, even if *Kumho* renders the scientific/nonscientific distinction irrelevant, the problems with the FBI's constitution of the "relevant scientific community" are greater than the mere fact that

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who identified latent prints for the FBI. Upon leaving the FBI, he has now publicly expressed his doubts concerning the validity of latent print individualization. See *infra* notes 269–276 and accompanying text.

<sup>173</sup> .

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<sup>175</sup> . United States v. Mitchell, 365 F.3d 215, 241 (3d Cir. 2004).

<sup>176</sup> . *Id.*

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law enforcement latent print examiners are not scientists. There are also the problems that the FBI's "community" is composed too narrowly and entirely of interested parties.

The FBI survey also carried great weight with the Supreme Judicial Court of Massachusetts in *Commonwealth v. Patterson*.<sup>177</sup> The court found, "[t]his survey is a sufficient basis on which the judge could have concluded there to be general acceptance of the theory in the fingerprint examiner community."<sup>178</sup> Interestingly, the court made this finding in a decision that found *inadmissible* a special application of latent print individualization called simultaneous impressions. The court found that simultaneous impressions lacked general acceptance in part because of the absence of a survey like the FBI's. Although an FBI Latent Print Unit Chief testified that simultaneous impressions were "generally accepted in the community of qualified fingerprint examiners," the court noted that, "[u]nlike his testimony in the single impression context, however, Agent [sic] Meagher's testimony is conclusory and unsupported by any evidence, let alone an *extensive multi-jurisdictional survey*."<sup>179</sup>

Although the court was correct to find that simultaneous impressions lack general acceptance, the opinion, by describing the FBI's fifty-three-respondent,

<sup>177</sup> . *Commonwealth v. Patterson*, 840 N.E.2d 12, 24 (Mass. 2005).

<sup>178</sup> . *Id.*

<sup>179</sup> . *Id.* at 29 (emphasis added). But see Steve Ostrowski, *Simultaneous Impressions: Revisiting the Controversy*, WEEKLY DETAIL, Nov. 5, 2001, <http://www.clpex.com/Articles/TheDetail/1-99/TheDetail13.htm>. Interestingly, a survey concerning the general acceptance of simultaneous impressions did exist at the time of *Patterson*, but the government inexplicably failed to enter it into evidence. The *Patterson* court noted that it would have found the survey unconvincing even if it were introduced into evidence because the sample was smaller than that of the FBI study (n=18, versus n=49) and it found only moderate acceptance of simultaneous impressions (ten out of eighteen respondents).

At the time of Ostrowski's (non-peer reviewed) survey, no empirical studies measuring the ability of latent print examiners to identify correctly simultaneous impressions existed. (One study has since been published. See John P. Black, *Pilot Study: The Application of ACE-V to Simultaneous (Cluster) Impressions*, 56 J. FORENSIC IDENTIFICATION 933 (2006). Given that, even under these circumstances, more than half (10/18) of latent print examiners were willing to "accept" simultaneous impressions illustrates the hazards of allowing practitioners, rather than scientists, to constitute the relevant scientific community.

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methodologically flawed, poorly worded survey of a highly biased sample in glowing terms ("an extensive multi-jurisdictional survey"), sets an extremely low bar for survey-based evidence of general acceptance.<sup>180</sup> Essentially, the opinion invites the government to satisfy the admissibility threshold for simultaneous impressions by conducting a survey of fifty some-odd law enforcement agents. Such an interpretation of the *Frye* rule would allow the government to claim general acceptance of any testimonial claim simply by conducting a survey of state law enforcement laboratories. This is a far cry from the original *Frye* case in which the court insisted that Marston win acceptance from his psychologist colleagues as a requirement of admissibility.

## 2. What is the "Relevant Scientific Community" for Latent Print Individualization?

If the existing survey is not satisfactory, how can we measure the state of general acceptance of latent print individualization? The problem is unusually vexing because latent print examiners appear to be a non-scientist practitioner community. Therefore, the nature of the non-practitioner "relevant scientific community" is not obvious. If a court operating under a deference model wants to avoid allowing a practitioner community to self-validate, where can it turn in an inquiry into general acceptance? The case of latent print evidence would seem to pose a new sort of problem, one which has not been addressed existing discussions about expert evidence. Legal scholars are accustomed to thinking about cases in which the contested claim has a relatively obvious *appropriate reference community*.<sup>181</sup> For example, in the *Frye* case itself, Marston was a Harvard-trained psychologist.<sup>182</sup> His claims to be able to discern whether his device accurately detected deception were rooted in his training as a psychologist. It therefore seemed appropriate for the *Frye* court to treat

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<sup>180</sup> . See

<sup>181</sup> . I am grateful to Professor Risinger, coiner of neologisms extraordinaire, for his inspiration in coining this term.

<sup>182</sup> . Henry T. Greely & Judy Illes *Neuroscience-based Lie Detection: The Urgent Need for Regulation*, 33 AM. J.L. & MED. 377, 385-86 (2007). See also ALDER, *supra* note 70.

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psychologists and physiologists as the “relevant scientific community” whose “acceptance” was required for Marston’s technique to be admissible. This formulation of the problem has persisted with regard to the lie detector ever since. Although adversaries over the admissibility of lie detectors vigorously debate how the opinions of psychologists of various qualifications should be weighed—with opponents of the lie detector emphasizing academic credentials and proponents emphasizing operational familiarity with lie detection techniques—neither side has seriously disputed the identification of psychologists in general as the relevant (or at least the primary) scientific community.

Other *Frye* inquiries have similarly managed to identify relevant scientific communities without undue difficulty. For example, courts have looked to audiologists, acousticians, speech scientists, acoustical engineers, anatomists, electrical engineers, linguists, phoneticists, physicists, physiologists, psychologists, and statisticians to constitute the “relevant scientific community” for voice spectrography.<sup>183</sup> Of course in such cases, the parties, not surprisingly, may differ as to which academic specialty is most relevant. But these are differences *between* scientific specialties, what Professor Brewer would call “extra-competition of credentialed experts.”<sup>184</sup>

A *Frye* analysis of latent print individualization poses a different issue altogether. The court is faced, not with a choice among scientific communities, but with the absence of any obviously “relevant scientific community” at all. The logic of the deference model is that the “relevant scientific community” is the group of people of whom the claimant must convince in order for the court to find her claims credible. The court does not exercise its own judgment as to the plausibility of the claim, but rather, seeks to determine whether the claimant has managed to convince the right people. Since Marston was a psychologist, the right people in his case were primarily psychologists. To be sure, Marston might have sought to convince the court that some other group ought to be considered the “relevant scientific community” for his claim, but he would probably

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<sup>183</sup> . FAIGMAN, FORENSIC SCIENCE ISSUES, *supra* note 80, at 296 n.4.

<sup>184</sup> . Brewer, *supra* note 6, at 1633.

not have been successful. This is because, first, his training was in psychology—there is a logic to requiring the claimant to convince the members of the discipline in which he was trained—and second, because the detection of deception is a scientific problem that would seem to fall within the domain of psychology.

Adopting this reasoning, it is by no means clear whom latent print examiners needed to convince. As Professors Kaye, Bernstein, and Mnookin note, "[w]ith procedures that have no application outside the courtroom . . . defining the relevant scientific field is a major obstacle to an evenhanded and predictable application of the general acceptance standard."<sup>185</sup> What the appropriate reference community should be for latent print examiners' claim to be able to individualize latent prints is not a trivial problem. Unlike Marston and other early polygraphers, latent print examiners do not emanate from an academic discipline to which a court could refer to see whether the claim has been "generally accepted," nor is it immediately apparent into which scientific discipline's domain latent print examiners' claim should fall. Plausible claims might be made for psychology, computer science, quality engineering, biology, and statistics. At this point in history, no discipline has won "jurisdiction" over the problem.<sup>186</sup> What, then, is a court to do? I suggest that the court has little choice but to look to the scholarly community as a whole and constitute an *ad hoc*, interdisciplinary *appropriate reference community* composed of those individuals who have made a reasonably informed effort to assess the issue of the validity of latent print examiners' claim to be able to individualize from latent prints. The court might call these scholars *meta-experts*, experts able to evaluate the expert knowledge claims of other experts.<sup>187</sup> It would appear then that a court

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<sup>186</sup> . ANDREW ABBOTT, *THE SYSTEM OF PROFESSIONS: AN ESSAY ON THE DIVISION OF EXPERT LABOR* (1988).

<sup>187</sup> . The term "meta-expert" is also used by Professor Brewer, *supra* note 6, at 1627. However, Professor Brewer posits a different sort of meta-expert to solve a less difficult problem. Professor Brewer's meta-experts help a judge choose among competing experts "in a given area." My meta-experts are deployed to solve a more difficult situation in which non-scientist expert practitioners (who give testimony that purports to be scientific) make claims that require evaluation by experts. My notion of the meta-expert is also inspired in part by Professors Collins and Evans's

undertaking a *Frye* inquiry would need to look not to a specific scientific discipline, such as psychology, but to the scientific community at large. It will be noted, of course, that most members of the scientific community at large do not analyze latent prints, and some readers may wonder how non-practitioners can evaluate the knowledge claims of practitioners. It is crucial to recognized that meta-experts are not evaluating latent print examiners' analyses of particular latent prints; they are evaluating the question of whether latent print individualization has been validated. This requires, not an evaluation of latent prints, but an evaluation of a *study* of the performance of latent print examiners on the task of attributing latent prints.<sup>188</sup> Similarly, in the *Frye* case itself, the court called on the "relevant scientific community" to evaluate whether proponents of the lie detector has amassed sufficient evidence to convince them that the device correctly detected deception. This evaluation did not require that the members of the "relevant scientific community" be capable of operating the device themselves. Their perceived competence lay in their ability to design and interpret

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comment that my own native discipline of Science & Technology Studies constitutes "expertise about expertise." H.M. Collins & Robert Evans, *The Third Wave of Science Studies: Studies of Expertise and Experience*, 32 SOC. STUDIES OF SCI. 235, 239 (2002).

Nonetheless, the notion of the meta-expert that I positing here does not require individuals with claims to "expertise about expertise," such as sociologists or philosophers of science. Instead, I am suggesting that any scholar who makes a reasonably informed evaluation of the empirical issue at hand would qualify as a "meta-expert."

In later work, Collins and Evans describe multiple "meta-expertises." HARRY COLLINS & ROBERT EVANS, *RETHINKING EXPERTISE* 45 (2007). Most of the meta-expertises they describe have little to do with what I am describing here because they primarily involve using social knowledge to make judgments about claims to expertise. Their notion of "downward discrimination," of which "peer review" is the best known variant, comes closest to what I am trying to capture here.

<sup>188</sup> . In the case of latent print individualization, the situation is further complicated by the fact that the government has never put forward any empirical study that it claims validates latent print individualization. Haber & Haber, *supra* note 35. Rather than critiquing some purported validation study, they are simply articulating the parameters of empirical evidence that would be necessary to support a particular knowledge and noting the absence of any such evidence. Since this amounts to proving a negative, meta-experts are not able to "prove" the absence of such a study. Instead, they can only assert that literature reviews have not revealed any such study.



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empirical studies of performance of a particular task, rather than the ability to perform the task being studied.

### 3. Polling the "Relevant Scientific Community"

A court undertaking a *Frye* inquiry in 1990 would have found no clear evidence that latent print examiners had convinced any non-practitioner scientist of the accuracy of latent print individualization.<sup>189</sup> Today however, the situation has changed. A number of scientists and legal scholars have evaluated the claims that latent print individualization is valid. Therefore, a court using a deference model can undertake to survey whether non-practitioner scientists and scholars accept the validity of latent print individualization. In what follows, this article examines three potential sources of information on the state of general acceptance of this claim in the broad scientific community: expert witness testimony, amicus curiae briefs, and published scholarly literature. All are well-recognized ways of assessing the state of general acceptance in a scientific community. In addition, all three involve acts in which an individual symbolically stakes his or her scientific or scholarly reputation on his or her opinion. In all three cases, this article examines compiled lists of "acceptors" and "non-acceptors." In doing so, this article by no means suggests that courts undertaking *Frye* inquires need always be bound by such crude head counts or that the rules for counting that it uses are the only ones that could be used. I am by no means suggesting that all matters of contested knowledge can be resolved by polling. I am, however, suggesting that such polling may be a necessary first step for a fact-finder undertaking a process of "practical epistemic deference." Polling results that contradict the fact-finders intuitions ought not merely be dismissed, but would seem to at least necessitate further inquiry. Therefore, such head counts may be suggestive as to the state of general acceptance. Moreover, there is some precedent for such an approach. Litigants have in the past

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<sup>189</sup> . Although numerous scientists have written about latent print identification, very few of them have said anything in writing about the *accuracy* of latent print identification. The few exceptions have merely asserted the accuracy of latent print identification, not supported the claim with any empirical evidence. See *infra* notes 235-238 and accompanying text.

