

**Forensic Identification Science:  
A Summary of the Theory, the Science, and the Legal History**

Michael I. Saks  
Arizona State University College of Law

Note: This paper is an edited and updated version of Saks, *Merlin and Solomon: Lessons from the Law's Formative Encounters with Forensic Identification Science*, 49 HASTINGS L. 1. 1069 (1998), with footnotes removed.

**The Problem of Identification**

Forensic identification science has selected for itself a project that is unknown to other fields: the unique identification of - or, more properly, *individualization* of - various objects, including persons, distinct from all others in the world. "Criminalistics is the science of individualization."

The question posed is whether a bullet can be traced back to the one and only one barrel through which it was fired, a signature to the hand that wrote it, a bitemark to the mouth of the biter, cut bolts to the instrument that cut them, and so on. Affirmative answers are offered daily in courtrooms across the country as firearms examiners, document examiners, forensic odontologists, tool mark experts, and numerous other forensic identification scientists purport to identify *the* gun, hand, mouth, tool, and so on, that left its traces at a crime scene, "to the exclusion of all others in the world. DNA typing is merely the latest addition to the family of forensic identification sciences. Each member of this family subscribes to the same assumptions and draws its inferences from the same basic logic. Typically, testimony based on such identifications is offered to place a defendant at the scene of a crime.

The capacity to make such identifications depends on the validity of a series of premises: That many kinds of biological and physical entities exist in unique, one-of-a-kind form; that they leave correspondingly unique traces of themselves; and that the techniques of observation, measurement, and inference employed by forensic science are adequate to link these traces back to the object that produced them. The claim usually has been presented in essentially this strong form: Individualization is "absolute specificity and absolute identification."

The goal of individualization contrasts with conventional science of virtually every kind. "Individualization is unique to forensic science." Normal science is concerned with grouping objects and events into meaningful classes, discovering systematic relationships among these classes, and developing and testing theoretical explanations for those shared attributes and relationships. While normal science looks only *between* classes, forensic identification science purposefully looks beyond "class characteristics" and looks *within* classes. While normal science is concerned with establishing regularities, forensic science is concerned with exploiting irregularities among objects within classes. Its central assumption is that objects possess enough differences that on adequate inspection one object cannot be mistaken for another.

## **The Theoretical and Empirical Basis of Individualization**

The question arises as to the basis for believing that all things are unique and that individualization is possible. Many forensic scientists are content to assert that no two of various types of objects can be alike, and leave it at that. Thoughtful efforts to justify these claims usually begin with notions from probability theory. But those scholars soon realize that probability theory simply cannot get there from here, and next they look in vain for another route.

For example, after using a manifestly probabilistic thought experiment to defend the proposition that no two fingerprints can be alike, Cummins and Midlo conclude: "It is unfortunate that this approach carries the implication that a complete correspondence of two patterns might occur, when as a matter of fact... such duplication is beyond the range of possibility." Failing to make the case with their best rationale, they retreat to anecdotes, assumptions ("nature never repeats"), and appeals to intuition ("common sense rejects as fantastic the idea" of two being alike). A small but perhaps growing number of forensic identification scientists accept the unavoidable: such identifications are in reality estimates of probability.

Unfortunately, the probabilities employed by traditional forensic identification science are subjective and intuitive. Only the newest of these, DNA typing, takes the burdens of the probabilistic nature of forensic identification science seriously. Only DNA typing collects data calculates the objective probability of a coincidental match. All other forensic identification fields content themselves with intuitive estimates of subjective probability.

Many forensic scientists who conceded the inherently probabilistic nature of their enterprise nevertheless refrained from undertaking data collection and the calculation of empirically based probabilities: "In most [handwriting identification] problems it would be impossible, or at least extremely impractical, to measure mathematically the degree of probability of accidental coincidence. ... These basic conditions prevent arithmetic determination of a probability factor." Others, however, have been distressed by the "almost complete lack of factual and statistical data pertaining to the problem of establishing identity" in their areas, and have started the belated work of building a rigorous foundation for forensic identification science. In contemporary practice, reliance on objective data and computations based on the data are found only in DNA typing.

No articulated theory exists that explains why unique identifiability must be the order of the universe. For most forensic scientists, merely repeating the assertion is sufficient. The origins of the forensic scientist's notion that no-two-are-alike can be traced to Adolph Quetelet, the 19th Century Belgian statistician and sociologist better remembered today as the father of descriptive social statistics. Based on a statistical concept we will examine shortly, Quetelet hypothesized that nature never creates biological duplicates. Alphonse Bertillon, whose influential father obtained a position for him as a minor clerk in the Paris Prefecture of Police, had learned of Quetelet's hypothesis from his father and grandfather, who were students of medicine, statistics, anthropology, and demography. With his father's help, in the early 1880s Bertillon overcame the

print and consults them as part of daily casework) the only way to find false matches would be to conduct special studies to look for them I know of only one such effort, by a document examiner who went looking for signatures from different people that were indistinguishably alike. He found them in abundance. In so doing, he falsified the core claim of handwriting identification. Of course, the best way to avoid finding duplicates is not to look for them As long as one refrains from looking for black swans one's belief that all swans are white is insulated from falsification. More importantly, no one really knows how diagnostic each identification technique is. Only by carrying out appropriate studies could that become known.

In addition, proficiency studies, undertaken only in the past 20 years, revealed varying rates of error. For example, for DNA typing, the error rate has been about 1-2%. For toolmarks as much as 35%. For handwriting identification about 36%. For bitemark identification, the latest data suggest about 60% false positive error. While these proficiency values are open to alternative calculations and interpretations, what is most significant is that these error rates exist at all. Even if forensic metaphysicians were right, that no two of anything are alike, for factfinders in earthly cases, the problem is to assess the risk of error whatever its source, be that in the basic theory or in the error rates associated with human examiners or their apparatus. That information can best be evaluated and supplied by placing the testimony in a probabilistic context that combines proficiency data, what is known about population baserate, and whatever else is relevant to assessing the probativeness of the testimony.

In light of the above, the finding that forensic science errors rival eyewitness errors as a cause of erroneous conviction of the innocent may not be so surprising. See the following table.

#### Factors Leading to Wrongful Conviction in about 100 DNA Exoneration Cases

Eyewitness errors	74
Forensic science	
Errors	66
Fraud, tainted evidence	31
Police misconduct	44
Prosecutorial misconduct	28
Bad Lawyering	19
False confessions	19
Dishonest informants	17
False witness testimony	17

## A Science Constructed in the Image of the Criminal Law

Forensic science plainly has something of value to offer criminal investigators and the courts. Why, then, does so much of it cling, instead, to an untenable absolutism and committed subjectivity? By contrast, conventional science would have proceeded along a different course, one guided by the necessity of collecting and analyzing data to test assumptions. In court, conventional scientists might be expected to share with the factfinder the analytic basis of their opinions, their data, and their data-based assessments of the risk of error. In short, conventional scientists would collect better data and offer them to the courts with far less exaggeration. Why doesn't forensic science proceed along that more recognizably scientific path?

The answer likely is that forensic science grew up in the criminal law. The exigencies imposed on it by police and prosecutors molded it into its contemporary shape. A particularly dramatic demonstration of this is the lengths to which some forensic scientists have been willing to go to provide courts with the testimony prosecutors wanted courts to hear, regardless of the truth. Paul Giannelli has summarized an array of fraudulent science, faked tests, and perjured testimony. But one need not look to such scandalous examples to find the influence of the adversary process at work. Consider the following demands under which forensic science has been required to operate.

