Comment on ‘Scientific validation of fingerprint evidence under Daubert’

SIMON A. COLE†
Associate Professor of Criminology, Law and Society, University of California, Irvine, CA 92697, USA

[Received on 16 February 2007; revised on 10 March 2007; accepted on 10 March 2007]

I have been asked to comment on the strangest article. Lyn and Ralph Haber describe a forensic assay that apparently has been offered in court at least 100 000 times over the last century in the United States alone; that offers conclusions of ‘individualization’ linking trace evidence to a single individual, to the exclusion of all other potential sources in the universe; and whose rate of error, proponents have claimed in sworn testimony, is ‘zero’. Yet, the article reports, there has been no empirical validation of this assay or even of its component steps. Worse still, the necessary preliminary steps for carrying out validation, such as producing an agreed upon published protocol, developing a standardized form for reporting conclusions and developing a metric of trace similarity, have not yet been done—100 years after the assay was first made the subject of sworn expert testimony in court. In place of empirical validation data, the proponents of this technique and the attorneys who employ it muster obviously bankrupt and deceptive arguments, such as the assay is valid because it has been accepted by courts and its own practitioners for 100 years; the assay is valid because each case in which it was used constituted a de facto test of its validity; the assay is valid because multiple analysts ‘usually’ (but how often is not specified) reach consistent conclusions; and the assay is valid when used by a competent analyst (an argument that is sustained by deeming all analysts implicated in exposed errors as ‘incompetent’ in post hoc fashion).

I must admit that I was surprised and perplexed by the Habers’ claims and even a bit incredulous as to whether the situation they described was real or some sort of figment of their joint imagination. How could such a state of affairs exists? All confusion dissipated, however, when I realized that the technique they were describing was ‘Analysis, Comparison, Evaluation–Verification’ (ACE-V), the current term favoured by North American latent print examiners (LPEs) for the ‘methodology’ of latent print individualization, a forensic technique that has demonstrated an astonishing ability to defy one’s expectations of both science and law.

Readers of an older generation who received some training in latent print identification may find themselves surprised by the reference to ‘ACE-V’ to describe a process which may have been taught to them simply as ‘latent print comparison’. One’s training in latent prints need not be that old for one to have never heard the term ‘ACE-V’. As recently as 2001, Olsen and Lee (2001) saw no need to name ‘the dominant method’ of latent print analysis other than to call it ‘the conventional method’.

Readers with backgrounds in science, meanwhile, may find themselves perplexed by references to ACE-V as a ‘methodology’ when ACE-V, which essentially lays out loosely defined steps of analysing prints, comparing their features, evaluating whether there is sufficient consistency to warrant a conclusion of ‘individualization’ and then repeating the process (‘verification’), would seem to

† Email: scole@uci.edu
be, as Zabell (2005) has put it, ‘an acronym, not a methodology’. If I have a criticism of the Habers’ excellent paper, it would be that it perhaps too easily accepts at face value LPEs’ claim that ACE-V is indeed a ‘methodology’, that it is and should be widely used and that it is distinct from simply looking at two images and deciding whether they derive from a common source. After all, it does not appear to be a ‘methodology’ in the traditional sense of a generalized approach to a wide variety of empirical problems, but rather something closer to a routine ‘procedure’, ‘process’, ‘protocol’ or even a ‘method’. If, on the other hand, ACE-V is a method distinct from other methods of comparing fingerprinting, then, as the Habers note, there is no evidence that ACE-V is actually in widespread use in the United States although there are claims that it is used universally in Canada. Crucially, ACE-V is so loosely defined and requires so little in the way of documentation that it is not clear how any external observer would be able to distinguish between a latent print analysis that employs ACE-V and one that employs what Olsen and Lee (2001) called ‘the conventional method’ of looking at two prints and deciding whether or not you think they originate from a common source. Nor, as the Habers make clear, is there any evidence that ACE-V is a more accurate method of latent print analysis than any other method.