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submitted such lists to courts charged with performing *Frye*  
analyses.<sup>190</sup>

a. Expert Testimony

Courts have suggested a variety of methods for measuring general acceptance. One is through the testimony of expert witnesses.<sup>191</sup> The first modern admissibility challenge to latent print individualization was held in *United States v. Mitchell*.<sup>192</sup> Although the hearing in *Mitchell* was governed by *Daubert*, not by *Frye*, the record can be used to assess the state of general acceptance of latent print individualization.

The government presented the testimony of seven expert witnesses. Three of the government experts were latent print examiners with no advanced scientific training, although some had engaged in a significant amount of scientific self-study.<sup>193</sup> All three were questioned as to whether "individualization, that is a positive identification, can result from comparisons of friction ridge skin or impressions containing sufficient quality (clarity) and quantity of unique friction ridge detail."<sup>194</sup> All three

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<sup>190</sup> . *United States v. Williams*, 583 F.2d 1194, 1198 (2d Cir. 1978).

<sup>191</sup> . *Harper v. State*, 292 S.E.2d 389, 395 (Ga. 1982) ("An evaluation of whether the principle has gained acceptance will often be transmitted to the trial court by members of the appropriate scientific community testifying as expert witnesses at trial."); *see also*

<sup>192</sup> . *United States v. Mitchell*, 365 F.3d 215 (3d Cir. 2004).

<sup>193</sup> . In particular, *see* DAVID R. ASHBAUGH, QUANTITATIVE-QUALITATIVE FRICTION RIDGE ANALYSIS: AN INTRODUCTION TO BASIC AND ADVANCED RIDGEOLOGY (1999). The other two witnesses were Ed German, of the U.S. Army, and Stephen Meagher, of the Federal Bureau of Investigation.

<sup>194</sup> . Trial Transcript, July 8, 1999, at 37, *United States v. Mitchell*, 365 F.3d 215 (3d Cir. 2004). The imprecise wording of this question is unfortunate. First, asking whether positive identification "can" result elides the fundamental issue of how often such conclusions of positive identifications are correct. Because of the word "can," the witnesses presumably must answer "yes," even if they believe the accuracy rate of latent print individualization to be very low. Indeed, even "non-acceptors" probably would have had to answer "yes" to this question. Second, the qualifier "containing sufficient quality (clarity) and quantity of unique friction ridge detail" presumably restricts the answer to a subset of "comparisons" in which "sufficient" quality and quantity are present. Since "sufficient" is not further specified, again, any witnesses would have to answer "yes" even if the subset of comparisons that meet this condition is extremely small.

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answered in the affirmative.<sup>195</sup> This would seem to show general acceptance of latent print individualization within the practitioner community. A fourth latent print examiner, who was called in rebuttal, had a Bachelor of Science degree, but he was not asked whether he accepted individualization.<sup>196</sup>

In addition, the government called three non-practitioner witnesses with scientific credentials. William Babler was a doctoral level anatomist.<sup>197</sup> Babler appeared to "accept" the premises given to him. However, Babler was questioned about his acceptance of the "premises" underlying latent print individualization, specifically the uniqueness and permanence of friction ridge skin, rather than about whether he accepted the validity of latent print individualization itself.<sup>198</sup> As noted above, the *Frye* inquiry must be on the technique itself, not its premises. Donald Ziesig, an engineer for Lockheed Martin, was questioned about his role in conducting a study using the Lockheed automated fingerprint matching system, not about the validity of latent print individualization.<sup>199</sup> Only one of the scientists, Bruce Budowle, a doctoral level biologist, was questioned about the validity of latent print individualization. In response to essentially the same question posed to the practitioners above, Budowle answered in the affirmative.<sup>200</sup> Thus, the government showed that latent print individualization was "accepted" by many non-scientist practitioners of the technique and by one non-practitioner scientist.

The defendant presented the testimony of three expert witnesses with scholarly credentials and varying degrees of practitioner competence. David Stoney, a doctoral level forensic scientist, was trained to analyze latent prints, but

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<sup>195</sup> . Trial Transcript, July 7, 1999, at 158-59, *Mitchell*, 365 F.3d 215 (Mr. Ashbaugh); Trial Transcript, July 8, 1999, at 37, *Mitchell*, 365 F.3d 215 (Mr. German); Trial Transcript, July 9, 1999, at 186, *Mitchell*, 365 F.3d 215 (Mr. Meagher).

<sup>196</sup> . Trial Transcript, July 13, 1999, at 53-60, *Mitchell*, 365 F.3d 215 (Mr. Wertheim).

<sup>197</sup> . Trial Transcript, July 7, 1999, at 7, *Mitchell*, 365 F.3d 215 (Dr. Babler).

<sup>198</sup> . Trial Transcript, July 7, 1999, at 74, *Mitchell*, 365 F.3d 215.

<sup>199</sup> . See *Mitchell*, 365 F.3d at 223.

<sup>200</sup> . Trial Transcript, July 9, 1999, at 141, *Mitchell*, 365 F.3d 215.

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primarily made his living in other areas, particularly  
microscopy.<sup>201</sup> James Starrs was a Professor of both Law  
and Forensic Science.<sup>202</sup> The third defense expert was the  
author of this article, who holds a doctorate in a social  
science (Science & Technology Studies). Neither Starrs nor  
the author claimed to be able to analyze latent prints. All  
three defense experts testified that latent print  
individualization had not been validated. Thus, broadly  
stated, they did not "accept" latent print individualization.

TABLE 1. General acceptance of the validity of latent print individualization among non-practitioners based on expert testimony, c. 1999.

**Acceptors**

No.	Name	Title	Affiliation	Degree	Institution	Discipline
1.	Bruce Budowle		Federal Bureau of Investigation	PhD		Biology

**Non-Acceptors**

No.	Name	Title	Affiliation	Degree	Institution	Discipline
1.	James Starrs	Professor	The George Washington University School of Law and Forensic Science Program	BA	St. John's University	English
2.	David Stoney	Director	McCrone Institute	PhD	University of California, Berkeley	Forensic Science
3.	Simon Cole	Postdoctoral Fellow	Institute for Health Care Policy, Rutgers University	PhD	Cornell University	Science & Technology Studies

The state of the scientific community at the time of the *Mitchell* hearing seems to be against admissibility under *Frye* (Table 1). Latent print individualization was self-certified by thousands of its own practitioners, but the government was able to identify only a single non-

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<sup>201</sup> . Trial Transcript, July 12, 1999, at 37, *Mitchell*, 365 F.3d 215 (Dr. Stoney).

<sup>202</sup> . *Mitchell*, 365 F.3d at 228.

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practitioner, credentialed scientist to say he "accepted" latent print individualization. Moreover, one might imagine that the fact that this single scientist happened to be an FBI employee would raise alarms for the court. In contrast, the defense was able to point to three non-practitioner scientists or scholars who did not accept the validity of latent print individualization. To be sure, the defense witnesses had weaknesses as well. The government could have pointed to Professor Starrs's lack of a doctoral degree, or the fact that the author was a social, not a natural, scientist. However, it would seem that these objections would be overcome by Starrs's position as a Professor of both Law and Forensic Science at a prestigious university and the fact that the author's training was in a social science discipline whose precise aim was to seek to understand the nature of scientific knowledge claims. In any case, it would seem difficult to see how a court would construe this lineup as "acceptance" unless it was relying on acceptance among practitioners. Only by excluding non-practitioners from the "relevant scientific community" could a court find latent print individualization generally accepted.

b. *Amicus Curiae* Briefs

One possible objection to basing a general acceptance evaluation on expert testimony is that the numbers are necessarily small. Taking expert testimony is a slow and unwieldy method of gauging the views of "relevant scientific community." A court might hesitate to rule a form of evidence, especially such a venerable form of evidence as latent print evidence, inadmissible based on the testimony of three individuals, even if they outnumbered their counterparts threefold.

One way of more efficiently getting the views of the "relevant scientific community" before the court is through *amicus curiae* briefs. By soliciting multiple signatories on a single brief, parties can convey the extent of support for a particular scientific proposition without overburdening the court with the testimony of each individual under oath. *Amicus curiae* briefs are relatively uncommon in the lower courts that have heard the majority of the admissibility challenges to latent print evidence. However, one appellate

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court, the Supreme Judicial Court of Massachusetts, did solicit amicus briefs pursuant to an interlocutory appeal of a denial of a motion to exclude latent print evidence. Although three briefs were submitted, two were submitted on behalf of legal organizations. One brief, however, is pertinent to assessing the acceptance of latent print individualization in the "relevant scientific community." This brief was submitted by the New England Innocence Project on behalf of fifteen scientists and scholars (including the author of this article).<sup>203</sup> All fifteen scientists and scholars agreed that latent print individualization lacked validation.

The composition of the amici was varied. Fourteen of the fifteen held terminal degrees (Ph.D. or J.D., several of them held both), the remaining signatory held a master's degree in Forensic Science. The amici came from a variety of disciplines: Biology, Mathematics, Statistics, Law, Political Science, Psychology, Linguistics, and Science & Technology Studies.<sup>204</sup> These disciplinary backgrounds reflect the fact, discussed above, that there is no single discipline in whose domain the problem of latent print individualization obviously falls. Contrary to common stereotypes about "the academic brief" being signed by scholars with no professional publications or knowledge about the issue at hand,<sup>205</sup> all but one of the signatories of this particular Brief have published articles or given conference presentations that deal, at least in part, with the issue of the validity of latent print individualization.<sup>206</sup> In the *Patterson* case the government was not able to identify any additional non-practitioners who "accepted" the claim of the validity of latent print individualization.

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<sup>203</sup> . David M. Siegel et al., *The Reliability of Latent Print Individualization: Brief of Amici Curiae of Scientists, Scholars, and the New England Innocence Project*, Commonwealth v. Patterson, 42 CRIM. L. BULL. 21, 21 (2006).

<sup>204</sup> . *Id.*

<sup>205</sup> . ANTONIN SCALIA & BRYAN A. GARNER, MAKING YOUR CASE: THE ART OF PERSUADING JUDGES, 104-105 (2008).

<sup>206</sup> . The exception is Professor Feinberg, who co-organized a 2005 National Academy of Science symposium on forensic science which included discussion of the validity of latent print individualization. National Academy of Sciences, Sackler Colloquium on Forensic Science, [http://www.nasonline.org/site/PageServer?JServSessionIdr001=wyea1ev6u1.app14b&pagename=sackler\\_forensic](http://www.nasonline.org/site/PageServer?JServSessionIdr001=wyea1ev6u1.app14b&pagename=sackler_forensic) (last visited Mar. 15, 2008).

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The *Patterson* case changed the general acceptance outlook substantially (Table 2). Although the majority of meta-experts had not accepted the validity of latent print individualization even at the time of *Mitchell*, their raw numbers were relatively small. By the time of *Patterson*, however, the number was significantly larger, which should have helped assuage any concerns that the court may have had about being misled by a small number of fringe scientists. Most of the new meta-experts presumably had been drawn to examine the validity claims of latent print individualization by the publicity generated by earlier admissibility challenges to latent print individualization. Moreover, while there was no good reason to treat the *Mitchell*-era scientists and scholars as "fringe," any such concerns should have been put to rest by the time of *Patterson*. While determined advocates could always impugn the motives or credentials of one or more of the *Patterson*-era scientists and scholars, there is quite simply no way to interpret the entire list as "fringe." Finally, aside from raw numbers, the trend of opinions at the time of *Patterson* was quite clear. While more and more non-practitioner scientists and scholars were supporting the claim that latent print individualization was not yet validated, few were supporting the opposite position. Thus, even if the state of general acceptance was clear at the time of *Mitchell*, by the time of *Patterson* it was even clearer.

TABLE 2. General acceptance of the validity of latent print individualization among non-practitioners based on amicus curiae briefs, c. 2005.