In order to win a conviction, the prosecution must prove its case "beyond a reasonable doubt." If the forensic scientist testifies: "I cannot tell these questioned and known evidence items apart, so they probably share a common origin, but of course this is only a subjective estimation based on intuition, because we've never mapped the distribution of what is out there" or "based on our sampling of the population we calculate the probability of a coincidental match to be at the following level of probability," room is left for some doubt. But doubt vanishes if the forensic scientist can say something along these lines: "Because the questioned and the known look alike, and because each person's or object's marks are unique in all the world, I can state with certainty my opinion that the defendant left the markers found at the crime scene)."

Gaps would be seized upon by the defense, which would argue to the jury: "If there is even *one* other match out there, that makes two people who might have done it, only one of whom is my client; that implies a 50:50 chance that someone other than my client is guilty. Surely you cannot regard that as guilt beyond a reasonable doubt." Courts have reversed convictions on the reasoning that a merely rare probability is not sufficient to prove guilt. From a litigator's viewpoint, there is a world of difference between saying this is *a* match and saying this is *the* match. What modern scientists do as a matter of course -- measure the risk of error

presents serious problems for prosecutors and therefore for forensic scientists.

Because of its institutional position within the legal system, the forensic identification sciences have taken on a shape that resembles no other science. Consider these special attributes: No other fields are as closely affiliated with a single side of litigation as forensic science is to criminal prosecution. Police crime laboratories were not begun in order to provide science for police and courts, but as public relations device. Even today, few of the personnel of crime laboratories have scientific training beyond the undergraduate level, and some not even that. Crime laboratories generate very little research, which to a scientist means they are not doing science, and to a lawyer should say at least that little progress is being made. At best, they apply science, but even that often is not the case. Progress might come from their colleagues in industrial or academic departments. But there are no industrial uses of what forensic identification scientists do. And the number of university programs to train forensic scientists can be counted on one's fingers. The maldistribution of forensic scientists so favors the prosecution that the defense has little access to any, which prevents the adversary process from working as intended to expose error. The institutional setting of forensic science promotes habits of thought that more closely resemble the thinking of litigators than of scientists. While science pursues knowledge through falsification, prosecutions are won by confirmatory proofs. This confirmatory bias dominates the thinking of most forensic scientists. Where science advances by open discussion and debate, forensic science has been infected by the litigator's preference for secrecy. Tests of the proficiency of crime laboratories are conducted anonymously, kept secret, and are not routinely published. It is ironic that while the effectiveness and accuracy of so many professional enterprises are available in published literature, the same is not true of a field whose sole purpose is to do some of the public's most public business.

#### Judicial Assessments of Forensic Science

In light of the weaknesses in the fundamental science of most forensic identification techniques, or the surprisingly high error rates uncovered by proficiency testing, one might wonder how these fields came to be approved for use by the courts that initially evaluated them. The short answer is that the forensic identification science evidence of all types were mostly passed through without much scrutiny. As a result, modern re-evaluations under *Daubert v. Merrell Dow* and *Kumho Tire v. Carmichael* have confronted these fields with far greater challenges to their admissibility than they ever before experienced.

#### Handwriting Identification

While by 1900 a substantial majority of American jurisdictions accepted such testimony, the prevailing attitude may be best exemplified by the opinion of the New York Court of Appeals in *Hoag v. Wright*, where the court said:

The opinions of experts upon handwriting, who testify from comparison only, are regarded by the courts as of uncertain value, because in so many cases where such evidence is received witnesses of equal honesty, intelligence and experience reach conclusions not only diametrically opposite, but always in favor of the party who called them.

case. Nowhere in the opinion did the court articulate the basis of the expertise it was evaluating, or discuss any scientific evidence bearing on the empirical claims, or illuminate the technique's theoretical premises, or explain why anyone should believe that fingerprint examiners can do what they claim the ability to do. Nor do the cited sources fill that gap. In addition, the court also referred to four experts who testified on behalf of fingerprint identification, each of whom had been studying or practicing fingerprint examination for three to four years before the trial. But the court's opinion shares nothing of what, if anything, these witnesses had to say on the validity of fingerprint identification.

The second American case to consider the admissibility of fingerprint evidence, *State v. Cerciello*, neither cited nor explained anything whatsoever concerning the expertise at issue. This court's scientific assessment was nothing more than a generalized endorsement of scientific progress:

[I]ts admission as legal evidence is based upon the theory that the evolution in practical affairs of life, whereby the progressive and scientific tendencies of the age are manifest in every other department of human endeavor, cannot be ignored in legal procedure, but that the law, in its efforts to enforce justice by demonstrating a fact in issue, will allow evidence of those scientific processes which are the work of educated and skillful men in their various departments....

So much for the *Cerciello* court's scientific reasoning. The court's legal reasoning amounted to this: the admission of expert opinion, "one of the prominent exceptions of the general rules of evidence," was permitted on so many other matters that the court could hardly refuse to permit another exception. Moreover, the jury could be relied upon to give the testimony whatever weight was appropriate.

The *Roach* court did no more than to cite *Castleton* and *Jennings*. Concerning the latter, the *Roach* court commented, credulously: "The opinion of Chief Justice Carter in that case contains an instructive and learned discussion of this whole subject." The opinion offers no citations to any scientific materials or any discussion of the principles claimed to be the foundation of the technique. The court focused on the "qualifications" of the witness rather than the content of the science. The court held: "In view of the progress that has been made by scientific students and those charged with the detection of crime in police departments ... we cannot rule as a matter of law that such evidence is incompetent."

In Texas, *McGarry v. State* rested its opinion squarely on *Jennings*, literally adopting the Illinois opinion as its own. After quoting at length from *Jennings*, *McGarry* held simply: "We conclude that the evidence of the witness was admissible."

The quality of judicial scrutiny of fingerprint evidence rarely exceeded that of *Jennings*, and sometimes it fell far shorter. While some cases made reference to actual early research studies on fingerprints, others cited Mark Twain's novel, PUDD'NHEAD WILSON, as authority for the infallibility of fingerprint evidence, or appealed to far higher authority:

"God's finger print language," the voiceless speech, and the indelible writing on

pieces were cut by the same knife." On rehearing a year later the Court affirmed its initial rejection of tool mark identification expertise.

Eighteen months after deciding *Fasick* and only six months after re-affirming itself, in *State v. Clark*, the Washington Supreme Court was presented with remarkably similar evidence, this time in the context of a rape case in which fir boughs and saplings had been cut and used by a rapist to construct a blind from which to attack his victim. Again a knife, again cut fir branches, again Luke May the proffered expert witness. But this time the Court held that expert opinion about whether the defendant's knife cut the branches was admissible: "The photomicrographs... conclusively establish, we are convinced, as doubtless the jury were, that the cuts were made with the same blade. "

There is little if anything in the *Clark* opinion that could help a judge in a subsequent case to understand why May's claims about a science of tool mark identification were valid. The case merely offered the conclusory and unexplained enthusiasm of a Court that only months before had rendered an opposite opinion on the very same question. Both the exclusion of the purported science in *Fasick* and its admission in *Clark* are unexplained.

Even taking into account subsequent judicial opinions, the body of legal authority is remarkably sparse and uninformative. While it is apparent that expert evidence on toolmarks and firearms identification is universally admissible today, it is equally obvious that this universal admissibility has been accomplished without judicial evaluation of the validity of the underlying science or its application.

### Voiceprints

The claim of "voiceprint" identification is that each person's vocal apparatus is unique and therefore the voice sounds each person makes are unique. By converting voice sounds to a visual display, using spectrographic equipment, the examiner can compare tracings and determine the identity of the person who belongs to the questioned voice.

Judicial opinions on the admissibility of such voice identification expertise are widely divided. By a recent count, expert testimony based on voice spectrographic analysis is admissible in six states and excluded in eight; admissible in four federal circuits and excluded in one. No consistent or coherent judicial view has evolved over time nor does a consistent view appear likely to emerge in the foreseeable future. The history of the courts' divergent responses to voice spectrography is instructive.