Indeed, one of the paradoxes of ‘ACE-V’ is that it has been formulated in such a way as to imply that, in fact, it is indistinguishable from ‘the conventional method’. ACE-V, we are told by its proponents, is not a new way of analysing latent print evidence but merely a new way of explaining the process for the benefit of external observers (such as jurors, judges and perhaps scholars). Under ‘ridgeology’ (later ‘ACE-V’), Ashbaugh (n.d.) has said, ‘The actual procedure used to make an identification and to present it in court, will not change. We are presently using correct methods. It is the ability to explain the process that is wanting’. As I have argued elsewhere (Cole, 1999), this curious formulation—the ‘new methodology’ that is, in fact, indistinguishable from the old methodology or even from no methodology at all—serves an important rhetorical and legal purpose in allowing latent print individualization to ground its credibility both on the veneration afforded to techniques that are a century old and on the deference paid to techniques with modern sounding labels. It allows LPEs to update their terminology without seeming to alter the technique that, they claim, has worked so well for a century or the argument that has served them so well in court: that validity is established through a century of use.

In my view, there is at least a good circumstantial case for the argument that ACE-V is not, in fact, a ‘new methodology’ at all but rather an attempt to shroud latent print individualization in a protective terminological coat that would allow it to receive scrutiny under the U.S. Supreme Court decisions Daubert v. Merrell Dow Pharmaceuticals (1993) and General Electric Co. v. Joiner (1997). The latter opinion, in particular, makes frequent mention of ‘methodology’. The timing after all is suspicious. As the Habers note, the term appeared first appeared in the literature in 1959 in reference to other forensic techniques. Although Ashbaugh used the ACE-V structure in his 1999 book, he did not use the acronym. It would seem that ‘ACE-V’ with reference to latent print individualization appeared in court before it appeared in the literature; it was the landmark Daubert hearing in United States v. Mitchell in 1999 that promulgated the idea that the methodology of latent print

---

1 Indeed, ACE-V falls short even as a protocol, inasmuch as a protocol might be expected to mandate the following of a particular set of steps in a particular order in order to minimize error. For example, a useful protocol might be expected to mandate that examiners analyse the unknown sample without reference to the known sample to which it will be compared, in order to minimize context effects (Saks et al., 2003; Dror et al., 2006). Although ACE-V might appear to do that, with its sequential steps of analysis and comparison, in fact, ACE-V does not explicitly ban the analyst from returning to the unknown sample after analysing the known, thus inviting errors caused by context effects.
individualization was ‘ACE-V’ (Triplett and Cooney, 2006). The implication is obvious: that the impetus for ACE-V came from the imperative of a Daubert hearing.

What such criticisms overlook, and what the Habers understand well, is that, in all legal disputes over the admissibility of latent print evidence, the legal actors have taken ACE-V at face value. Indeed, there is little point, legally or scientifically, to debating whether ACE-V is or is not truly a ‘methodology’. Although there may be some value to knowing the true intent of those who devised ACE-V and perhaps bringing greater clarity to the distinctions between ‘methodologies’, ‘methods’, and ‘procedures’, legally little, if anything, depends on whether or not ACE-V is appropriately called a ‘methodology’. The crucial legal and scientific question concerning latent print individualization is the validity or accuracy of the technique, not what the technique is called and whether or not it is properly described as a ‘methodology’. In ‘Scientific validation of fingerprint evidence under Daubert’, the Habers perform the exercise of taking ACE-V at face value. Assuming that ACE-V is appropriately construed as a ‘methodology’ and that LPEs in fact use it, the Habers ask, what would be necessary to demonstrate its validity, and what has been done? Their answer, of course, is divergent: virtually everything remains to be done.

The Habers perform this exercise in painstaking—and admirable—detail. They meticulously delineate the steps that would be necessary to validate ACE-V and note the absence of evidence that even the preliminary necessary steps have been taken. Again, readers might wonder why such a scrupulous accounting is necessary. Why expend so much energy documenting the fact that the method of latent print individualization has not been validated? That latent print individualization has not been validated has now been attested to by numerous scholars, not least among them the Habers themselves (Haber and Haber, 2003; Stoney, 1997; Faigman, 2002; Starrs, 1999; Cole, 2006; Lawson, 2003; Berger, 2005; Mnookin, 2001; Saks, 1998; Siegel et al., 2006; Kennedy, 2003; Moriarty, 2004; Saks and Koehler, 2005), and is, I assume, well on its way to becoming educated common sense among those who think about issues of science, probability and law. Indeed, it is becoming increasingly difficult to find a scholar who will argue that latent print individualization is valid (for an exception, see Moenssens, 2003). Why, then, belabour the point yet again?