**Acceptors**

No.	Name	Title	Affiliation	Degree	Institution	Discipline
None						

**Non-Acceptors**

No.	Name	Title	Affiliation	Degree	Institution	Discipline
1.	Mark Acree	Principal	Apex Consulting	MSFS	University of Alabama, Birmingham	Forensic Science

No.	Name	Title	Affiliation	Degree	Institution	Discipline
2.	Robert Bradley	Professor	Illinois State University	PhD	University of Kentucky	Political Science
3.	David Faigman	Professor	Hastings School of Law	MA, JD	University of Virginia	Psychology; Law
4.	Stephen Fienberg	Maurice Falk Professor	Carnegie Mellon University	PhD	Harvard University	Statistics
5.	Paul Giannelli	Richard Weathered Professor	Case Western Reserve University School of Law	MS, JD, LLM	The George Washington University; University of Virginia	Forensic Science; Law
6.	Lyn Haber	Principal	Human Factors Consultants	PhD	University of California, Berkeley	Linguistics
7.	Ralph Haber	Professor Emeritus	University of California, Santa Cruz	PhD	Stanford University	Psychology
8.	Donald Kennedy	Professor; President Emeritus	Stanford University	PhD	Harvard University	Biology
9.	Jennifer Mnookin	Professor	University of California, Los Angeles	PhD; JD	MIT; Yale University	Science & Technology Studies; Law
10.	Joëlle Anne Moreno	Professor	New England School of Law	JD	University of Pennsylvania	Law
11.	Jane Moriarty	Professor	University of Akron School of Law	JD	Boston College	Law
12.	D. Michael Risinger	Professor	Seton Hall School of Law	JD	Harvard University	Law
13.	John Vokey	Professor	University of Lethbridge	PhD	McMaster University	Psychology
14.	Sandy Zabell	Professor	Northwestern University	PhD	Harvard University	Mathematics

### c. Published Literature

Perhaps the most common method of evaluating general acceptance is by examining the published literature.<sup>207</sup> It is where scientists and scholars take most

<sup>207</sup> . ("In general, the proponent of the evidence should prove general acceptance by surveying scientific publications. Studies demonstrating the validity of new (*or old*) methods, appearing without contradiction in



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seriously the notion of being held to the arguments to which they sign their names. Put simply, scientists and scholars expect to defend the arguments they make in published literature. Therefore, a court seeking to assess the state of general acceptance of a particular proposition might do well to survey the scientific literature speaking to that proposition. Courts have indicated approval of the notion of referring to the scientific and legal literature in making assessments of general acceptance.<sup>208</sup> As Judge Altenbernd, put it, "[t]he *Frye* standard is not a direct measure of scientific trustworthiness. Instead, it is based on the assumption that the science will be trustworthy if scientists worthy of trust have published articles and made public statements in support of the scientific principle or procedure."<sup>209</sup>

There is almost no discussion of latent print validation in the forensic science literature. The most prestigious forensic journals (*Journal of Forensic Sciences* and *Forensic Science International*) contain some material on the development and imaging of latent prints, on the variability of friction ridge skin,<sup>210</sup> on fingerprint forgery,<sup>211</sup> and one

prominent scientific journals, reference works, or textbooks, are perhaps the best indicia of general acceptance.") (emphasis added); Giannelli, *supra* note 10, at 1217.

<sup>208</sup> . *People v. Shirley*, 723 P.2d 1354, 1376 (Cal. 1982) ("[S]cientists have long been permitted to speak to the courts through their published writings in scholarly treatises and journals."); *People v. Kelly*, 549 P.2d 1240, 1247 (Cal. 1976) ("[A]mici have cited a number of scientific and legal articles containing differing forms of opposition to the admissibility of voiceprint evidence. Such writings may be considered by courts in evaluating the reliability of new scientific methodology.").

<sup>209</sup> . *Brim v. State*, 779 So. 2d 427, 435-36 (Fla. Cir. Ct. 2000) (citation omitted).

<sup>210</sup> . See, e.g., Nicole Egli et al., *Evidence Evaluation in Fingerprint Comparison and Automated Fingerprint Identification Systems—Modeling Within Finger Variability*, 167 FORENSIC SCI. INT'L 189 (2006); C. H. Lin et al., *Fingerprint Comparison I: Similarity of Fingerprints*, 27 J. FORENSIC SCI. 290 (1982); Cedric Neumann et al., *Computation of Likelihood Ratios in Fingerprint Identification for Configurations of Any Number of Minutiae*, 52 J. FORENSIC SCI. 54 (2007); Cedric Neumann et al., *Computation of Likelihood Ratios in Fingerprint Identification for Configurations of Three Minutiae*, 51 J. FORENSIC SCI. 1 (2006); David A. Stoney & John I. Thornton, *A Critical Analysis of Quantitative Fingerprint Individuality Models*, 31 J. FORENSIC SCI. 1187 (1986); John I. Thornton, *The DNA Statistical Paradigm vs. Everything Else*, 42 J. FORENSIC SCI. 758 (1997); John I. Thornton, *The Snowflake Paradigm*, 31 J. FORENSIC SCI. 399 (1986).

<sup>211</sup> . Boris Geller et al., *Fingerprint Forgery—A Survey*, 46 J. FORENSIC SCI.

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report on proficiency testing,<sup>212</sup> but essentially no discussion of validation.<sup>213</sup> There is a short discussion of latent print validation in a less well known forensic journal, but that article essentially conceded lack of validation and characterized latent print individualization as a "leap of faith."<sup>214</sup>

Beyond the general forensic science literature, there is also narrower literature on forensic *identification*, an area in which latent prints are an important component.<sup>215</sup> Taken together, these sources provide substantial literature about latent print identification. But does this literature support the case for general acceptance?

One might first question whether this constitutes scientific literature.<sup>216</sup> My argument here, however, need not rely on such unkind insinuations. Even if we grant journals such as *JFI* status as a full-fledged scientific journal, the fact of the matter is that almost nothing in it addresses the validity of latent print individualization, and nothing at

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731 (2001); Boris Geller et al., *A Chronological Review of Fingerprint Forgery*, 44 J. FORENSIC SCI. 963 (1999).

<sup>212</sup> . Joseph L. Peterson & Penelope N. Markham, *Crime Laboratory Proficiency Testing Results, 1978-1991, II: Resolving Questions of Common Origin*, 40 J. FORENSIC SCI. 1009 (1995).

<sup>213</sup> . It is perhaps worth nothing that a review in the *Journal of Forensic Sciences* of the author's book, that makes the claim that latent print individualization has not been validated, makes no mention of the fact that the book makes this claim. One might imagine that such a claim would be of importance, or at least interest, to forensic scientists. James A. Bailey, *A Review of Suspect Identities: A History of Fingerprinting and Criminal Identification* 48 J. FORENSIC SCI. 476-77 (2003).

<sup>214</sup> . David A. Stoney, *What Made Us Ever Think We Could Individualize Using Statistics?*, 31 J. FORENSIC SCI. SOC'Y 197 (1991).

<sup>215</sup> . The "flagship journal" in this area is clearly the *Journal of Forensic Identification (JFI)*. Other journals include *Fingerprint Whorld* and *The Print*. In addition, a great deal of latent print practitioner literature is also "published" online. Important web sites that post original articles include [www.clpex.com](http://www.clpex.com), [www.forensic-evidence.com](http://www.forensic-evidence.com), [Fingerprints.tk](http://Fingerprints.tk) ([www.xs4all.nl/~dactyl/index.htm](http://www.xs4all.nl/~dactyl/index.htm)), [Ridges](http://Ridges) and [Furrows](http://Furrows) ([www.ridgesandfurrows.homestead.com/index.html](http://www.ridgesandfurrows.homestead.com/index.html)), and [www.latent-prints.com](http://www.latent-prints.com).

<sup>216</sup> . The online articles are neither peer reviewed nor subjected to a selective publication process; print journals are peer reviewed, though not all categories of articles are peer reviewed; and the editorial boards of a number of journals have members who are less than fully credentialed (the *JFI*, the leading publication, has just over half with advanced—master's or above—degrees in science). Many contributors are not scientists.

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all in it provides evidence in support of the validity of latent print individualization. It contains almost no articles dealing with the topic of the validity of latent print individualization. Those few articles that do address validity are unsatisfactory in terms of providing support for the claim. Some steer around the issue altogether.<sup>217</sup>

A careful reading of these articles reveals that they contain no references to any studies, data, or other evidence supporting the validity of latent print individualization. In sum, though the practitioner literature is substantial and useful for arbitrating important questions like how best to image latent prints, none of it directly addresses the validity question,<sup>218</sup> and therefore, is simply irrelevant to the question of whether the validity claims of latent print individualization are generally accepted. By contrast, the legal and scientific literature cited in note 241, does address the question of the validity of latent print individualization, and it is this literature that a court seeking to evaluate the general acceptance of latent print individualization should turn.

#### Anatomical Literature

One category of literature not represented in note 241 is anatomical literature. Some latent print proponents have argued that anatomy is the "science" in which latent print

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<sup>217</sup> . A case in point is a recent article written by Wertheim and Maceo that mentions validity in its opening paragraph and never broaches the topic again. Kasey Wertheim & Alice Maceo, *The Critical Stage of Friction Ridge Pattern Formation*, 52 J. FORENSIC IDENTIFICATION 35 (2002); see also Cole, *Fingerprint Identification*, *supra* note 35. In an email, one of the authors argued that my critique was unfair because the article was not *intended* to address the validity of latent print individualization, just "biological uniqueness." That's fine, but it supports my argument that validity is simply *unaddressed* in the practitioner literature. Instead, the article is an extended effort to explicate the "biological uniqueness" of friction ridge skin, an issue that, as already stated here and elsewhere in the literature, is irrelevant to the validity of latent print individualization. See, e.g., Cole, *Fingerprint Identification*, *supra* note 35.

Other articles simply declare the validity of latent print individualization. John D. "Dusty" Clark, *ACE-V: Is It Scientifically Reliable and Accurate?*, 52 J. FORENSIC IDENTIFICATION 401 (2002); Kasey Wertheim, *Letter re: ACE-V: Is It Scientifically Reliable and Accurate?*, 52 J. FORENSIC IDENTIFICATION 669 (2002).

<sup>218</sup> . This argument is made in greater detail in Cole, *Fingerprint Identification*, *supra* note 35.

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individualization is rooted,<sup>219</sup> and some courts have endorsed this argument.<sup>220</sup> It is true that there is a substantial body of literature concerning the formation and variability of friction ridge skin. Should this literature be considered evidence of general acceptance of latent print individualization in the relevant scientific community? Can anatomists constitute the relevant scientific community for the issue of latent print individualization, and, if so, do they generally accept it?

Generally speaking, those anatomists who do research on friction ridge skin do not concern themselves with the accuracy of latent print individualization. Their research interest is in the formation of friction ridge skin and, to a lesser extent, its function. At times, they have offered opinions as to the *uniqueness* of friction ridge skin. One anatomist, William Babler, testified to such an opinion in the admissibility hearing in *Mitchell*. But, neither Babler nor any other anatomist has ever offered an opinion, in print, as to the accuracy of latent print individualization. This is why Babler is not included on Tables 1-3.

Perhaps the best way to delineate the anatomical literature is to refer to a bibliography of 120 references submitted into evidence by the government in the first *Daubert* admissibility challenge in *Mitchell*. The bibliography was offered in response to both the "peer review and publication" and the "general acceptance" prongs of *Daubert*.<sup>221</sup> It's an impressive body of literature. However, none of the anatomical literature cited in this bibliography addresses the issue of the validity of latent print individualization. Some of the literature addresses the formation of friction ridge skin, some of it discusses looking for correlations between friction ridge skin patterns and disease or behavioral characteristics, some if it explores whether ethnicity can be predicted from friction ridge skin patterns, and some seeks to trace ancestry through friction ridge skin patterns. None of this is directly relevant to attributions of the source of latent prints by professional

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<sup>219</sup> . DAVID R. ASHBAUGH, QUANTITATIVE-QUALITATIVE FRICTION RIDGE ANALYSIS: AN INTRODUCTION TO BASIC AND ADVANCED RIDGEOLOGY (1999).

<sup>220</sup> . *Llera Plaza II*, 188 F. Supp. 2d 549 (E.D. Pa. 2002).

<sup>221</sup> . ("there is extensive peer review and widespread acceptance of the basis [*sic*] method of fingerprint analysis.").

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latent print examiners.

Let me offer some support for this assertion. At the time of the *Mitchell* hearing, I estimate that I had already read thirty-one of the sources listed in the bibliography through my own research on the history of fingerprinting.<sup>222</sup> I knew, from having read these sources, that very few of them even addressed the validity of latent print individualization,<sup>223</sup> and those that did, stated that it had not been validated.<sup>224</sup> At least one was a history book that had little to say about validation.<sup>225</sup> At least one of the sources explicitly eschewed discussion of latent print evidence altogether, leading one to wonder why it was included on the list at all.<sup>226</sup> I, therefore, suspected that the remainder of the sources did not address validity either. In order to make a provisional test of this hypothesis, I selected twenty-seven sources that seemed, based on their titles *most likely* to contain information about latent print individualization. In this exercise, I excluded sources that seemed *least likely* to contain information about latent print individualization. For example, I excluded a book entitled *Handbook of Mathematical [sic] Functions, with Formulas, Graphs, and Mathematical Tables*, another entitled *Statistics*, and another entitled *Evolution*.<sup>227</sup> (However, out

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<sup>222</sup> . SIMON A. COLE, *SUSPECT IDENTITIES: A HISTORY OF FINGERPRINTING AND CRIMINAL IDENTIFICATION* (2001).