First, there is no greater agreement in recent years than there was in the earliest days of voice spectrography. Of the first ten courts to consider the technique, six admitted it and four excluded it. The most recent ten to consider it were evenly divided on admission or exclusion.

Second, we can see that the legal test of admissibility applied by the courts is highly correlated with the holding to admit or exclude. Of the courts that claimed to apply the *Frye* test in a broad fashion - that is, treated the relevant scientific community as consisting of a range of relevant fields and not merely the one narrowly concerned with performing the particular

After the 1970s, there was no longer any debate in the courts - if ever there had been about the admissibility of bite mark expert opinions to identify perpetrators of crime. Every subsequent appellate case concerning the issue has held bite mark identification testimony to be admissible. The rationale for admission in all of these cases has followed one of two general approaches. Some courts have followed the *Marx* precedent in holding that bite mark identification is generally accepted within the relevant scientific community. Others simply held that whether to admit such expert opinion lies within the discretion of the trial court.

Perhaps the most unusual legal development in these cases has been a recent appeal by the defendant convicted in *People v. Milone*. Paroled after serving nearly twenty years in prison for murder, Milone continued to insist upon his innocence and sought legal redress to clear his name. In federal court, under both the *Frye* and *Daubert* standards, he challenged the original decision to admit the expert bite mark testimony. Another murder victim was later found in the same area where the victim in the Milone case had been found. A potential bite mark from the second murder victim was linked to one Macek. The marks in the two cases were judged by at least one forensic odontologist to be indistinguishable from each other. The Court of Appeals for the 7th Circuit expressed sympathy with Malone's request, in light of the new evidence presented, but declined to rule on the case for lack of a constitutional basis for granting relief, and because principles of federalism precluded a federal court from re-examining an issue of fact that is reserved to the state courts.

The bite mark cases suggest that the quality of a field's science can have less impact on the courts' decisions about the field than the courts' decisions about the field have on the field's beliefs about itself. As a field forensic dentists appreciated the magnitude of the challenge of unique identification better than most identification scientists before them, and the need for but lack of data supporting the opinions they would like to be able to offer, and therefore came to court with admirable self-doubt. But the pivotal opinions like *Marx* and *Milone* swept aside those doubts. The pivotal opinions also did not find admissibility doctrine to present much of a barrier. Sharp disagreement within the field about their capability to individualize did not prevent courts from finding general acceptance.

## **Conclusion**

Remarkably, serious scientific research on the fundamental claims of most areas of forensic identification science lies in the future, because the past is nearly devoid of it. More remarkably, the chief source of motivation for these areas of forensic science to overcome their neglect of science has been today's courts applying new law. Perhaps that is not so surprising, considering that it was the courts which, during most of the previous century, by their own indifference to science, led these forensic "sciences" to believe that science, research, data, empirical evidence were not required for admission. As long as empirical testing was not required, none was conducted. But in the wake of *Daubert* and *Kumho Tire*, it appears that in the future courts will require far more of the forensic sciences than they did in the past.

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FOOLPROOF FORENSICS?  
EVEN SCIENCE MAY NOT MAKE VERDICTS INFALLIBLE

Beth Daley GLOBE STAFF

In proposing a new death penalty for Massachusetts last month, Governor Mitt Romney offered firm assurance that no innocent people would be executed: Convictions, he said, will be based on science. According to the proposal, a death-penalty verdict would require not only an especially heinous crime, but also "conclusive scientific evidence" of guilt. If it passed, Massachusetts would become the first state to require a scientific link to a crime to impose a death sentence.

Romney's plan, however, comes at a difficult time for courtroom science. Much scientific

D. Michael Risinger  
(excerpt from *Modern Scientific Evidence*)  
Analyzing *U.S. v. Prime*

A much more puzzling opinion was issued by Judge Lasnik in *United States v. Prime* [220 F. Supp. 2d 1203 (W.D. Wash. 2002)] and it is puzzling for unusual reasons. This was not the usual off-hand opinion which characterizes many of the decisions admitting handwriting identification testimony. The charges against Michael Prime involved counterfeit U.S. postal money orders. The prosecution intended to call Kathleen Storer, a forensic document examiner for the U.S. Secret Service, to testify that Prime's handwriting "appeared on counterfeit money orders and other documents." Later in the opinion, the court indicates that what Ms. Storer was asked to evaluate for authorship was made up of writing on 78 different documents (76 covered by her first report and 2 by her second report), including envelopes, money orders, "post-it notes," express mail labels and applications for postal boxes. Some of the writing was cursive and some handprinting. Ms. Storer was given extensive known writings of three persons (Prime and co-defendants Hiestand and Hardy), and asked if any of them wrote any of the writing on the 78 documents. She found that Hiestand wrote "portions of eight documents" and Hardy wrote "portions" of one document, and Prime wrote "portions" of 45 documents, and "probably" wrote "portions" of 14 more. The documents contained 38 signatures, the authorship of which Storer indicated "could not be determined." Given these facts, it is obvious that, even in the opinion of Storer, the questioned writings presented multiple tasks, and the record is unclear concerning the extent of the writing involved in each separable task. This is important, because, though the court is one of the few to manifest an awareness, at least formally, of its obligation under *Kumho Tire* to evaluate reliability in regard to the specifics of the particular case, *id.* at 1210-1211, it later functionally lumps all of the tasks performed by Storer into a single task for purposes of its analysis.

The first three sections of the court's opinion are (except perhaps for a few details) unexceptionable, even admirable. Section I analyzes Daubert and Kumho Tire, recognizing that, while they speak of "flexibility" and "discretion," the focus of that flexibility and discretion (its "overarching subject") is "evidentiary relevance and reliability," and that a flexible standard "does not imply a lax one." One objection to the first part of the opinion is that, having said that the standard ought not to be lax, the court then implies that laxness is cured by the "equal intellectual rigor" test. *Id.* at 1205. Of course, that test tests nothing when it is the accuracy of the group practice itself that is being evaluated. Section II recounts the history of handwriting identification expertise in the federal courts, pre- and post-Daubert, including a very thoughtful examination of Judge McKenna's opinion in *Starzecpyzel*. It quite fairly notes that the Court of Appeals decisions are generally simply affirmations of whatever the district courts have done pursuant to the abuse of discretion standard of *General Electric v. Joiner*, and therefore are of limited use as pronouncements on actual reliability. It further notes that the results in the District Courts have been "uneven," with some admitting the proffers without restriction, some excluding them, and some admitting them with restriction. It then turns to the application of the Daubert/Kumho requirements to the facts of *Prime*.

The court's first decision was to properly reject the defense's argument that the 1998 global skill to be globally evaluated

Johnson v. Commonwealth:  
How Dependable Is Identification by Microscopic Hair Comparison?

Michael I. Saks

A professor from Harvard Law School once gave a speech to the Association of American Law Schools in which the professor cautioned that legal education had a deleterious effect on a person's ability to think sensibly about reality and how to evaluate claims about the material world. Suppose, said the professor, you asked the average young adult whether the moon was made of green cheese. Before law school, a typical response would be: "I don't know; we probably need to get hold of a piece of it in order to find out." To a law school graduate, however, the answer, "It could be argued that the moon is made of green cheese," would seem entirely adequate. If you do not already realize that *Johnson v. Commonwealth*, 12 S.W.3d 258 (1999), confirms the Harvard professor's worst fears, you will by the end of this case comment. But first we need some legal background.

Admissibility of Scientific Evidence in American Law

Before *Frye v. United States*, 293 F. 1013 (D.C. Cir. 1923), courts used the "marketplace" test. In trying to determine whether a proffered expert witness had valid opinions to offer, courts of the 19th Century asked themselves whether, in the commercial marketplace, consumers of that expertise found its opinions and advice worth purchasing with their hard earned money. If the expertise were valued in the marketplace, then courts also were willing to value it and allow it as expert testimony. Thus, consumers of an asserted expertise were the principal judges of its validity.