The answer, as many know, is that two crucial constituencies remain unconvinced of the point: LPEs themselves and courts.² In this context, the Habers charge once more into the breach in an effort to close once and for all the rhetorical loopholes through which latent print individualization’s lack of validity has been evaded, such as the claim that casework constitutes a de facto validation

² This point is perhaps overstated in that it is not always easy to discern what these constituencies believe about the validity of latent print individualization. Although many courts have blithely pronounced latent print individualization to be valid, some of the more recent and thoughtful opinions (e.g. United States v. Sullivan, 2003; United States v. Mitchell, 2004) have recognized the absence of validity—and determined that this does not impact the legal admissibility of the evidence. Leaving aside for the moment the question of whether or not these represent strained readings of admissibility law, it would perhaps be most accurate to characterize these courts not as believing that latent print individualization has been validated, but rather as not believing that is has been validated but also not believing that it matters.

As for LPEs, although they sometimes debate validity on email and discussion boards, they have produced relatively little literature that actually engages the validity question. Although a small number of articles may be found which use fallacious reasoning to claim that latent print individualization has been validated (Moenssens, 2003), most literature tends to skirt around the issue by hastily shifting the discussion towards more palatable topics like ‘biological uniqueness’ (Wertheim and Maceo, 2002; for a fuller discussion, see Cole, 2006). Part of the reason that there is so little professional discussion of validity is presumably precisely the point made in the preceding paragraph of this note: i.e. since courts—the principal ‘customer’, as Meagher (2007) has aptly put it, of LPEs’ services—have clearly communicated that validity does not matter to them, there is little reason for LPEs to concern themselves with it and still less reason to go out on a limb by admitting that it is lacking.
study. For regular readers of this journal, the question will not be whether what the Habers say is true but why it is necessary to say it again. In this sense, the Habers are to be commended for having taken on an unenviable task. Readers with a scientific background are bound—almost inevitably, through no fault of the Habers—to find their arguments pedestrian and obvious, whereas those readers whose opinions are for practical purposes the ones that matter—LPEs and judges—are unlikely to understand it. The Habers have admirably managed to chart a course through these waters that lays out in methodical detail what would be necessary for the validation of latent print individualization.

Upon reading the Habers’ latest effort, I returned to a question that I have often asked myself: why do scientists and scholars see the validation issue so differently from the way LPEs see it? I have always suspected that LPEs either do not understand what validation is or pretend not to understand it, but the Habers’ article, with its psychometric perspective, has crystallized a new hypothesis.

I suspect that the difference between research scientists and LPEs may arise from what organizational theorists call ‘role ambiguity’ (Kahn, et al., 1964): confusion about the role of the LPE in the scientific enterprise. A problem with latent print analysis, and other forensic assays that rely on visual comparisons by human examiners rather than instrumental measurements, has always been that the examiner’s brain is the de facto scientific instrument (Ashbaugh, 1999; Triplett and Cooney, 2006; Rudin and Inman, 2005). From the Habers’ psychometric perspective, LPEs are nothing more than perceivers. LPEs are essentially no different than college students in a perception experiment or rats in a maze. For a research scientist, there is nothing askance about this; it is quite as it should be. The way to measure the accuracy of LPEs as detectors is to subject them to controlled studies, signal detection analysis (Phillips, et al., 2001) and so on. In the Habers’ view of the scientific enterprise, LPEs are subjects, not experimenters; they have little or no agency.

There is only one problem with this. Many LPEs conceive of themselves as scientists, rather than potential subjects of scientific experiments. LPEs, especially those with some scientific background, have a hard time conceiving of themselves as rats in the maze. In short, the Habers conceive of LPEs as research subjects and themselves as experimenters; LPEs conceive of themselves as experimenters (and conceive of the Habers, it would seem, as irritating interlopers; Langenburg, 2002). This leads to profound misunderstandings when the subject of validation comes up. LPEs, who cannot conceive of any validation study being done in which they do not play the experimenter’s role, find themselves perplexed by the Habers’s proposals to treat them as manipulable research subjects. Some researchers, however, have had some success convincing LPEs to assume the role of research subjects, though, paradoxically, they have achieved that success in part by placing an LPE alongside the research scientist in the role of experimenter (Dror and Charlton, 2006; Dror et al., 2005, 2006; Busey and Vanderkolk, 2005).