<sup>223</sup> . For example, some of the better known sources address only the formation or uniqueness of friction ridge skin, not the accuracy of latent print individualization. FRANCIS GALTON, *FINGER PRINTS* (1892); HARRIS HAWTHORNE WILDER & BERT WENTWORTH, *PERSONAL IDENTIFICATION: METHODS FOR THE IDENTIFICATION OF INDIVIDUALS, LIVING OR DEAD* (1918); HAROLD CUMMINS & CHARLES MIDLO, *FINGER PRINTS, PALMS AND SOLES: AN INTRODUCTION TO DERMATOGLYPHICS* (1943). For more detail on this argument, see Cole, *Fingerprint Identification*, *supra* note 35.

<sup>224</sup> . MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (David L. Faigman et al. eds., 1st ed. 1997); I. W. Evett & R. L. Williams, *A Review of the Sixteen Points Fingerprint Standard in England and Wales*, 46 J. FORENSIC IDENTIFICATION 49 (1996). *But see* BERNARD ROBERTSON & G. A. VIGNAUX, *INTERPRETING EVIDENCE: EVALUATING FORENSIC SCIENCE IN THE COURTROOM* 137 (1995). *But* this discussion largely elides discussion of validity, concluding only that "Fingerprint identification is a matter of expert judgment." *Id.* at 146.

<sup>225</sup> . *IDENTIFICATION WANTED: DEVELOPMENT OF THE AMERICAN CRIMINAL IDENTIFICATION SYSTEM, 1893-1943* (Donald C. Dilworth ed.) (1977).

<sup>226</sup> . COLIN G.G. AITKEN, *STATISTICS AND THE EVALUATION OF EVIDENCE FOR FORENSIC SCIENTISTS* 132 (1995). ("Evaluation of fingerprint evidence is not discussed here.").

<sup>227</sup> . I subsequently obtained *Evolution*. There do not appear to be any references to fingerprinting, latent prints, or even to friction ridge skin.

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of curiosity, I included *Grey's Anatomy*, even though I did not expect that it would have much to say about the validity of latent print individualization.) I assigned a research assistant to look up these articles and search them for any reference to latent print identification (not specifically to validity). Many of the sources were old or in relatively obscure journals, and my assistant was unable to locate nine of the sources. (It should be noted that this search was conducted at a branch of the largest research library in the world.)<sup>228</sup> To be sure, more diligent searching might eventually unearth more of these sources. In addition, if the source was not available in the University of California Libraries, we did not choose to entail the costs of interlibrary loan. But I am satisfied that such measures were not necessary as explained below.

Of the eighteen sources that were successfully retrieved, only five of them even remotely discussed latent print identification at all. The remainder discussed the embryological formation of friction ridge skin, the inheritance of friction ridge skin patterns, or "dermatoglyphics," the interpretation of friction ridge skin patterns. At least one did not even contain the word "fingerprint."<sup>229</sup> Another concerned the formation of skin in general, not even specifically friction ridge skin.<sup>230</sup> (In addition, according to my assistant, *Grey's Anatomy* does not discuss fingerprinting at all.) Of the five that did discuss latent print identification, one discussed the identification of prints by automated systems not humans,<sup>231</sup> and two stated

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The following search terms were not found in the index: fingerprints, hands, papillary, friction, skin, epidermis, palm.

<sup>228</sup> . The University of California libraries, <http://libraries.universityofcalifornia.edu/> (last visited Mar. 15, 2008).

<sup>229</sup> . See, e.g., Sumiko Kimura & Tadashi Kitagawa, *Embryological Development of Human Palmar, Plantar, and Digital Flexion Creases* 216 ANATOMICAL REC. 191 (1986).

<sup>230</sup> . B. Allen Flaxman & Paul F. A. Maderson, *Growth and Differentiation of Skin*, 67 J. INVESTIGATIVE DERMATOLOGY 8 (1976).

<sup>231</sup> . Andrea A. Roddy & Jonathan Stosz, *Fingerprint Features—Statistical Analysis and System Performance Estimates*, 85 PROC. IEEE 1390 (1997). Automated systems are used to search for candidate matches in a database, not to determine the source of a latent print. Their performance at this task does not determine the accuracy of source attributions made by human examiners. In any case, human examiners claim higher accuracy rates than the measured performance of automated systems.

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that fingerprints were "useful" for individual identification.<sup>232</sup> Only two directly addressed the issue of accuracy.<sup>233</sup> One of these asserted that latent print identification was "accurate," and both claimed it was "infallible."<sup>234</sup> A closer examination of these two authorities revealed no studies, data, or evidence supporting these assertions but only *ipse dixit* declarations. Chatterjee declared that "it was proved scientifically that identification from fingerprints was infallible," but offered no indication as to what proof this statement refers.<sup>235</sup> The only proof discussed in the remainder of the article was Galton's purported proof of the *persistence* of friction ridge details.<sup>236</sup> Puri stated that "[i]t is now an established fact that the science of fingerprints is an exact one and the most accurate method of human identification."<sup>237</sup> Again, there was no indication as to what it was that supposedly "established" this "fact." Puri made reference to court decisions that supposedly "show that identification through fingerprints is flawless and infallible,"<sup>238</sup> but of course a court decision can show no such thing.<sup>239</sup> The remainder of the article was devoted to the "identical twins" argument in favor of the uniqueness of friction ridge skin, which, again, invokes the fingerprint examiner's fallacy and fails to address the validity of latent print individualization.

With more resources, I could go through the whole list.

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<sup>232</sup> . S.M.S. Bhalla, *Can the Science of Fingerprints Be Stifled by Human Ingenuity or Manipulation*, 15 J. INDIAN ACAD. FORENSIC SCI. 24 (1976); Ralph M. Garruto & C.C. Plato, *Fingerprints, Palms, and Soles: Historical Transitions*, 27 BIRTH DEFECTS ORIGINAL ARTICLE SERIES 7, 10 (1991) *reprinted in* DERMATOGLYPHICS: SCIENCE IN TRANSITION (C.C. Plato et al. eds., 1991) (which is the work cited in the government's bibliography).

<sup>233</sup> . K. S. Puri, *Do Monoovular Twins Have Identical Fingerprints?*, INT'L CRIM. POLICE REV. 45 (1968); S. K. Chatterjee, *Origin of Fingerprint Science and Its Development During Last 75 Years*, 13 J. INDIAN ACAD. OF FORENSIC SCI. 2 (1974).

<sup>234</sup> . Puri, *supra* note 233, at 45; Chatterjee, *supra* note 233, at 2.

<sup>235</sup> . Chatterjee, *supra* note 233, at 2.

<sup>236</sup> . *Id.* at 3.

<sup>237</sup> . Puri, *supra* note 233, at 45.

<sup>238</sup> . *Id.*

<sup>239</sup> . Courts do not typically perform validation studies. A legal opinion cannot provide evidence of validation unless that decision *refers* to some study or data that does provide validation. For more detail on this argument, see Simon A. Cole, *'Implicit Testing': Can Casework Validate Forensic Techniques?*, 46 JURIMETRICS 117 (2006).

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However, at this point I am convinced that it is extremely unlikely that the *Mitchell* bibliography contains a hidden gem that demonstrates the validity of latent print individualization. I suspect that if there were such a gem, the government would have pointed it out in *Mitchell* or a subsequent case. Therefore, at this point, I am willing to take the risk of being proven wrong.

Thus, the anatomical literature cannot provide evidence in support of the general acceptance of latent print individualization. Moreover, anatomists do not constitute the relevant scientific community for the matter of the validity of latent print individualization for the simple reason that they have evinced in their published literature no interest whatsoever in this question. Anatomists are the relevant scientific community for questions that do fall within their interest and expertise.<sup>240</sup> But, if a court is interested in literature that addresses the validity of latent print individualization it is to another (largely legal) body of literature that it must turn.

#### Literature on the Validity of Latent Print Individualization

A glance at the legal and scientific literature reveals authorities by twenty different authors or sets of authors attesting to the lack of validation of latent print individualization.<sup>241</sup> It also reveals authorities by two

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<sup>240</sup> . Questions such as: How is friction ridge skin formed? What is its biological function? To what degree are friction ridge skin patterns inherited? Do certain friction ridge patterns correlate with disease or behavioral propensities or ethnic groups?

<sup>241</sup> . See DAVID FAIGMAN, ET AL, MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY § 27-2.3.1, at 386 (2nd ed. 2002) ("Woe to fingerprint practice were such [*Daubert* admissibility] criteria applied."); Michael Saks, *Merlin and Solomon: Lessons from the Law's Formative Encounters with Forensic Identification Science*, 49 HASTINGS L. J. 1069, 1106 (1998) ("By conventional scientific standards, any serious search for evidence of the validity of fingerprint identification is going to be disappointing. . . . A vote to admit fingerprints is a rejection of conventional science as the criterion for admission. A vote for science is a vote to exclude fingerprint expert opinions."); James E. Starrs, *Judicial Control Over Scientific Supermen: Fingerprint Experts and Others Who Exceed the Bounds*, 35 CRIM. L. BULL. 234 (1999) ("Instead of meaning incapable of error, fingerprint identifications are declared to be infallible on account of the uniqueness of fingerprints to each person . . ."); David A. Stoney, *Measurement of Fingerprint Individuality*, in ADVANCES IN FINGERPRINT TECHNOLOGY



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authors holding the opposite position.<sup>242</sup> These two lists of authorities unequivocally strengthen the case against the general acceptance of latent print individualization validity.<sup>243</sup>

Many of the scholars who produced this literature have already been accounted for in our previous two surveys. Indeed, most of the entrants in our previous two tables produced some published literature. "New entrants" to the field are reported in Table 3: those scholars who appear in a survey of the literature but did not already appear in our

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327, 383 (H. C. Lee and R. E. Gaensslen eds., 2001) ("From a statistical viewpoint, the scientific foundation for fingerprint individuality is incredibly weak."); Jennifer L. Mnookin, *Fingerprint Evidence In An Age of DNA Profiling*, 67 BROOK. L. REV. 13 (2001) ("In the case of fingerprinting, the general rate of error is simply not known."); SIMON A. COLE, *SUSPECT IDENTITIES: A HISTORY OF FINGERPRINTING AND CRIMINAL IDENTIFICATION* (2001); David L. Faigman, *Is Science Different for Lawyers?* 297 SCIENCE 339 (2002) (fingerprinting has "not been seriously tested"); Paul Giannelli, *Fingerprints Challenged!* 17 CRIM. JUST. 33, 35 (Spring 2002) ("In its interpretation of *Daubert*, *Plaza I* is a well-written opinion. *Havvard* is not."); Robert Epstein, *Fingerprints Meet Daubert: The Myth of Fingerprint "Science" is Revealed*, 75 SO. CAL. L. REV. 605, 657 (2002) ("Having considered the various indicators of reliability set forth by the Supreme Court in *Daubert*, it is evident that at the present time, latent fingerprint identifications do not constitute reliable evidence."); Jessica M. Sombat, *Latent Justice: Daubert's Impact on the Evaluation of Fingerprint Identification Testimony*, 70 FORDHAM L. REV. 2819, 2825 (2002) ("the result Judge Pollak reached when he excluded expert testimony concerning fingerprints [in *Llera Plaza I*] was fair."); *Recent Case*, 115 HARV. L. REV. 2349, 2352 (2002) ("Fingerprint expert testimony does not survive application of the *Daubert* factors . . ."); Lyn Haber & Ralph Norman Haber, *Error Rates for Human Fingerprint Examiners*, in *AUTOMATIC FINGERPRINT RECOGNITION SYSTEMS* 339 (N. K. Ratha & R. Bolle eds., 2004) ("no data have been collected on how accurately latent print examiners match different images of the same finger."); Donald Kennedy, *Forensic Science: Oxymoron?* 302 SCIENCE 1625 (2003) (Fingerprinting's "reliability is unverified either by statistical models of fingerprint variation or by consistent data on error rates."); David H. Kaye, *The Non-science of Fingerprinting: United States v. Llera Plaza*, 21 QLR 1073, 1087 (2003) ("As *Llera-Plaza I* so clearly reveals, this [the evidence advanced in support of the admissibility of latent fingerprint individualization] does not satisfy *Daubert*."); Jennifer L. Mnookin, *Fingerprints: Not a Gold Standard*, 20 ISSUES SCI. & TECH. 47 (2003) ("Judge Pollak's first opinion [restricting latent fingerprint individualization testimony] was the better one."); Tamara F. Lawson, *Can Fingerprints Lie? Re-weighing Fingerprint Evidence in Criminal Jury Trials*, 31 AM. J. CRIM. L. 1, 65 (2003) ("Currently fingerprint analysis is under attack because of the lack of study done on the accuracy of the examiners . . ."); Tara Marie La Morte, *Sleeping Gatekeepers: United States v. Llera Plaza and the Unreliability of Forensic Fingerprinting Evidence under Daubert*, 14 ALB. L.J. SCI. & TECH. 171, 173

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surveys of expert witnesses and amici. In this table, I have tried to include all scholarly articles that directly address the issue of the validity of latent print individualization. As a scholar whose work focuses on the problem of the validity and admissibility of latent print individualization, I relied on my own research and literature searches to compile this survey.