As the *Frye* court realized, such a test was impossible to apply to expertise that had no life in any commercial marketplace, such as a field that was invented exclusively for forensic purposes. So, when confronted with the need to determine whether a proffered polygraph expert had a valid basis for his opinions, the *Frye* court employed an analog to the commercial marketplace: the intellectual marketplace. The court asked not whether an expertise enjoyed general acceptance among consumers, but whether the expertise had gained general acceptance "in the particular field in which it belongs." Thus, the *Frye* test replaced consumers with producers as the principal judges of validity.

*Daubert v. Merrell Dow Pharmaceuticals*, 509 U.S. 579 (1993), of course, made judges the principal judges of the validity of proffered expertise. And it called upon those judges, when confronted with empirical claims to assess, to think like scientists: Are the claims testable and have they been tested? Have those tests been conducted using sound research methodology (perhaps the central lesson of the paragraph in *Daubert* that begins with the words "peer review and publication")? What do the findings of well designed studies reveal? "General acceptance" still can be considered, but with a scientist's skepticism - as the Supreme Court later made clear in *Kumho Tire v. Carmichael*, 526 U.S. 137 (1999), when it observed: "Nor... does the presence of *Daubert's* general acceptance factor help show that an expert's testimony is reliable where the discipline itself lacks reliability." So, general acceptance within a field counts for something only

after the field has been otherwise determined to be a sound one. To have held otherwise would have been to allow *Frye* to swallow *Daubert*.

The essential logic of *Daubert* and its progeny could not be simpler. What better way is there to find out whether something works or not than to empirically test it? And then to look at the quality of those studies and what the results of good studies show. If you had a serious illness and you wanted your doctor to recommend an effective treatment, would you want your doctor to choose a treatment whose sellers assure buyers that the treatment they are selling is terrific? (*Frye*.) Or would you want your doctor to look at the research literature testing what works and suggest something to you that has been demonstrated to be effective? (*Daubert/Mitchell*.)

The essence of *Daubert's* gatekeeping task is to look at the research offered by the proponent on the specific "task at hand" in the case at bar and see what it shows. If the research satisfies the court that the expertise is sufficiently dependable, it is to be admitted. If the research fails to establish that the expertise is dependable, it is to be excluded. The proponent of expert evidence that has no research or minimal research or weak findings to support it cannot gain admission - the fate of any party bearing the burden of proof but who is unable to meet its burden.

The U.S. Supreme Court plainly realized that there were many old kinds of asserted expertise that had won admission under *Frye* (or had been admitted without being subjected to any test at all), and which would be challenged under *Daubert* as they never had been challenged before:

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. Of course, well-established propositions are less likely to be challenged than those that are novel, and they are more handily defended.

As one federal court later observed:

[*Daubert*] may mean, in a very real sense, that "everything old is new again" with respect to some scientific and technical evidentiary matters long considered settled. Alarmists may see this as undesirable. The more probable outcome is that judges, lawyers and expert witnesses will have to learn to be comfortable refocusing their thinking about the building blocks of what truly makes evidence that is beyond the knowledge and experience of lay persons useful to them in resolving disputes. The beneficiaries of this new approach will be the jurors that have to decide increasingly complex cases. *Daubert*, *Kumho Tire*, and now Rule 702 have given us our marching orders, and it is up to the participants in the litigation process to get in step. *United States v. Horn*, 185 F.Supp.2d 530 (D.Md. 2002).

A field that has the right stuff, and has done its scientific homework, would have no trouble demonstrating that what it is selling is worth buying. If its claims are true, its adherents should

have no trouble showing that to be so. But a field that has been engaged in a parody of science, dressing up in lab coats but never doing the research needed to test the extent and limits of its claims, and making claims that exaggerate what is known about its subject matter and its own skills, such a field would have the gates closed to it - unless and until it can demonstrate the validity of its claims.

#### Admissibility of Scientific Evidence in *Johnson*

At trial in *Johnson v. Commonwealth*, counsel for the defendant challenged the admissibility of hair identification expert testimony under *Mitchell v. Commonwealth*, 908 S.W.2d 100 (1995), Kentucky's adoption of *Daubert*. In light of *Daubert*, and presumably of *Mitchell*, one would have expected the trial court simply to require the proponent to demonstrate the soundness of what it was proffering. A field of supposed science that has been in business for a century ought by now to have a mountain of studies about its subject matter and itself with which its claims, if valid, could be "handily defended." But the trial court did something even simpler: it refused to require any showing by the proponent, and denied the opponent's motion to exclude. The reason for that decision, apparently, was that microscopic hair comparison had long been admitted in Kentucky - though obviously not under the test now required by Kentucky law.

The trial court's failure to place the burden of proof on the proponent of the admission of evidence, and then to admit the testimony without the proper showing having been made, would seem to be an elementary error. That the proponent of evidence has the burden of establishing that its proffer meets the requirements for admission is a quotidian legal notion. (The rule is ancient, followed (almost) universally, fair, and efficient. Where the proffer is an asserted expert in some assertedly scientific field, the proponent has the proposed witness who has the supposed knowledge that will answer the question.)

On review by the Kentucky Supreme Court, however, the trial court's refusal to require the proponent to "show us the data" was upheld as a proper application of *Dauber/Mitchell*. To reach this result, the Court had to explain how it could be that an asserted expertise, never before tested under *Daubert/Mitchell*, did not need to be tested, and could be found "scientifically reliable" without any court ever doing the one thing that was the touchstone of the new test: looking at the data.

FIRST, the court argues that well established findings of science need not be revisited and proven over and over again. This seemingly sensible view not only strikes a blow for judicial efficiency, it spares courts the intellectual burden of a task which is fundamentally difficult for many of them, namely, evaluating the validity of asserted scientific claims. In the words of the *Johnson* Court:

*Daubert* also recognized that some scientific methods, techniques and theories are so firmly established as to be proper subjects of judicial notice pursuant to FRE 201 (b)(2). Thus, in *United States v. Martinez*, it was held that once an appropriate appellate court holds that the *Daubert* test of reliability is satisfied, lower courts can take judicial notice of the reliability and validity of the scientific method, technique or theory at issue. Courts are "right to admit or exclude much evidence

without 'reinventing the wheel' every time by requiring the parties to put on full demonstrations of the validity or invalidity of methods or techniques that have been scrutinized well enough in prior decisions to warrant taking judicial notice of their status." 3 C. Mueller and L. Kirkpatrick, *Federal Evidence* § 353, at 657 (2d ed.1994). (Case citations omitted.)

The trouble is that none of what those authorities were talking about is present in *Johnson*.

The cited footnote in *Daubert* was referring to overpoweringly well tested and thoroughly confirmed findings or principles: "theories that are so firmly established as to have attained the status of scientific law, such as the laws of thermodynamics, properly are subject to judicial notice." Is microscopic hair comparison on a par with the laws of physics? Are the principles of hair comparison "scientific laws"? Indeed, the gravamen of the cited footnote in *Daubert* should have taken the *Johnson* Court in the opposite direction, concluding not that scrutiny of the claims of hair examiners can be dispensed with but rather that "well-established propositions... are more handily defended" and must be defended. If the claims of microscopic hair identification are sound, they can easily be shown to be so. But if they are not, they will fail. And the only way a court can determine whether the claims are sound is going to be to require the data to be presented at a proper hearing.