3 Champod’s charitable suggestion (this volume) that LPEs’ intuitive ‘feel’ for the extremely high ‘selectivity’ (i.e. the variability) of friction ridge skin makes it difficult for them to understand why anyone would feel the need to validate latent print individualization strikes me as quite plausible.

4 I became aware of this through a personal exchange with Alice Maceo, a scientifically trained LPE who had participated in psychological perception experiments involving attaching electrodes to her scalp at some discomfort to her (Busey and Vanderkolk, 2005). Maceo argued (as I recall it) that validation was appropriate only to things like scientific instruments and chemical assays and not, therefore, to latent print analysis. I agree with this, but I think of latent print analysis as more like an instrumental assay than an open-ended search for knowledge. Therefore, I countered (cleverly, I thought) that latent print analysis was instrumental analysis and that the instrument was her brain, and that was why researchers had found it necessary to connect electrodes to it. I meant nothing pejorative by this, but Maceo, I thought, was perhaps not amused.
LPEs’ conception of themselves as experimenters, meanwhile, leads them to conceptualize latent print individualization, not as an assay to be validated, but rather as a discipline whose claim to ‘scientific’ status must be defended. (The Daubert court did not help matters here by being less than clear in distinguishing the issue of ‘reliability’ of evidence from the issue of whether or not evidence was ‘scientific’; Edmond, 2002.) Thus, we now have numerous works by LPEs purporting to demonstrate that ACE-V follows (or is) something called ‘the scientific method’ (Acree, 1998; Ashbaugh, 2002; Wertheim, 2000; Beeton, n.d.; Tripplett and Cooney, 2006). This leads to some rather convoluted arguments, I suspect because ACE-V is more like a routine assay than an open-ended mode of scientific inquiry. Thus, to merely pick on the best developed of these exegeses, we have a requirement that ‘data’ (rather than a hypothesis) be ‘falsifiable’; the claim that Newton’s Law of Gravity ‘was accepted by most people as infallible for more than 200 years’; the notion that ‘hypothesis testing’ has an error rate and the assertion that ‘ACE-V is synonymous with hypothesis testing’ (Tripplett and Cooney, 2006). These statements suggest a certain conflation of the notions of validation, hypothesis testing and ‘scientific method’. How else to explain the extraordinary declaration, ‘ACE-V is a valid scientific method if it is used as a valid scientific method’ (Tripplett and Cooney, 2006, original emphasis)? Indeed, if we note that ‘valid’ is being used here as an honorific (see Haack, 2005, on the use of ‘scientific’ and ‘scientific method’ as honorifics in legal discourse), rather than as a concept related to ‘validation’, we can readily understand why there is a lack of understanding between those who, like the Habers, focus on validation and those more concerned with whether latent print analysis is generally ‘good’.

The interested reader can experience the tortuous nature of these arguments for themselves, but the more important question is why LPEs pursue this argument in the first place? There is little to be gained legally by demonstrating that one follows ‘the scientific method’. Although the Daubert Court did—unfortunately, as I have suggested—include some discussion of ‘the scientific method’, Daubert, especially as clarified in Kumho Tire v. Carmichael (1999), requires that evidence be relevant and reliable—nothing more, nothing less. And, as the Habers note, to say ACE-V follows ‘the scientific method’ is not to say that it is valid. The answer may be that LPEs, when seeking to defend their knowledge claims, turn to ‘method’ instead of validation because to turn to validation, as conventionally understood, would be to consign themselves to research subject roles.

The ‘method’ analogy fits poorly because ‘the scientific method’—leaving aside for the moment the notorious difficulty of adequately defining such a construct (Haack, 2005)—applies best to open-ended disciplinary modes of searching for knowledge about the world, like, say, experimental physics. Validation, meanwhile, applies best to simple assays. A physicist, e.g. would be perplexed by the question ‘Has experimental physics been validated?’ but could readily tell you whether and how their various instruments used in experimental physics had been validated. Likewise, it would be absurd to suggest that a particle detector uses the scientific method. The misunderstanding, in short, hinges on whether the LPE is understood as an experimenter or an instrument.