It should be noted that, like the other tables, Table 3 is a table of scholars, not of published works. Scholarship is measured as an indicator of the views of the scholar who

(2003) (discussing "strong indications that the fingerprinting field should not survive a rigorous Daubert analysis."); JANE CAMPBELL MORIARTY, PSYCHOLOGICAL AND SCIENTIFIC EVIDENCE IN CRIMINAL TRIALS § 12:15 (2004) ("The assumption of the validity of fingerprinting rests upon law, rather than science."); Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Ruling from Jennings to Llera Plaza and Back Again*, 41 AM. CRIM. L. REV. 1189, 1215 (2004) ("It is clear that no studies exist that measure the accuracy of fingerprint examiners when they make conclusions of identification."); Nathan Benedict, *Fingerprints and the Daubert Standard for Admission of Scientific Evidence: Why Fingerprints Fail and a Proposed Remedy*, 46 ARIZ. L. REV. 519, 538 (2004) ("... judges have generally relied on their instincts and the long history of judicial acceptance of fingerprint evidence to admit it without serious consideration of the science behind it."); Zabell, *supra* note 35, at 178 ("*ACE-V is an acronym, not a methodology.*") (emphasis in the original); Michael Mears & Therese M. Day, *The Challenge of Fingerprint Comparison Opinions in the Defense of a Criminally Charged Client*, 19 GA. ST. U. L. REV. 705, 745 (2003) ("Those forensic experts who have examined this issue, as opposed to those whose livelihood depends upon perpetuating the misconception that fingerprint analysis is based upon the scientific method, have found the fingerprint field to be scientifically deficient."). Not all of the cited quotations refer directly to lack of validation. (See, for example, the quotation from Professor Zabell.) However, in all of the cited works, the overall message of the article is one of non-acceptance. Moreover, many of the authors (such as Zabell) also gave further indication of their views by signing the amicus brief discussed above.

<sup>242</sup> . André Moenssens, *Fingerprint Identification: A Valid Reliable "Forensic Science"?*, 18 CRIM. JUST. 31 (2003); Stephen M. Stigler, *Galton and Identification by Fingerprints*, 140 GENETICS 857 (1995); Stephen M. Stigler, *The Fingerprint Controversy*, 20 ISSUES SCI. & TECH. 12, (Winter, 2004). Professor Moenssens has been quite clear in his views. I am including the other scholar, Professor Stigler in the conservative spirit of interpreting the evidence in the light least favorable to my argument.

At the same time, I want to be careful to be fair to Professor Stigler. I must confess that I suspect that were the issue presented as sharply as I have endeavored to do here Professor Stigler's views might turn out to be more consistent with the "non-acceptors" views than his published writings indicate. But this is mere supposition on my part, and I characterize him as an "acceptor" here in the conservative spirit of

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produced it. Therefore, I have included each scholar as a single line item, no matter how many works addressing the validity of latent print individualization the scholar has produced. This seems the fairest way to proceed so as not to allow the counts to be dominated by a small number of scholars who produce a large numbers of works repeatedly drawing the same conclusion. A small number of scholars' views were difficult to categorize,<sup>244</sup> and they are not included in Table 3.

As Table 3 indicates, a survey of the published literature further strengthens the case against the general acceptance of latent print individualization. A survey of the published literature adds two highly credentialed scholars to the list of acceptors, but ten more non-acceptors. If we remove Mr. Epstein and Mr. Mears and Ms. Day, on the grounds that they have been adversaries in an admissibility challenge to latent print evidence, that leaves eight new non-acceptors. It is also important to note that, a survey of

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interpreting the evidence in the light least favorable to my argument.

<sup>243</sup> . See also Jennifer L. Mnookin, Comment, *The Validity of Latent Fingerprint Identification: Confessions of a Fingerprinting Moderate*, 7 LAW PROBABILITY & RISK 127 (2008).

<sup>244</sup> . Principally, this refers to the group of scientists including and surrounding Professor Champod. Their views have been expressed in numerous articles and a comprehensive book: Christophe Champod, *Edmond Locard—Numerical Standards and 'Probable' Identifications*, 45 J. FORENSIC IDENTIFICATION 136 (1995); Christophe Champod & Ian W. Evett, *A Probabilistic Approach to Fingerprint Evidence*, 51 J. FORENSIC IDENTIFICATION 101 (2001); CHRISTOPHE CHAMPOD ET AL., *FINGERPRINTS AND OTHER RIDGE SKIN IMPRESSIONS* (2004). Although these scholars express confidence in latent print identification, they also acknowledge that it has not been validated and characterize its conclusions as based on a "leap of faith." CHAMPOD ET AL., *supra* at 33. This characterization is consistent with that of Dr. Stoney, who has testified for defendants in admissibility challenges. David A. Stoney, *Fingerprint Identification: Scientific Status*, in *MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY* 55 (Faigman, et al. eds., 1997). But Champod et al. have not so testified. In addition, they reject the concept of "individualization," Champod, *supra*; Champod & Evett, *supra*; CHAMPOD ET AL., *supra*, which is fundamental to contemporary latent print practice, at least in the United States. See Scientific Working Group on Friction Ridge Analysis Study and Technology, *Friction Ridge Examination Methodology for Latent Print Examiners*, 3 (2002), available at [http://www.swgfast.org/Friction\\_Ridge\\_Examination\\_Methodology\\_for\\_Latent\\_Print\\_Examiners\\_1.01.pdf](http://www.swgfast.org/Friction_Ridge_Examination_Methodology_for_Latent_Print_Examiners_1.01.pdf). Under these circumstances, it does not seem justified to characterize these excellent scholars as either "acceptors" or "non-acceptors."

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 published literature alone would be even more lopsided than Table 3 indicates because most of the entrants on Tables 1 and 2 have produced published literature. The relative number of authorities in notes 241 and 242 give some indication of what a survey of the published literature would look like.

TABLE 3. General acceptance of the validity of latent print individualization among non-practitioners not listed in Table 1 or 2 based on published literature.

**Acceptors**

	<b>Name</b>	<b>Title</b>	<b>Affiliation</b>	<b>Deg.</b>	<b>Institution</b>	<b>Discipline</b>	<b>Publ'n</b>
1	André Moenssens	Douglas Stripp Professor of Law	University of Missouri, Kansas City	JD, LLM	Illinois Institute of Technology; Northwestern University	Law	Criminal Justice
2	Stephen Stigler	Ernest DeWitt Burton Distinguished Service Professor	University of Chicago	PhD		Mathematics	<i>Genetics; Issues in Science &amp; Technology</i>

**Non-Acceptors**

	<b>Name</b>	<b>Title</b>	<b>Affiliation</b>	<b>Deg.</b>	<b>Institution</b>	<b>Discipline</b>	<b>Publ'n</b>
1	Nathan Benedict	Law student		JD	?	Law	<i>Arizona Law Review</i>
2	Margaret Berger	Professor	Brooklyn Law School	JD	?	Law	<i>American Journal of Public Health</i>
3	Robert Epstein	Attorney	Federal Defender	JD	Harvard University	Law	<i>Southern California Law Review</i>
4	David Kaye	Professor	Arizona State University School of Law	JD	?	Law	<i>Quinnipiac Law Review; Int'l Statistical Review</i>

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	Name	Title	Affiliation	Deg.	Institution	Discipline	Publ'n
5	Tara Marie La Morte	Law student		JD	?	Law	<i>Albany Law Journal of Science &amp; Technology</i>
6	Tamara Lawson	Professor	St. Thomas School of Law	JD	?	Law	<i>American Journal of Criminal Law</i>
7	Michael Saks	Professor	Arizona State University School of Law	PhD	Ohio State University	Psychology	<i>Numerous law review articles, Modern Scientific Evidence</i>
8	Katherine Schwinghammer	Law student		JD	?	Law	<i>American Journal of Criminal Law</i>
9	Jessica Sombat	Law student	Fordham University School of Law	JD	Fordham University	Law	<i>Fordham Law Review</i>

At this point, by the most conservative accounting, the "score" in the scientific community stands at twenty-five to three against acceptance.<sup>245</sup> In summary, we can conclude that non-acceptors have always outnumbered acceptors and that the difference is becoming more pronounced as time goes on. As of this writing, a significant number of non-practitioner scientists and scholars have expressed clear opinions that latent print individualization lacks validation. At the same time, the government and fingerprint community have shown remarkably little

<sup>245</sup> . I certainly recognize the crudity of "keeping score" in this manner. It should be noted, however, that for many years a prominent latent print examiner web site featured a "Daubert score" in the upper right corner of its home page recording the number of victories for latent print identification in *Daubert* admissibility challenges. Although still in use as recently as 2007, the "score" has now been discontinued. See Latent Print Examination, <http://onin.com/fp/> (last visited Mar. 15, 2008). To see the "score," see Latent Print Examination (via the Internet Archive: *The Way Back Machine*), <http://web.archive.org/web/20070702092813/http://www.onin.com/fp/> (last visited Mar. 15, 2008)

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success at finding any non-practitioner scientist or scholar to take the position that latent print individualization has been validated. The result of a *Frye* analysis, therefore, should be clear.

#### 4. Anticipated Objections

As noted above, one of the characteristics of the *Frye* admissibility regime is that designations of the "relevant scientific community" are highly contestable. One might anticipate that proponents of latent print evidence would object to the constitution of the relevant scientific community, as I have construed it in the preceding section.

First, one might object that the scientists and scholars in Tables 1 and 2 consist simply of paid defense experts and that their opinions are, therefore, of no value. Some scholars have argued that individuals with a substantial pecuniary, or even a non-pecuniary, interest, should be excluded from a properly constituted relevant scientific community in a proper *Frye* analysis.<sup>246</sup> One might arguably exclude the opinions of up to five members of Tables 1-2 on this ground because they have testified on behalf of criminal defendants in challenges to latent print evidence.<sup>247</sup> However, the need to exclude these five individuals is debatable because even those scholars who argue for the exclusion of interested experts suggest that the test should be applied carefully and should only exclude experts whose "livelihood," to quote the Michigan Supreme Court, is "intimately connected with the new technique."<sup>248</sup> Thus, under this careful test, it is not clear that these individuals should necessarily be excluded at all.

However, even if they are excluded, the overall acceptance picture does not change because the non-

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<sup>246</sup> .

<sup>247</sup> . Professor Starrs, Drs. Haber and Haber, Professor Faigman, and the author.

<sup>248</sup> . *People v. Young*, 391 N.W.2d 270, 276 n.24 (Mich. 1986); see also Schwartz, *supra* note 33, at 210. The court's reasoning that "a certain degree of 'interest' must be tolerated if scientists familiar with the theory and practice of a new technique are to testify at all," *id.* at 210-11, is supported by the fact that elimination of the five meta-experts who have ever testified for defendants would eliminate some of the experts whose knowledge of validity issue is greatest and who have written most extensively about it.

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acceptors still dramatically outnumber the acceptors. Moreover, if the opinions of those individuals who have ever testified on behalf of criminal defendants were bracketed, it would also be necessary to bracket the opinions of interested government experts, whose livelihood is far more "intimately connected" to the technique than any of the individuals who have testified on behalf of defendants. This would have the effect of eliminating all latent print examiners' opinions from consideration. It would also be necessary to eliminate two of the three scholars who support the claim of latent print individualization. Dr. Budowle is an employee of the FBI, a law enforcement agency housing the largest collection of fingerprint records in the world. Professor Moenssens, though a distinguished scholar, was himself a practitioner of latent print analysis.<sup>249</sup> Thus, eliminating "interested" experts would, in fact, leave the government in a worse position because the data would show that latent print individualization evidence is accepted by only one non-practitioner while still not accepted by twenty non-practitioners.

Another potential objection to the scholars in Tables 1-3 is that some of them are not scientists. Some scholars have suggested that a rigorous application of *Frye* requires that the relevant scientific community consist of scientists.<sup>250</sup> Many of the meta-experts listed on Tables 1-3 are legal scholars, some are social scientists, and two are forensic scientists. Each of these categories might reasonably be deemed non-scientists. Whatever the merits of the argument to bracket the opinions of some scholars on Tables 1-3 as non-scientists, the argument on the whole is unconvincing. First, some individuals who may appear to be legal scholars in fact have scientific training. One legal scholar on Table 3, despite being a professor of law, not only has a master's level degree in law, but also a doctoral degree in psychology.<sup>251</sup> Another law professor has a master's degree in psychology in addition to his degree in law, and another has a master's degree in forensic science

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<sup>249</sup> . Andre A. Moenssens, *The Fingerprint Witness in Court*, 54 *FINGER PRINT & IDENTIFICATION MAG.* 3 (1973).