The *Martinez* court is talking about a situation where an appellate court holds that the requirements of *Daubert* have been properly satisfied in other proceedings, so that a later district court does not have to go over the same territory redundantly. But that is not what happened here. There had been no prior hearing at which the claims of microscopic hair identification had been rigorously scrutinized and the requirements of *Daubert/Mitchell* found to have been met. Moreover, both the earlier court and the court seeking to forego a *Daubert* hearing would both have to have been sufficiently careful in framing the task-at-hand that a reviewing court could be sure that the data reviewed in the former hearing fit the task at hand in the latter case. In *Johnson*, for example, a single questioned hair was available to work with. Had a previously conducted *Daubert/Mitchell* hearing addressed itself to *that* difficult evidence situation? (As we know, there was no such hearing in a sister court addressing itself to anything.) A finding of nothing more than general acceptance would not have sufficed, given the touchstone requirements of *Daubert*, elaborated in *Kumho Tire*, disapproving of general acceptance as a continuing substitute for a review of the relevant research data. As Justice Scalia emphasized in his concurrence in *Kumho Tire*: "Though... the *Daubert* factors are not holy writ, in a particular case the failure to apply one or another of them may be unreasonable, and hence an abuse of discretion. "

If the Kentucky Supreme Court is saying that an appellate court can make the substantive decision for the trial courts, then it is saying that review is *de novo*. If that is the rule in Kentucky, then the Court must conscientiously conduct its own review of the research literature and explain what it found there. (The U.S. Supreme Court, of course, has gone in the opposite direction, holding in *General Electric Co. v. Joiner*, 522 U.S. 136 (1997), that appellate review is deferential. In practice, federal district courts revisit uncertain sciences repeatedly, working their way toward a clearer understanding and an eventual consensus.) But, in any event, the Kentucky Supreme Court undertook no such *de novo* review of the question of the validity of the claims of

hair identification examiners.

Similarly, the language quoted from Mueller & Kirkpatrick is drawn from the midst of a discussion of how courts are to meet their obligation to ensure the validity of proffered science, and how they might do so efficiently. A court's first obligation under *Dauber/Mitcheli* is not to be efficient but to ensure the validity of the proffered science. The *Johnson* Court seems so intent on sparing judges the burden of "reinventing the wheel" that it is willing to spare them the burden of inventing the wheel altogether.

SECOND, the Kentucky Supreme Court argues that the question of scientific reliability and validity of hair comparisons is something Kentucky courts can learn about through judicial notice, and that the taking of judicial notice shifts the burden of proof to the opponent of evidence admission.

Part of the problem here is that there are two kinds of facts that can be judicially noticed, serving different purposes and accompanied by different standards and procedural requirements. The Court is contradictory about (and probably confused about) which kind it is trying to invoke as the vehicle for relieving the proponent of admission of the obligation to meet its burden of actually proving that the claims of hair comparison can be trusted.

Initially, the *Johnson* Court quotes *Daubert's* invocation of "judicial notice pursuant to FRE 201 (b)(2)" as the magic wand for this job. But FRE 201(b)(2), like KRE 201 (b)(2), is a reference to "adjudicative facts," which are facts specific to the immediate parties in the case at bar. An adjudicative fact, in order to be judicially noticed, must be "not subject to reasonable dispute in that it is... capable of accurate and ready determination by resort to sources whose accuracy cannot reasonably be questioned." Where are these indisputably accurate sources on the scientific reliability of hair comparison? The Court seems to realize there aren't any, because it next looks for a way to dispense with the indisputability requirement of 20 1 (b)(2). The escape hatch is found in some language of the Study Committee Commentary to KRE 201, which says that "a matter need not be beyond dispute to be part of a court's reasoning." How can that be? The Rule requires in disputability, but the Commentary says it's not necessary? The answer is that the Study Committee's Commentary, borrowing from the Federal Rules Advisory Committee Comments, is talking about a completely different realm of facts, "legislative facts," facts a court (like a legislature) relies on when it is making law. Rule 201 is about adjudicative facts at trial. The Commentary is talking about legislative facts in judicial lawmaking.

So is the *Johnson* Court making law (legislative facts) or finding facts (adjudicative facts)? The opinion incoherently and paradoxically does both at once. By relieving the court below, and itself, of the obligation to take judicial notice only of facts that are "indisputable," it seems to be making law through a finding of legislative fact. But by declaring that "trial courts in Kentucky can take judicial notice that this particular method or technique [of hair comparison] is deemed scientifically reliable," as a matter of trial court discretion, rather than announcing a rule of law which courts below are obligated to follow, the Court is behaving as if it is making a much more limited finding of adjudicative fact. The opinion probably seeks to have it both ways because only that mixture of language, contradictory though it may be, seems capable of excusing Kentucky courts from doing the work they committed to doing when *Daubert/Mitchell*

was adopted.

Relatedly, the *Johnson* Court reassigns the burden of proof from the proponent of admission to the opponent: Judicial notice relieves the proponent of the evidence from the obligation to prove in court that which has been previously accepted as fact by the appropriate appellate court. It shifts to the opponent of the evidence the burden to prove to the satisfaction of the trial judge that such evidence is no longer deemed scientifically reliable." We have to t1)' to make our way through some confusion. The burden of persuasion cannot be shifted *because of* judicial notice. Judicial notice is one way for a party to meet its burden of production, and perhaps of persuasion. The burden of persuasion normally (and almost invariably) starts and stays on the proponent of admissibility of evidence. The proponent might try to satisfy its burden of production by saying: we ask the court to take judicial notice, etc., etc. And the court could agree to do so or decline. And at the end of the hearing (if there had been a hearing) the fact judicially noticed might also be found to satisfy the proponent's burden of persuasion. If the burden of persuasion has been shifted, for some reason, from its customary place on the shoulders of the proponent, to the opponent, the court never explains why.

One would have thought that, given a challenge to a form of expert evidence that had never passed muster under the newly applicable rule, the burdens of production and persuasion remained on the proponent of the evidence. And that the opponent's pointing out the absence of any testing under *Daubert/Mitchell* would have been more than sufficient to trigger a 104(a) hearing where the proponent could present the evidence it has to present, or request the taking of judicial notice, in its effort to meet its burden of persuasion. For example, in *Jacobs v. Government of the Virgin Islands*, 53 Fed.Appx. 651,2002 WL 31887857 (3rd Cir. 2002), the Government was silent in the face of a challenge to the admissibility of its proffered fingerprint expert evidence, and for its failure to meet its burden, the court ruled the expert testimony inadmissible. The taking of judicial notice would shift the burden of production over to the opponent, but not the burden of persuasion. The Kentucky Supreme Court, however, says that somewhere along the way, for some unspecified reason, the burden of persuasion was shifted to the opponent and the opponent failed to meet its burden to prove that hair comparison evidence is unreliable.

FINALLY, whatever the legal nature of the fact it seeks to take judicial notice of, the Court zeroes in on the one key fact the decision is designed to turn upon: that microscopic hair comparison is "generally accepted." How does the Court show that hair comparison is "generally accepted" in Kentucky? Easy. It cites five earlier Kentucky cases, two from the 1950s and three from the 1970s, which upheld the admission of microscopic hair comparison expert testimony. Well, actually that's not so easy. Because, as the Court acknowledges:

Although we have never specifically addressed the scientific reliability of this method of hair analysis, we must assume that it at least satisfied the *Frye* test of general acceptance; for otherwise, the evidence would never have been admitted in the first place. The absence in our previous opinions of any in-depth analysis under the "general acceptance" test was probably due to the overwhelming acceptance of this procedure as a reliable scientific method for the past fifty years.