The sad reality, however, is that even the Habers’ latest articulation of the problem is unlikely to matter, at least to courts. The reason for this hinges not on what the Habers say, but on who they are. As the Habers themselves note towards the end of their article, courts have deemed the opinions of research scientists largely irrelevant to the question of whether latent print individualization is valid. They further note that the number of scientists and scholars who have made some sort of public statement (such as publishing an article, giving conference papers, offering sworn expert testimony or signing an amicus curiae brief) supporting the basic assertion that latent print
individualization has not been validated has grown steadily over the past decade since Stoney (1997) first took this position. More significant is the conspicuous absence of any scientist or scholar, who is not a LPE, taking the opposite position. And yet, courts have not only ignored scholars’ opinions on this matter but also actively endeavoured to exclude research scientists from the ‘relevant scientific community’ (to use the Frye v. United States (1923) terminology) concerning the validity of latent print individualization by restricting that group to those who actually practice latent print analysis. Thus, in Commonwealth v. Patterson (2005), one of the few even partial victories for a defendant in an admissibility challenge to latent print evidence, the Supreme Judicial Court of Massachusetts, while noting that the government had not demonstrated ‘general acceptance’ in the ‘relevant scientific community’ of a very specific application of the technique known as ‘simultaneous impressions’, defined that community narrowly as those who practice the technique. In so doing, the court explicitly discounted both the defendant’s plea that the ‘relevant community’ includes research scientists who had concerned themselves with the evidence of latent print individualization’s validity and the Southern District Court of New York’s opinion in United States v. Starzecpyzel (1995) that disinterested parties, such as academics, should be included in the ‘relevant community’ on the question of whether a practitioner group’s knowledge claims had been properly validated.6

Thus, some courts have essentially taken the position that the opinion of the Habers and all other scholars is irrelevant in the face of practitioners’ confidence in the validity of their own practices. The contempt for scholarship and scholars with which such arguments are framed is sometimes palpable. To offer just one example, a prosecution brief in one case described the demand of validation from a technique with a ‘hundred years of experience of experts in the field’ as a ‘request ... only an academic could make with a straight face’ (State v. Columbus, 2006).7

As I have warned elsewhere (Cole, 2006), the courts’ attitude does not merely fail to encourage practitioners to take an interest in validating the claims they make in sworn testimony, it actively discourages them. As long as courts permit practitioners to express unvalidated claims with absolute confidence in sworn testimony, those practitioners and the prosecutors who employ their testimony have a strong disincentive to perform any validation research as this can only result in diminishing the strength of their testimonial claims. This, of course, renders doubly paradoxical LPEs’ insistence on seeing themselves, rather than external observers, as agents of validation research. This, in my view, is the most troubling backdrop to the painstaking labours that the Habers have undertaken in this article: that unless courts reverse the contempt in which they hold scholarly work and scholarly opinion, it will all count for very little.

Acknowledgements
I am grateful to Christophe Champod and Jennifer Mnookin for their comments on drafts of this commentary.

---

5 This proviso is added to account for the assertions that latent print individualization is valid by André Moenssens, who certainly must be counted as a scholar, but also happens to have a background as a LPE. Moenssens’s arguments have been rebutted at great length in Cole (2006).

6 It is, by the way, no longer ‘merely’ academics who recognize the lack of validation of latent print individualization. An amicus curiae brief filed in Patterson to this effect was signed by a former Federal Bureau of Investigation LPE, in addition to 14 other scientists and scholars, including the author and the Habers (Siegel et al., 2006).

7 In the interest of full disclosure, the academic who made this ‘request’ was the author.
Funding

This material is partially based upon work supported by the National Science Foundation under Grant No. SES-0115305. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily reflect the views of the National Science Foundation.

REFERENCES

Commonwealth v. Patterson, 2005 840 N.E.2d 12 (Mass.).
State v. Columbus, 2006 State’s Memorandum in Support of Admitting Latent Fingerprint Identification Testimony (Minn. Dist. Ct.).


Moriarty, J. C., 2004 Psychological and Scientific Evidence in Criminal Trials. Thomson West, Eagan, MN.