<sup>250</sup> .

<sup>251</sup> . *Contra* *People v. Clevenger*, 2003 WL 22872446, \*8 (Cal. Ct. App. 2003) (dismissing Professor Saks as a "law professor" who "does not . . . claim to be a scientist").

in addition to his degree in law. Second, many of the legal scholars on Tables 1-3 are evidence scholars and have acquired a sophisticated understanding of scientific arguments, methods, and procedures.<sup>252</sup> Third, legal scholarship is changing; currently it verges closely to the scientific, or at least the social scientific, than it did in the past. Although some legal scholars continue to limit themselves to doctrinal, textual analysis, many contemporary legal scholars deploy sophisticated empirical and statistical analyses.<sup>253</sup> Fourth, given that latent print individualization (as opposed to other uses of friction ridge information, such as biometrics or dermatoglyphics) is almost solely used in legal, not scientific, settings, it is hardly surprising that legal scholars are among the most likely members of the scholarly community to take the time to develop a reasonably comprehensive understanding of the evidence concerning its validity. Therefore, it might reasonably be argued that the legal academic community does, to some extent, constitute the relevant scientific community for the question of the validity of latent print individualization.

With regard to the social scientists on the list, the question of whether or not social science should be characterized as "science" is a hotly debated one. Even if one concluded that it should not, two of the meta-experts on the list (including the author) were trained in a discipline that might be an exception. Science & Technology Studies (STS) is a field of social science that takes science as its object. As such, training in this discipline involves a great deal of training, education, and thought about what makes various propositions, theories, research programs, or disciplines "scientific." Individuals with this training might be viewed as especially well equipped to assess whether or not a body of evidence supports a particular knowledge claim.

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<sup>252</sup> . For example, Professor Kaye is undoubtedly among the leading legal scholars in the world in terms of his understanding of statistical inference and also commands sophisticated knowledge of the science that contributes to DNA testing.

<sup>253</sup> . See Robert C. Ellickson, *Trends in Legal Scholarship: A Statistical Study*, 29 J. LEGAL STUD. 517, 529 (2000); Elizabeth Chambliss, *When Do Facts Persuade? Some Thoughts on the Market for "Empirical Legal Studies,"* 71 LAW & CONTEMP. PROB. (forthcoming 2008).



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Finally, it might be objected that four of the entrants on Table 3 were authored by law students. It is perhaps appropriate to assign less weight to student-authored publications. Nonetheless, the conventions of legal scholarship dictate that a great deal of legal scholarship is student-authored. Although student-authored literature is traditionally accorded less weight than faculty-authored material, it is not uncommon for student-authored literature to be treated as authority in legal scholarship and indeed in judicial opinions by even the highest courts.<sup>254</sup> There seems, therefore, to be no sound justification for bracketing the opinions of student authors. In any case, even removing student-authored material does not significantly change the overall acceptance picture. Notably, every single student-authored article on the subject finds that latent print individualization has not been validated. If there were a plausible argument to be made that latent print individualization *has* been validated, one would imagine that some law professor would direct a student to it, as a more interesting research and writing project than yet another article noting the lack of validation. And yet, no such article has appeared. Could this be because no such argument can be made? It might perhaps be argued that all the legal scholarship finds against validation because that is the only side of the argument that needs articulating, given that courts have already articulated the argument that latent print individualization is validated. Even if this were correct, at this point in history enough legal scholarship finding a *lack* of validation has been produced that a legal scholar or law student could today perceive the argument *for* validation as one that needs articulating, in the scholarly literature if nowhere else. Again, that no legal scholar other than Professor Moenssens has done so might suggest to a court undertaking a *Frye* analysis that the argument is not one that legal scholars believe.

As with the elimination of interested individuals, any strict bracketing of non-scientists actually strengthens rather than weakens the case against general acceptance of latent print individualization evidence. Such a procedure

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<sup>254</sup> . Bart Sloan, *What Are We Writing For? Student Works as Authority and Their Citation by the Federal Bench, 1986-1990*, 61 GEO. WASH. L. REV. 221, 227 (1992).

would eliminate the vast majority of practitioners, who lack backgrounds in science. This would leave the technique accepted only by a small community of practitioners who have backgrounds in science (if one wants to label someone with a degree in science who practices latent print identification for a living a "scientist") and three non-practitioner scientists. But a significant number of non-acceptors would remain.

## 5. Summary

Based on the evidence assembled in Tables 1-3, a general acceptance analysis of latent print individualization evidence under *Frye* should be an easy case. As long as a court resists the temptation to allow the practitioner community to self-validate its own knowledge claims, the picture is quite clear. The acceptors include only three scholars, two of whose opinions arguably should be eliminated because they are too closely interested. The non-acceptors, however, include more than twenty scholars from a diversity of disciplinary perspectives. While various criticisms might be made of various individuals among the non-acceptors, none applies to all of them, not even the criticism of not being practitioners. In their totality, this group wields a high degree of academic firepower: they include two members of the National Academy of Science, one of the most prestigious honors bestowed in scientists in the United States, the former President of one of the top five research universities in the United States, and some of the legal academy's most eminent evidence scholars. They include four Harvard degrees, (the "acceptor" group includes one). Although the motives or qualifications of some of these individuals may be impugned, even the elimination from consideration of a couple of them leaves the opinions of some of the others standing. The point here is that the weight of scholarly opinion seems to be approaching very closely, despite all personal and disciplinary differences, a common conclusion that latent print individualization lacks validation. This degree of consensus, it would seem, is precisely what the notion of "general acceptance in the relevant scientific community" was intended to capture.

None of this is to suggest that courts must, as a general

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rule, deem propositions unaccepted every time they are presented with a petition containing the signature of twenty-five people with advanced degrees, or that degrees from fancy universities should automatically connote authority. Deference to scientists and scholars must surely be exercised with caution. However, in this case, the totality of evidence of non-acceptance, combined with the failure of the proponents of the evidence to attract any significant support from any informed observers outside the practitioner community would seem to be a situation that should make a court very uncomfortable about deeming the evidence "generally accepted."

### C. NON-NOVELTY

A common argument holds that the *Frye* test only applies to "novel" expert evidence. Therefore, it is argued, even if latent print individualization would fail a general acceptance analysis, it would not reach that analysis because it is not novel evidence. Since *Frye* postdated the introduction of latent print evidence to U.S. courts by more than two decades,<sup>255</sup> this view would find that latent print evidence was never properly the subject of *Frye* analysis. The supposed non-novelty loophole has probably been one of the chief deterrents to admissibility challenges to latent print evidence under *Frye*.

In fact, the non-novelty loophole should not be treated as an obstacle to challenging the admissibility of latent print evidence under *Frye*. First, the notion that *Frye* limited itself to novel evidence is a myth. The *Frye* opinion contains no reference to novelty. Instead, it has been suggested that the notion that *Frye* is limited to novel evidence can be traced to a law professor.<sup>256</sup>

It is true that some state courts appear to have added a "novelty" requirement in their cases adopting *Frye*.<sup>257</sup> Other

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<sup>255</sup> . See *People v. Jennings*, 96 N.E. 1077, 1080 (Ill. 1911).

<sup>256</sup> . See DAVID H. KAYE ET AL., *THE NEW WIGMORE: EXPERT EVIDENCE*, 299 (2004); James E. Starrs, *Frye v. United States Restructured and Revitalized: A Proposal to Amend Federal Evidence Rule 702*, 26 *JURIMETRICS J.* 249, 252-53 (1986).

<sup>257</sup> . See, e.g., *Still v. State*, 917 So. 2d 250 (Fla. Dist. Ct. App. 2005); *Dirling v. Sarasota County Gov't*, 871 So. 2d 303 (Fla. Dist. Ct. App. 2004); *State v. Sercey*, 825 So. 2d 959 (Fla. Dist. Ct. App. 2002); *U.S. Sugar Corp. v. Henson*, No. 1D99-27982000, 2000 WL 1880340 (Fla. Dist. Ct. App.

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courts have rejected the novelty requirement.<sup>258</sup> But even some courts with a novelty requirement, such as the California Supreme Court, have interpreted it to refer not merely to new techniques, but also to new information about the general acceptance, or even reliability, of even time-honored techniques. *People v. Kelly* states that the non-novelty loophole remains open only "until new evidence is presented reflecting a change in the attitude of the scientific community."<sup>259</sup> Clearly, as demonstrated by the above discussion, *infra* Part , the attitude of the scientific community has been developing rapidly over the last decade. A litigant could demonstrate the existence of such new information, as explicated above, and such an interpretation would, therefore, allow a *Frye* challenge to latent print individualization evidence. Legal scholars agree that "the 'novelty' requirement should not bar relitigation if the scientific community changes its mind when further research reveals that a previously trusted technique no longer is valid."<sup>260</sup> This is precisely the case for latent print evidence. Indeed, the courts' openness to "new evidence reflecting a change in the attitude of the scientific community" is particularly pertinent to the case of latent print evidence where only the publicity generated by earlier admissibility challenges drew the attention of the mainstream scientific community to the issue of its

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Dec. 29, 2000); *State v. Meador*, 674 So. 2d 826 (Fla. Dist. Ct. App. 1996); *People v. Kelly*, 549 P.2d 1240, 1244 (Cal. 1976), *rev'd on other grounds*; *In re Commitment of Simons*, 821 N.E.2d 1184 (Ill. 2004); *Donaldson v. Cent. Ill. Pub. Serv. Co.*, 767 N.E.2d 314, 324 (Ill. 2002), *abrogated on other grounds*; *People v. Basler*, 740 N.E.2d 1 (Ill. 2000); *In re Marriage of Alexander*, 857 N.E.2d 766 (Ill. App. 5th 2006); *People v. Cumbee*, 851 N.E.2d 934 (Ill. App. 2d. 2006); *In re K.T.*, 836 N.E.2d 769 (Ill. App. 3d 2005); *In re Commitment of Bushong*, 815 N.E.2d 103 (Ill. App. 2d 2004); *People v. Canulli*, 792 N.E.2d 438 (Ill. App. 4th 2003); *Owens Corning v. Bauman*, 726 A.2d 745, 767 (Md. Ct. Spec. App. 1999), *abrogated on other grounds*.

<sup>258</sup> . See, e.g., *Gilbert v. DaimlerChrysler Corp.*, 685 N.W.2d 391, 408 (Mich. 2004).

<sup>259</sup> . *Kelly*, 549 P.2d at 1245; see also *Fishback v. People*, 851 P.2d 884, 891 (Colo. 1993).

<sup>260</sup> . John William Strong, *Language and Logic in Expert Testimony: Limiting Expert Testimony by Restrictions of Function, Reliability, and Form*, 71 OR. L. REV. 349, 367 (1992) ("no apparent reason exists . . . for limiting the standard to 'novel' scientific evidence").

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validity.<sup>261</sup> Finally, to the extent that courts do enforce a non-novelty loophole, they defy common sense, as legal scholars have pointed out.<sup>262</sup>

#### D. NONSCIENTIFIC EVIDENCE

Another "loophole" in *Frye* is that many courts refuse to apply it to "nonscientific" evidence.<sup>263</sup> Should this be a bar to subjecting latent print evidence to *Frye*? It would seem not. The rationale for applying the *Frye* test to scientific evidence is that "it is widely agreed that propositions perceived as 'scientific' by the jury possess an unusually high degree of persuasive power."<sup>264</sup> Arguably, nonscientific evidence that does not enjoy the extra degree of persuasive power associated with "science," need not bear the burden of a *Frye* inquiry. But, while the issue of whether latent print individualization truly is or is not properly characterized as "science" is a contested and vexing,<sup>265</sup> there can be little doubt that it is presented to the jury as "science." Therefore, it should be required to meet the *Frye*

<sup>261</sup> . Andy Newman, *Fingerprinting's Reliability Draws Growing Court Challenges*, N.Y. TIMES, Apr. 7, 2001, at A8; Andy Newman, *Judge Rules Fingerprints Cannot Be Called a Match*, N.Y. TIMES, Jan. 11, 2002, at A14; Seth Stern, *A Harder Day in Court for Fingerprint, Writing Experts*, CHRISTIAN SCI. MONITOR, Jan. 16, 2002, at 2; Michael Specter, *Do Fingerprints Lie?*, NEW YORKER, May 27, 2002, at 96; David L. Faigman, *Is Science Different for Lawyers?*, 297 SCIENCE 339 (2002); Adrian Cho, *Fingerprinting Doesn't Hold Up as a Science in Court*, 295 SCIENCE 418 (2002); Shannon P. Duffy, *Philadelphia Judge Reverses Landmark Fingerprint Decision*, LEGAL INTELLIGENCER, Mar. 14, 2003; Michael Higgins, *Fingerprint Evidence Put on Trial*, CHI. TRIB., Feb. 25, 2002, at 1; Giannelli, *supra* note 241, at 33; Mike Weiss, *U.S. Judge Challenges 'Science' of Fingerprints*, S.F. CHRONICLE, Feb. 24, 2002, at A1; Edward Lazarus, *Why Judges Rarely Change Their Minds*, LEGAL AFFAIRS, July/August, 2002, at 39.