Let's play that back in slow motion so we do not miss what is happening. Prior decisions had "never specifically addressed the scientific reliability of this method of hair analysis." Then how can they now be used to establish its scientific reliability? Prior decisions did not engage in "any in-depth analysis under the 'general acceptance' test." The Court is too generous. The truth is that not one of the cited cases engaged in *any* analysis of admissibility of any kind or even mentioned *Frye* or general acceptance or any other legal test to which microscopic hair comparison expertise was being subjected. Indeed, it is not evident that a challenge to admissibility under Rule 702 or its common law equivalent was even raised in any of these cases (the closest to it was a cryptic mention in a case from 1950), so those courts might have had no occasion to conduct such an analysis. In any event, if none of those prior decisions found general acceptance, how can they now be used to establish general acceptance? The *Johnson* Court assumes: "we must assume that it at least satisfied the *Frye* test of general acceptance; for otherwise, the evidence would never have been admitted in the first place." That is a very shaky assumption. As already noted, it does not appear that a challenge to admissibility of the hair experts was even lodged, so the courts would have had no occasion to test the asserted expertise under general acceptance or any other test. If challenges had been raised, and each of these courts admitted anyway, without conducting a *Frye* or any other admissibility test, they would be doing what most courts have done over most of the 20th Century with most scientific evidence, especially government proffers of forensic "science." *Frye* itself was an obscure test, ignored for decades. As one scientific evidence treatise notes, judges did not have much "interest in the *Frye* test until a few years before the promulgation of the Federal Rules of Evidence." That was 1975. "By the 1980s, it was being cited as much each year as it had been in its first fifty years. The first citation to *Frye's* general acceptance test in an opinion by a Kentucky court occurs in 1983.

The *Johnson* Court insists that the absence of any analysis under the general acceptance test "was probably due to the overwhelming acceptance of this procedure as a reliable scientific method for the past fifty years." What can one say about such a statement? The force - indeed, the very legitimacy - of courts depends on what their opinions say. If there is no argument or reasoning or even mention in an opinion about a matter central to what is at issue, then the opinion has nothing to say on that point, and it must be resting its holding on something else or on nothing. The issue in *Johnson* simply does not appear to have been an issue in those cases. But, never mind, the *Johnson* Court can just invent the missing pieces. That the *Johnson* Court fills in what is missing would not be quite so astonishing if it were not the central issue in the decision - as the Court itself has framed the inquiry. And the Court is willing to just make up what it needs out of thin air.

Next, and finally, the court cites 10 opinions from other jurisdictions, which it tells us "specifically hold that human hair analysis by microscopic comparison is an accepted and reliable scientific method or technique." The Court says nothing about the contents of those opinions, to demonstrate the quality of the evidence and reasoning of those opinions, or even what issue they were addressing. Were they inquiring into general acceptance or something else? If general acceptance, was the inquiry a narrow *Frye* test (accepted by those who make their livings doing what they say they accept) or a broad *Frye* test (accepted by a wider range of relevant scientific disciplines)? Before abandoning *Frye* for *Daubert*, did Kentucky subscribe to a broad or a narrow *Frye* test? Which approach maps onto *Daubert/Mitchell*? (Given the *Daubert* trilogy, it is hard to believe that the narrow approach to general acceptance is still viable.) Seven

of the ten opinions were from the era before *Daubert* (and all ten of them, of course, preceded *Kumho Tire*, though one might hope that the three that followed *Daubert* were alert to *Daubert's* logic, and did not need to wait to be told by *Kumho Tire* that general acceptance alone does not save a field that has no other demonstrable validity). The Court's best chance for convincing us that those ten opinions mean something more than its own fanciful readings (mindreadings might be more apt) of the five prior Kentucky opinions would have been to *tell us what there is to learn about the general acceptance of hair comparisons from those opinions*. But the Court does no more than to cite them.

## Conclusion

*Johnson* is a rare opinion - at least we might hope that it is rare. First, the Court excised from *Daubert/Mitchell* every element that makes it the new test that it is, and built the opinion's entire analysis on the weakest and most suspect element, general acceptance. Then, instead of requiring a fresh examination of the general acceptance of hair identification (among hair comparison examiners? more broadly among real scientists?) pursuant to the new law or in light of new scientific findings, the Court merely looked at its own cases from generations ago. Upon finding in those opinions no inquiry into the issue of general acceptance, the Court imagined that the reason for silence was that the technique was so obviously accurate and dependable that those courts felt no need to say so. (The exact contrary seems far more likely: if a challenge had been raised, and if those courts were aware of information supporting admission, they would have eagerly referred to it. Since they did not, either there was no such information or they had no need to mention it because no proper challenge had been raised. And that would make *Johnson* a case of first impression on the question of hair comparison for an appellate court in Kentucky. )

What is most paradoxical about the opinion is that, pursuant to new law which plainly conditions admission of expert evidence on a scientific-minded appraisal (that is, a look at the relevant empirical data) of the expertise at issue, the conclusion that hair comparison is "scientifically reliable" is arrived at without any judge at any time having to look at any studies or data whatsoever. Nothing could be more at war with the letter or spirit of *Daubert*.

Had any court at any stage in the process of considering this challenge merely pulled from a library shelf some recent publications about hair comparison (entirely appropriate if a court is in the process of taking judicial notice of legislative facts) it might have found the following textbook views of the reliability of identification by hair comparison:

In an exclusionary mode, hair is a rather good form of evidence. If the evidence hair is blond, straight, and twelve inches long, it may be emphatically eliminated as having originated from a person whose exemplar hair is black, curly, and two inches long. In an inclusionary mode, however, hair is a miserable form of evidence. The most that can be said about a hair is that it is consistent with having originated from a particular person, but that it would also be consistent with the hair of numerous other people. Stronger opinions are occasionally expressed, but they would not be supportable.

The authors of that passage, Thornton & Peterson, both prominent forensic scientists, writing in 2002, also state that, in the view of most forensic scientists, the reliability of hair comparison is "very low," and they therefore rate vulnerability to a *Daubert* challenge as "high."

Less than six months after the Kentucky Supreme Court assured the people of the Commonwealth that identification by hair examination was "scientifically reliable," it emerged that a defendant in another case, William Gregory, had been falsely convicted, largely on the strength of hair comparison expert testimony, served 7 years of a 70 year sentence for a crime he was innocent of, and released after DNA testing showed that he could not have been the person who committed the rape.

The hair identification error in William Gregory's case is not unusual. Review of a large set of DNA exoneration cases found this one form of expert testimony to be the sixth leading cause of erroneous convictions. (All forensic science errors or exaggerations added together make them collectively the second leading cause of erroneous convictions, second only to eyewitness errors.)

A recent study by the FBI compared cases in which results on the same hair were available from both microscopic hair examinations and mitochondrial DNA testing in 95 cases. If we treat the mtDNA results as the criterion of accuracy, microscopic hair examination experts saw associations in all of the cases where mtDNA found associations. But microscopic hair examiners mistakenly saw associations for 35% of the comparisons where mtDNA indicated no match.

All of which leads one to ask: why not simply put the proffered expertise to the legal test? Why should a court work so hard to avoid finding out exactly what *Daubert*? *Mitchell* want the court to find out: how good or bad the expert evidence is? If the science were sound, would any of these judicial gymnastics be necessary?

Old truths do not necessarily remain true or become more true. Sometimes they are found to have been in error and need to be withdrawn, or at least need to be revised, tempered, or amended. Thus, old admissibility decisions can become obsolete. As time passes, knowledge grows. There should be more, hopefully many more, studies today than there were 25 or 50 years ago. Courts ought to want to know what is known today, not what was know generations ago. Gradually, the scientists and practitioners of a field come to generally accept the new knowledge and abandon obsolete beliefs. (Think leeches.) What is scientific advancement about if not discarding less valid knowledge and replacing it with more valid knowledge?

Forensic science has suffered from a paucity of empirical research evaluating its claims and limits. As studies begin to be conducted, courts ought to be curious about what they show, and prepared to rule accordingly. Deferring to the data is exactly what *Daubert* and *Mitchell* are about.