<sup>262</sup> .

<sup>263</sup> . Strong, *supra* note 260, at 367; Edward J. Imwinkelried, *The Escape Hatches from Frye and Daubert: Sometimes You Don't Need to Lay Either Foundation in order to Introduce Expert Testimony!*, 23 AM. J. TRIAL ADVOC. 1 (1999).

<sup>264</sup> . Strong, *supra* note 260, at 367.

<sup>265</sup> . Michele Triplett & Lauren Cooney, *The Etiology of ACE-V and its Proper Use: An Exploration of the Relationship Between ACE-V and the Scientific Method of Hypothesis Testing*, 56 J. FORENSIC IDENTIFICATION 345 (2006); André Moenssens, *Is Fingerprint Identification a "Science"?*, [http://www.forensic-evidence.com/site/ID/ID00004\\_2.html](http://www.forensic-evidence.com/site/ID/ID00004_2.html) (last visited Mar. 15, 2008); Bernard W.N. Robertson, *Fingerprints, Relevance and Admissibility*, 2 N.Z. RECENT L. REV. 252, 256 (1990) ("Fingerprint 'experts' are in fact technicians and not scientists.").

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standard. As Professor Strong put it, "[p]ropositions possessing significantly increased potential to influence the trier of fact as 'scientific' assertions should be required to meet scientific standards of reliability."<sup>266</sup>

#### IV. *FRYE* RULINGS

Astonishingly, until 2007 there was no ruling that explicitly considered the admissibility of latent print individualization evidence under *Frye*. As discussed above, this glaring lacuna was probably due to several factors including the non-novelty loophole and the defense bar's assumption that such challenges were only plausible under *Daubert*. There have, however, been some admissibility rulings in *Daubert* or mixed jurisdictions that have considered the issue of the general acceptance of latent print individualization.

Many courts have reasoned that latent print evidence satisfies the general acceptance requirement by restricting the relevant community to fingerprint examiners or to the forensic community.<sup>267</sup> Some courts have relied exclusively on general acceptance to find latent print evidence admissible under *Daubert*.<sup>268</sup>

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<sup>266</sup> . Strong, *supra* note 260, at 368.

<sup>267</sup> . See, e.g., United States v. Sullivan, 246 F. Supp. 2d 700, 703 (E.D. Ky. 2003) ("The court finds that ACE-V is generally accepted in the fingerprint analysis and forensic science fields . . ."). While it would be difficult to dispute this assertion as it concerns fingerprint examiners, it is far less clear that it is accurate as it concerns the field of "forensic science" more generally. In any case, although the court noted that "[t]he plaintiff's expert, Joy Younce, testified that ACE-V is the standard methodology used by fingerprint examiners in analyzing fingerprints," *id.* (emphasis added), the court refers to no evidence about the state of general acceptance among forensic scientists. See also United States v. Mitchell, 365 F.3d 215, 241 (3d Cir. 2004); United States v. Collins, 340 F.3d 672, 682 (8th Cir. 2003) ("Fingerprint evidence and analysis is generally accepted."); United States v. Crisp, 324 F.3d 261, 268 (4th Cir. 2003) ("While the principles underlying fingerprint identification have not attained the status of scientific law, they nonetheless bear the imprimatur of a strong general acceptance, not only in the expert community, but in the courts as well."); United States v. Abreu 406 F.3d 1304, 1307 (11th Cir. 2005).

<sup>268</sup> . Michael J. Saks, *Reliability Standards: Too High, Too Low, or Just Right? The Legal and Scientific Evaluation of Forensic Science (Especially Fingerprint Expert Testimony)*, 33 SETON HALL L. REV. 1167, 1181 (2003).

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A. *COMMONWEALTH V. PATTERSON*

Perhaps the most extended discussion of general acceptance can be found in the Supreme Judicial Court of Massachusetts's ruling in *Commonwealth v. Patterson*.<sup>269</sup> The case was an unusual one in that it dealt both with the admissibility of latent print individualization testimony in general and with a particular application known as "simultaneous impressions."<sup>270</sup> Massachusetts is a mixed *Frye-Daubert* jurisdiction, but the decision in *Patterson* relied heavily on general acceptance. The court began by acknowledging that its own ruling in *Canavan's Case*<sup>271</sup> mandated that the relevant scientific community "be defined broadly enough to include a sufficiently broad sample of scientists so that the possibility of disagreement exists," not "so narrowly that the expert's opinion will inevitably be considered generally accepted."<sup>272</sup> This would seem to clearly call for extending the relevant scientific community beyond the narrow confines of practitioners.

At one point, the court appeared to claim that there are some "scientists" who accept latent print individualization, naming one: Professor Babler.<sup>273</sup> But, as the court acknowledged, and I noted above, Babler's research concerns "the underlying premises of fingerprint examination," not the validity of latent print individualization.<sup>274</sup> Indeed, as the court did *not* mention, Babler has never made any statement concerning the

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<sup>269</sup> . 840 N.E.2d 12 (Mass. 2005).

<sup>270</sup> . The details of simultaneous impression identification need not concern us here, but it consists of aggregating consistent ridge detail from multiple latent prints when no one of those latent prints has "sufficient" ("sufficiency" being an undefined concept in latent print analysis) ridge detail for identification. Such aggregation is, of course, only legitimate if it is known that the latent prints were laid down by a single hand—that is, "simultaneously" and not by different hands at different times. The ability of latent print examiners to distinguish between simultaneous and non-simultaneous sets of latent prints had never been measured at the time *Patterson* was decided, though one pilot study has now been conducted. John P. Black, *Pilot Study: The Application of ACE-V to Simultaneous (Cluster) Impressions*, 56 J. FORENSIC IDENTIFICATION 933 (2006).

<sup>271</sup> . 733 N.E.2d 1042 (Mass. 2000).

<sup>272</sup> . *Patterson*, 840 N.E.2d at 25.

<sup>273</sup> . *Patterson*, 840 N.E.2d at 24 n. 12.

<sup>274</sup> . *Id.*

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validity or accuracy of latent print individualization.

In the final analysis, the court did not rely on Babler, but instead simply excluded all scientists from the relevant community altogether and limited the community to practitioners. Directly after acknowledging its own call for breadth in *Canavan's Case*, the court then said:

The judge properly ensured that the technical community in which latent fingerprint identification and ACE-V is generally accepted is broad enough to include 'some *practitioners* who acknowledge flaws in the methodology' and tolerant enough to allow 'some, albeit, limited room for dissent.'<sup>275</sup>

Therefore, the Supreme Court upheld the trial court's finding "that both latent fingerprint identification theory in general and the ACE-V methodology in particular are generally accepted in the fingerprint examiner community" because of "the unanimous and long-standing acceptance of latent fingerprint identification theory."<sup>276</sup> In upholding the trial court's finding of "unanimous" acceptance, the Supreme Court not only sanctioned the exclusion of all non-practitioners from the "relevant scientific community," but also the exclusion of even those practitioners who dared to doubt that the validity of latent print individualization had been established, such as Mr. Acree, a latent print examiner who signed the *amicus curiae* brief.<sup>277</sup> (discussed *supra*, Part . Thus, the court's designation of the "relevant scientific community" was not really disciplinary, but merely ideological. Mr. Acree was presumably excluded from the relevant scientific community not because he wasn't a practitioner—he was—but merely because he held the "wrong" opinion.

However, the court went on to rule that the trial court *did* abuse its discretion in admitting latent print evidence concerning simultaneous impressions. The government did not make a sufficient showing that simultaneous impressions were generally accepted within even the latent print practitioner community. But, even in excluding simultaneous impressions, the court emphasized its narrow definition of the relevant scientific community. It explicitly noted that it is the fingerprint community, not the broader

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<sup>275</sup> . *Id.* at 25 (emphasis added).

<sup>276</sup> . *Id.* at 20.

<sup>277</sup> . See Part III. B. 3. b. *supra*.



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scientific community, whose "general acceptance" would be necessary to render simultaneous impression evidence admissible: "if the Commonwealth establishes that the application of ACE-V to simultaneous impressions is generally accepted *in the fingerprint examiner community*, the evidence is properly admitted."<sup>278</sup>

As evidence of its claim that the practitioner community was "broad" in and of itself, the court offered the following: first, "the guidelines and standards developed by the Scientific Working Group on Friction Ridge Analysis, Study and Technology (SWGFAST) committees are subject to repeated discussion, critique, and debate *by the entire SWGFAST community and by members of the IAI* [International Association for Identification]."<sup>279</sup> Second, "[a]dditional room for disagreement lies in the ongoing debate over how many points of similarity, if any, are needed to conclusively make a match."<sup>280</sup> Third, the court noted that some latent print examiners disapprove of the use of simultaneous impressions, the application of latent print analysis at issue in *Patterson*.<sup>281</sup>

The first assertion is, of course, laughable because the court has, in its very articulation of the latent print community's supposed "breadth" and "tolerance" for dissent, acknowledged that it is in fact quite clearly a closed community whose doctrines are not open to discussion by those who are not either members of the professional organization or to an elite appointed body, convened by the FBI, that seeks to set voluntary practice guidelines for the profession (SWGFAST). The second two assertions speak to matters of debate within the profession (the number of points necessary to establish an individualization and the appropriateness of individualization from simultaneous impressions), but not to the fundamental validity of latent print individualization itself.

The court conflates a discipline's commitment to robust internal debate within the community with the discipline's ability to achieve "external validation"—to convince

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<sup>278</sup> . *Patterson*, 840 N.E.2d at 29 (emphasis added).

<sup>279</sup> . *Id.* (emphasis added).

<sup>280</sup> . *Id.*

<sup>281</sup> . *Id.*

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qualified outsiders that the discipline's claims are true. Certainly, a court should want to find robust internal criticism within any expert community seeking to offer evidence in court. Classically believed to be an indispensable attribute of a healthy scientific community (or, for that matter, any community devoted to rational inquiry whether or not it calls itself "science"), this sort of open inquiry is supposed to encourage the rigorous testing of ideas and help eliminate weak claims.<sup>282</sup> However, while internal criticism may be necessary for admissibility under *Frye*, it is not sufficient. Astrologers could make a persuasive showing of robust internal debate by showing that astrologers disagree as to what particular practices should be used to answer particular questions, what particular astrological techniques should be used under what circumstances, how to interpret various astrological signs, and so on. But, astrologers cannot demonstrate external validity. They have not convinced qualified outsiders—scientists—that the discipline's claims are true. In *Patterson*, the court was able to find evidence of robust internal debate, but it was able to find no evidence of external validation.<sup>283</sup> The court then fundamentally

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<sup>282</sup> . Among the thinkers who claimed that such open inquiry was an ideal of scientific inquiry were the sociologist Robert Merton who called it "organized skepticism" and the philosopher Karl Popper. ROBERT K. MERTON, *SOCIAL THEORY AND SOCIAL STRUCTURE: TOWARD THE CODIFICATION OF THEORY AND RESEARCH* (1949); KARL POPPER, *CONJECTURES AND REFUTATIONS* (1965).

<sup>283</sup> . Indeed, though they are not all in the *Patterson* record, there are numerous indicators of the latent print community's outright hostility to external validation. See Donald Kennedy, *Forensic Science: Oxymoron?*, 302 *SCIENCE* 1625 (2003) (reporting that a National Research Council report on forensic examination "was dropped because the government insisted on rights of review that the Academies have, at least in the recent past, refused to grant a sponsor."); *United States v. Mitchell*, 365 F.3d 215, 255 (3d Cir. 2004) (claiming to be "deeply discomforted" by credible, though contested, allegations "that a conspiracy within the Department of Justice intentionally delayed the release of [a grant] solicitation" for validation research until after the defendant was convicted.); Itiel E. Dror, *Cognitive Effects and Cognition of Forensic Experts*, Presentation to the National Academies' Meeting of the Committee on Identifying the Needs of the Forensic Sciences Community (Dec. 6, 2007) (presentation slides available at <http://www7.nationalacademies.org/stl/Forensics.html>) (reporting "attempts to hide/bury/censor" issue of cognitive and contextual bias in the production of a friction ride Sourcebook by the National Institute of Justice by removing a commissioned article that had been favorably peer reviewed from the Sourcebook).

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changed the meaning of the "breadth" principle. Whereas all the previous discussions of "breadth," including the court's own, conceive of breadth as extending the discussion beyond the practitioner community to the scientific community, in *Patterson* breadth has come to be defined by "tolerance" for dissenting views *within the practitioner community itself*.

Even if the court did find that robust internal criticism could substitute for external validation, what is remarkable is how underwhelming was the court's endorsement of the latent print community's adherence to this ideal of robust internal criticism. Far from finding a resounding commitment to open inquiry, the court characterizes the latent print community as allowing only "limited room for dissent." This rather lukewarm characterization raises more questions than it answers. What, precisely, did the court mean when it said that room for dissent is "limited" in the latent print practitioner community? If it is a scientific community, or even a non-scientific professional community, why is room for dissent "limited"? If it is a community that serves the interests of justice, why is room for dissent "limited"? Why, if fingerprinting is as clear cut, as accurate, indeed as "infallible" as its proponents claim, is there a need to "limit" dissent in the practitioner community anyway? And, if latent print examiners constitute a community that limits dissent, wouldn't that be all the more reason to weigh heavily the opinions of outsiders and not allow such a community to constitute a self-certifying "relevant technical community"? Isn't the suspicion that a community limits dissent precisely the sort of situation that necessitates broadening the field of inquiry in the general acceptance analysis?

Perhaps, in using the term "limited room for dissent," the court had in mind some of the statements from latent print practitioners themselves that may be found in the exhibits submitted in the *Patterson* case, attesting to the prevalence of "dogma" and a "cultish demeanor" within the latent print community. Perhaps it had in mind Mr. Ashbaugh's statement:

In the past the friction ridge identification science has been akin to a divine following. Challenges were considered heresy and challengers frequently were accused of chipping at the foundation

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of the science unnecessarily. This cultish demeanor was fostered by a general deficiency of scientific knowledge, understanding, and self-confidence within the ranks of identification specialists. A pervading fear developed in which any negative aspect voiced that did not support the concept of an exact and infallible science could lead to its destruction and the destruction of the credibility of those supporting it.<sup>284</sup>

Or perhaps this:

The failure of the identification community to challenge or hold meaningful debate can also be partly attributed to the fact that the friction ridge identification science has been basically under the control of the police community rather than the scientific community. In the eyes of many police administrators, friction ridge identification is a tool for solving crime, a technical function, as opposed to a forensic science.<sup>285</sup>

Or perhaps it had in mind the following statement by Mr. Grieve, long the editor of the *Journal of Forensic Identification*:

[T]his categorical requirement of absolute certainty has no particular scientific principle but has evolved from a practice shaped more from allegiance to dogma than a foundation in science. Once begun, the assumption of absolute certainty as the only possible conclusion has been maintained by a system of societal indoctrination, not reason, and has achieved such ritualistic sanctity that even mild suggestions that its premise should be re-examined are instantly regarded as acts of blasphemy. Whatever this may be, it is not science.<sup>286</sup>

Such statements, coming from the technique's *own practitioners*, would seem to make the latent print community a poor choice for a practitioner community that is permitted to "self-validate" its own claims and exclude outsiders from the assessment of these claims. In sum, the Patterson decision is unconvincing because the court allowed admissibility to rest upon internal criticism instead of the external validation it demanded in *Canavan's Case* and allowed it to rest upon an extremely weak finding of commitment to robust internal criticism at that.

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<sup>284</sup> . DAVID R. ASHBAUGH, QUANTITATIVE-QUALITATIVE FRICTION RIDGE ANALYSIS: AN INTRODUCTION TO BASIC AND ADVANCED RIDGEOLOGY, 4 (1999).

<sup>285</sup> . *Id.*

<sup>286</sup> . David L. Grieve, *Possession of Truth*, 46 J. FORENSIC IDENTIFICATION 521, 528 (1996) (describing "shock" and "disbelief" "within the forensic science community" at the results of the 1995 test).



get into evidence.

This article has focused on the neglected issue of the admissibility of latent print evidence in *Frye* jurisdictions. While this should be of importance to those wrestling with the admissibility of latent print (and other forensic) evidence in those jurisdictions, the research reported here has implications that go beyond *Frye* jurisdictions and go beyond latent print evidence. The process of applying *Frye* to a single form of evidence has focused our attention on a number of important principles that should apply for *Frye* analyses of any evidence, including the problems with practitioner-only evidence, the importance of breadth, and the challenges of constituting a relevant scientific community for techniques like latent print identification whose testimonial claims are not obviously within the domain of any particular scientific discipline. I will explore some of these further implications below.

#### A. IMPLICATIONS FOR *DAUBERT* JURISDICTIONS

As I have noted above, I have turned my attention from *Daubert* to *Frye* only reluctantly, due to my strong belief, a belief shared by the overwhelming bulk of legal scholarship on the issue, that latent print individualization evidence does not satisfy any reasonable application of *Daubert*.<sup>292</sup> Nonetheless, the argument presented here for exclusion of latent print individualization evidence under the *Frye* rule may yet be of some relevance for consideration of the same evidence under the *Daubert* standard. *Daubert* still incorporates the general acceptance standard as one of the five factors designed to assess its "reliability" requirement. Thus, the case presented here demonstrates clearly that latent print individualization evidence fails to satisfy at least one of the five *Daubert* factors, and, moreover, it fails to satisfy the factor that it has widely been assumed it would have easiest time satisfying.

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2000).

<sup>292</sup> . Most evidence scholars who have addressed the issue agree that if *Daubert* has any meaning at all, it must preclude the admission of evidence that cannot demonstrate reliability. See, citations in note 241, *supra*.

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B. *Frye* OR *Daubert*?

Although *Daubert* is frequently criticized, relatively few scholars, principally Professor Schwartz, have gone so far as to argue that *Frye* is actually preferable.<sup>293</sup> Should this case study change our view on this question?

I have previously argued that, in the understanding of the scientific basis of latent print individualization, *Daubert* has had a transformative effect.<sup>294</sup> What is probably the principal flaw in the arguments vouching for latent print individualization—the confusion between uniqueness and accuracy that I have labeled the “fingerprint examiner’s fallacy”—was not clearly enunciated until after the *Daubert* decision.<sup>295</sup> It would not be going too far to suggest that *Daubert* had a salutary effect on the understanding of the scientific basis underlying latent print individualization merely by prompting a renewed look at the evidence after nearly a century of “general acceptance.”

In addition, of course, *Daubert’s* focus on reliability sharpened the questions that were asked and focused attention of the crucial, and hitherto neglected, issue of validity. That pilot accuracy studies are now being undertaken, after a century of use of the technique in court, may to some extent be ascribed to the influence of *Daubert*.<sup>296</sup> Thus, the principal merit of *Daubert* has been symbolic in compelling various legal system actors to look more closely and more searchingly at many types of evidence that have long been taken for granted. But these searching inquiries have not necessarily generated rigorous admissibility rulings.

As demonstrated above, *Daubert’s* celebrated vagueness has essentially allowed trial judges free rein to rule according to their instincts. One undervalued virtue of *Frye* is that, however vague it may be, it is less vague than

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<sup>293</sup> . Schwartz, *supra* note 33.

<sup>294</sup> . Simon A. Cole, *Jackson Pollack, Judge Pollak, and the Dilemma of Fingerprint Expertise*, in *EXPERTISE IN REGULATION AND LAW* 98 (Edmond ed., 2004).

<sup>295</sup> . See David A. Stoney, *Fingerprint Identification: Scientific Status*, in *MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY* 55 (Faigman, et al. eds., 1997); Fred Woodworth, *A Printer Looks at Fingerprints*, *MATCH!*, Winter, 1997, at 36.

<sup>296</sup> . Kasey Wertheim et al., *A Report of Latent Print Examiner Accuracy During Comparison Training Exercises*, 56 *J. FORENSIC IDENTIFICATION* 55 (2006).

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*Daubert*. At some point, the existence of general acceptance becomes difficult to fudge, especially if the principles enunciated above, against practitioner-only acceptance and for breadth, are adhered to. My principal reason for skepticism about Professor Schwartz's embrace of *Frye* lay in my sense that it would be too receptive to closed communities, despite Professor Schwartz's and some courts' efforts call for a broad interpretation of *Frye*. It would appear, however, that applying *Frye*'s notion of the "relevant scientific community" is not necessarily any more difficult than applying *Daubert*'s notion of reliability.

This discussion would seem to support the emerging scholarly view that *Frye* and *Daubert* are not as different as scholars have previously assumed. Most evidence that fails *Daubert* should probably fail *Frye* and vice versa. Indeed, this case study shows that even one of Professor Saks's prime examples of a technique that satisfied *Frye* and failed *Daubert*<sup>297</sup> actually fails *Frye* too. The difficulties appear to lie not so much with which admissibility standard is chosen, but in operationalizing either standard in an even-handed manner.<sup>298</sup>

#### C. THE CLASH OF THE LEGAL AND SCIENTIFIC COMMUNITIES

By showing the lack of general acceptance of the validity of latent print individualization, this articles raise a larger issue that transcends the narrow question of legal admissibility: the disconnect between the legal and scientific communities as concerns the validity of latent print individualization. The evidence assembled above clearly demonstrates that members of the scientific community do not accept that the validity of latent print individualization has been established. In the course of demonstrating that latent print individualization lacks general acceptance, this article has demonstrated the more important fact that only with difficulty can the government produce any non-practitioner scientists who accept the

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<sup>297</sup> . Saks, *supra* note 42.

<sup>298</sup> . See generally Margaret A. Berger, *What Has a Decade of Daubert Wrought?*, 95 AM. J. PUB. HEALTH S59 (2005); Susan Haack, *Trial and Error: The Supreme Court's Philosophy of Science*, 95 AM. J. PUB. HEALTH S66 (2005); D. Michael Risinger, *Navigating Expert Reliability: Are Criminal Standards of Certainty Being Left on the Dock?*, 64 ALB. L. REV. 99 (2000).



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claim that latent print individualization is valid. And yet, paradoxically, few courts have acknowledged this deficiency, and those that have have tended to deem it irrelevant to admissibility.<sup>299</sup> Most courts have tended to uphold the admissibility of latent print individualization with ringing pronouncements about its validity. As I have noted elsewhere, these pronouncements have come to stand in for scientific validation when proponents of latent print individualization are called upon to justify their claims.<sup>300</sup> Indeed, courts have essentially *become* the "relevant scientific community" for latent print evidence, a fact they sometimes inadvertently acknowledge when they discuss "general acceptance" in the "judicial communit[y]" as if it were something conferred by courts themselves, rather than by an external expert community.<sup>301</sup> What might "acceptance" in the "judicial communit[y]" mean, other than following precedent? Such reasoning turns a *Frye* analysis into an exercise in following legal precedent, rather than the deference to an external expert community that stands at the heart of *Frye*.<sup>302</sup> Moreover, courts' upholding of the admissibility of latent print evidence has required them to essentially deem irrelevant the views of the scientific community.

It is difficult to think of comparable examples of scientific issues upon which the legal and scientific communities stand in such stark and dramatic disagreement. Will courts continue to hold out against the

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<sup>299</sup> . United States v. Sullivan, 246 F. Supp. 2d 700 (E.D. Ky. 2003); United States v. Mitchell, 365 F.3d 215 (3d Cir. 2004); *Llera Plaza II*, 188 F. Supp. 2d 549 (E.D. Pa. 2002).

<sup>300</sup> . Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Ruling from Jennings to Llera Plaza and Back Again*, 41 AM. CRIM. L. REV. 1189 (2004).

<sup>301</sup> . United States v. Crisp, 324 F.3d. 261, 268 (4th Cir. 2003) (discussing "general acceptance, not only in the expert community, but in the courts as well"); *Id.* at 269 (referring to "the consensus of the expert and judicial communities that the fingerprint identification technique is reliable."); Megan J. Erickson, *Daubert's Bipolar Treatment of Scientific Expert Testimony—From Frye's Polygraph to Farwell's Brain Fingerprinting*, 55 DRAKE L. REV. 763, 809 (2007) ("The courts willingly overlook the subjectivity inherent in latent fingerprint evidence testimony because of what the court considers to be its own 'general acceptance' (even if 'general acceptance' means within the judicial community, rather than scientific community).").

<sup>302</sup> . Giannelli, *supra* note 10, at 1218-19.

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view of the scientific community? Will the scientific  
community become more aggressively interventionist? The  
recent formation of a panel on forensic science by the  
National Academies, the most prestigious and credible  
scientific organization in the United States, raises the  
possibility of a more forceful intervention on behalf of the  
scientific community.<sup>303</sup> But whether and to what extent the  
panel will directly address the issue of validity remains to  
be seen.

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<sup>303</sup> . The National Academies, Committee on Science, Technology, and  
the Law, <http://www7.nationalacademies.org/stl/Forensics.html> (last  
visited Mar. 15, 2008).