I cannot conclude without acknowledging that a dissent by Justices Stumbo and Lambert makes the same essential point that I have made, though they did it much more succinctly. That dissent notes, among other things, that it is the "clear mandate in *Mitchell v. Commonwealth* that

'pursuant to KRE 702 and *Daubert*, expert scientific testimony must be proffered to a trial court. The trial court judge must conduct a preliminary hearing on the matter utilizing the standards set forth in *Daubert*.'" (Citations omitted.) And, further, that "the majority's holding improperly removes the burden of demonstrating admissibility from the proponent of the evidence, and instead requires the opponent of the evidence to prove its inadmissibility. Such has never been the law of this Commonwealth. *Daubert*, as did its predecessor *Frye*, establishes a hurdle of admissibility which must be overcome by the proponent of the evidence before it may be admitted at trial."

## Resources

Max Houck & Bruce Budowle, *Correlation of Microscopic and Mitochondrial DNA Hair Comparisons*, 47 *FORENSIC SCI* 964 (2002).

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

BARRY SCHECK ET AL., *ACTUAL INNOCENCE* (2000).

John Thornton and Joseph Peterson, *The General Assumptions and Rationale of Forensic Identification*, § 24, in *MODERN SCIENTIFIC EVIDENCE* (David L. Faigman et al., eds., 2002).

## Saks, Comment on the Data Supporting Claims of Expertise in Criminal Profiling

[Draft Letter to American Psychological Association "Monitor" (Submitted July 2004)]

"Criminal profiling: the reality behind the myth" (Jul/Aug 2004), poses the question: "Does profiling really work," and summarizes several studies. The data of those studies actually are far more negative about profiler performance than the studies' authors happen to mention in their discussions of the findings.

In Pinizzotto & Finkel's study, FBI profilers did have the highest number of accurate statements, but that is because they made the most total statements. They also had the highest number of inaccuracies. Accuracy as a proportion of total statements shows: Rape case: profilers 82%, detectives 81%, psychologists 82%, students 91%. Homicide case: profilers 76%, detectives 83%, psychologists 76%, students 84%.

One of the studies by Kocsis et al. (*Journal of Interpersonal Violence*, 2000), is a good example of massaging data until they come out "right." Proper analysis revealed no significant differences among any of the groups on any measure. So Kocsis et al. mixed and matched data until they obtained a difference favoring profilers. That became the conclusion of their study.

Profiling no doubt could benefit from some serious psychological research. But so far most of the help it has received is just hype - something the field has no shortage of.

For a fascinating review of the phenomenon, the history, the law, the dubious science, and the PR of profiling, see Risinger & Loop (2002). Three card monte, Monty Hall, modus operandi and "offender profiling": Some lessons of modern cognitive science for the law of evidence, *Cardozo Law Review*, 24, 193-285.

With DNA analysis, the problem is different. The scientific underpinnings of DNA analysis are well-tested and conceded to be solid even by critics. But the certainty of a DNA match can be overshadowed by the larger question of how the DNA evidence was obtained and handled. In the O.J. Simpson murder case, for instance, defense attorneys cast doubt on DNA results because of sloppy lab work, ultimately suggesting investigators planted the evidence at the scene. And a DNA match to a crime scene, many defense attorneys point out, only proves a suspect was there not that he or she committed a crime.

"The problems with DNA are partly human error, or worse, human corruption," said Harvey Silverglate, a Boston civil-rights attorney who fears innocent people may still be convicted under the Romney plan.

Human error is also emerging as a key problem in crime labs, both in Massachusetts and around the country. Stephan Cowans, who was convicted in 1998 of shooting a police officer in a Roxbury backyard, was freed from prison this year after it was revealed the fingerprint evidence used to convict him did not come from his finger.

A recent article in *Champion*, a magazine published by the National Association of Criminal Defense Lawyers, noted widespread problems at crime labs across the country, many exacerbated by overwork and small budgets. In Massachusetts, a state report two years ago noted that space in the State Police crime lab in Sudbury was so limited that scientists had to extract evidence from suspect and victim's clothing on alternate days to avoid cross-contamination.

Stung by that report, officials say the state has since gone through a voluntary accreditation by a national board that sets standards for crime labs. But that is only partially true: The Sudbury lab is accredited only in DNA testing and "criminalistics," the analysis of trace evidence, fibers and tool markings. The offices of ballistics and fingerprint analysis are not accredited; nor is the state's DNA database. State officials say they are attempting to get them accredited and are also seeking a large increase in funds for that lab.

Under the death penalty plan, Romney has pledged to ensure that all labs are operating as flawlessly as possible so there will be no questions about the way evidence is collected or analyzed. If valid questions do arise, prosecutors would not seek the death penalty.

Death penalty opponents agree that if labs were better monitored and funded fully, there would be less suspicion about whether the evidence was tainted or analyzed incorrectly. And the authors of the Massachusetts death penalty proposal are clear in wanting an independent scientific review of the collection, analysis and presentation of evidence, along with other safeguards. But as long as humans are involved in science, either analyzing it or, interpreting it, mistakes can happen, others say.

"What we say in forensic science is the more certain the scientist is, the less reliable the scientist is," said James Starrs, a professor of law and forensic sciences at George Washington University. "We all want to be on safe ground, always looking for a magic bullet. But our society can easily be taken in by science, and that is worrisome."

resistance of his superiors and used Quetelet's hypothesis to develop the first system of forensic identification, known as anthropometry, or *bertillonage*. Bertillon measured eleven different physical features of each prisoner and assembled the measures into special files. If Quetelet were right, Bertillon would be able to identify prisoners who had been arrested before and on rearrest were using aliases. At the same time, Bertillon had a source of data with which to begin testing Quetelet's hypothesis.

With the invention of *anthropometry*, forensic identification science was born, and Bertillon was its father. Bertillon's system proved useful in detecting recidivists. Though Bertillon rejected and resisted fingerprinting, eventually, fingerprinting replaced anthropometry as the archetypal tool of forensic identification. Fingerprinting was more convenient and useful, since people often left their prints, but never their anthropometric measurements, at crime scenes.

Bertillon nevertheless had established the concept that on some attributes people varied to such a degree that certain measures could be useful in identifying them. Once the courts accepted fingerprint evidence as being uniquely identifying, fingerprints became an icon for every other kind of identification evidence - toolmarks, bullets, bitemarks, handwriting, voiceprints, shoe prints, broken glass, and so on. That DNA typing often is referred to as "genetic fingerprinting" and voice spectrography as "voiceprints" illustrates the lineage of identification science. Examiners would explain that their object of study was just like fingerprints, and courts usually believed them. What for the social statisticians of the 19th Century had been a hypothesis to be tested became for most forensic scientists and courts of the 20th Century an article of faith, past questioning and beyond question.

The trouble for modern courts is that predecessor courts made no serious attempt to evaluate the claims of forensic identification scientists. The earliest American courts to consider fingerprinting relied on little more than general encyclopedias, a Mark Twain novel, and the mere assertion of the expert witnesses themselves that their claims were valid. From start to finish, there is nothing in those earlier opinions that could help a modern court answer the questions of scientific validity required by *Daubert v. Merrell Dow Pharmaceuticals*. A similar lack of scrutiny is to be found in the foundational judicial opinions admitting most other forensic identification sciences, including handwriting (where the best the courts could say for the claim of expertise was that it couldn't be any worse than the common law method of identifying the author of a writing), toolmarks (where the state supreme court that led the way flipped from total rejection to total acceptance with virtually no explanation of what changed its mind), and bitemarks (where a field that did not believe in its own ability to identify biters was turned around by courts that concluded that they could).

### **The Multiplication Rule and Beyond**

Asked for hard evidence, even today each of the subfields of forensic identification science rests its claims of infinite variation and unique identifiability on nothing more than Quetelet had offered, namely the multiplication rule of probability applied to populations. The essential idea of this concept is that if objects vary on a number of independent (ie., uncorrelated) dimensions, the probability of occurrence of any one combination of characteristics is found by multiplying together the probabilities associated with each dimension. Such calculations

typically produce probabilities that are vanishingly small. Having made this general point, the next step in the argument - and it is offered by the forebears of each forensic identification science subfield - is to appeal to the audience's intuition to make the leap into concluding that no two handwritings, toolmarks, fingerprints, gun barrels, or whatever, could be alike.

Heavy reliance on the multiplication rule as the foundation of individualization encounters numerous problems. First, of course, as Cummins and Midlo realized, probabilistic models cannot prove absolutes, such as that no two are alike. This is not physics, where two objects cannot occupy the same place at the same time. This is micro-taxonomy, where no law of nature prevents two or many objects from falling into the same category. Any given bridge hand has a probability of occurrence of less than 1 in 600 billion. Yet it would be obvious nonsense to insist that nature has arranged the universe so that once a bridge hand is dealt it will never be dealt again, or that it must wait for the 599,999,999,999 other hand to be dealt before the original hand could make a reappearance. Nevertheless, in forensic science there has been a leap from notions of probability to belief in a doctrine of unique individuality. Even if unique individuality did rule the universe, establishing the validity of a forensic technique would require testing the system of measurement and classification as well, even (or especially) if its principal tool is human perception and judgment.

Second, many of the rule's applications to forensic science may violate the independence assumption, so that the probabilities are not so small as the usual illustrations imagine them to be.

Third, with the exception of recent work involving biological markers, such as DNA typing, the various forensic identification sciences have not taken the trouble to collect data on populations of forensically relevant objects so that the probability of erroneous matches can be calculated. Instead, examiners implicitly assume the odds are one-to-infinity.

Fourth, the steps from observation of similarity to the conclusions that are offered to courts must traverse a minefield of potential errors of probabilistic inference that few forensic scientists, and even fewer lawyers or judges, are equipped to navigate.

The preceding problems, ironically, are precisely the shortcomings that led the California Supreme Court in *People v. Collins* to reject another sort of identification evidence. Presumably a California court would be constrained to reject forensic identification evidence (other than DNA evidence) for exactly the same failings. With real data and correct calculations, they would survive such scrutiny.

Additional reasons exist for preferring forensic identification techniques to be constructed on a foundation of real data and formal probability models. One is that it is the main road from subjective impressions to science. Another is that for several identification techniques, the assumption of no-two-alike has already been empirically disconfirmed. As a result, the need to make more accurate estimations of the reduction in uncertainty afforded by these techniques has become patent.

For every identification technique other than fingerprinting (which keeps a record of each

Two events finally gained respectability for handwriting expertise. One was the publication in 1910 of Albert S. Osborn's QUESTIONED DOCUMENTS, with an introduction by John Henry Wigmore. Together, Osborn and Wigmore conducted a quarter century public relations campaign on behalf of "scientific" handwriting identification expertise as practiced by Osborn and described in his book.

The "arrival" of document examination was finally secured by the Lindbergh Baby kidnapping case, *State v. Hauptmann*, in 1935. Osborn was the chief witness called to testify that Bruno Richard Hauptmann had written all of the ransom notes found or sent after the abduction of the son of Charles A. Lindbergh. The public seemed to need to believe Hauptmann was guilty, wanted him convicted, and was grateful to those who supplied the evidence. Osborn became a celebrity. For 60 years following the affirmance of *State v. Hauptmann*, no reported opinion rejected handwriting expertise, nor was much skepticism displayed towards it. Rather, it became universally accepted as scientific and dependable.

After standing unquestioned for most of this century, re-evaluation of handwriting identification expertise has been underway in recent years. The judicial treatment of handwriting under *Daubert* has moved from recognition of a lack of any scientific basis but a refusal to exclude the testimony (in *U.S. v. Starzecpyzel*) to a refusal to allow conclusions but permission for the expert to describe the handwriting similarities and differences (in *U.S. v. Hines*) to complete exclusion under certain circumstances (in *U.S. v. Fujii*).

In the trial of Timothy McVeigh in the Oklahoma Bombing case, the court prohibited document examiners from testifying to any identification of the author of a writing unless the government could convince the court at a *Daubert* hearing that their claims rested on a scientific foundation. The government declined to attempt to do so and no expert document examiners testified at the trial.

## **Fingerprints**

Case law upholding the admission of fingerprint evidence begins in Illinois in 1911 with *People v. Jennings*. Within the decade New Jersey, New York, Nevada, and Texas joined Illinois in approving the admissibility of fingerprint evidence. These initial jurisdictions established the rationale for admissibility. Little more than the passage of time was necessary for eventual universal acceptance.

These cases, germinal not only for fingerprint identification but for the many other forensic individualization techniques soon to spawn in its path, invested little effort assessing the merits of the proffered scientific evidence. Rather, for the most part, these courts casually cited treatises on criminal investigation, or general approval of science, or, eventually, other cases admitting such evidence.

For example, in *Jennings*, the court recognized the novelty of the expertise at issue, noting that "the courts of this country do not appear to have had occasion to pass on the question." In upholding the admissibility of fingerprint expertise, the *Jennings* court cited two general encyclopedias, three treatises on crime investigation methods, and one recent English

the fingers, hand palms, and foot soles of humanity by the All-Wise Creator for some good and useful purpose .....[namely,] the ultimate elimination of crime ... [by] unquestionable evidence of identity in all cases.

Before long, courts had ample precedents from sister jurisdictions to cite as authority for the infallibility of fingerprint evidence. Popular and judicial intuitions about fingerprints are so strong that not a case can be found that entertains any serious doubt about the scientific perfection that has been achieved by fingerprint examination.

application that constitutes the technique at issue - only one admitted expert testimony of voice identification. And vice-versa. Of courts that employed the *Frye* test narrowly -- narrowing the relevant scientific field to "those expected to know," that is, those which perform the specific application at issue -- not one excluded the testimony. These two versions of the *Frye* test, and their predictably opposite conclusions, illustrate one of the familiar criticisms of *Frye*, namely, that defining the relevant scientific fields broadly or narrowly largely dictates the conclusion that will be reached.

Of courts that claimed to employ a "relevancy" or "reliability" test - frequently equated, at least in the past, with the test embodied in the Federal Rules of Evidence - thirteen admitted voice identification expert testimony and three excluded it. What courts required for the expertise to be sufficiently "reliable" varied considerably. Most concluded that as long as there was something to be said on behalf of voice identification, that was enough to let it in. One court noted only that the witness was a credentialed expert and cited other jurisdictions that had admitted such testimony. Using a similarly minimal threshold, however, another court excluded the evidence, concluding that its almost presumptive reliability was outweighed by its risk of being given excessive weight by factfinders. Yet another court gave the scientific evidence on the proffered expertise a close and thoughtful examination, much like what the *Daubert* gloss on the Federal Rules would seem to require, and concluded that voice identification expert testimony was inadmissible.

There were courts that employed the test suggested by McCormick, that is, admissibility based essentially on logical relevancy under FRE 401, and exclusion if probative value was substantially outweighed by the concerns invoked by FRE 403. Every court employing this kind of test found voice identification expert testimony admissible.

Some opinions reached their conclusions without employing a discernible legal test.

The mere fact that some courts refused to admit voice identification expert evidence is itself significant in light of the traditional receptiveness of the courts to forensic individuation techniques.

Unique assistance in evaluating the available data came into being only a decade after voice identification made its first appearance in the courts. Help came in the form of a careful review of voice spectrography by the National Academy of Sciences. A panel of highly knowledgeable scientists from diverse relevant fields was formed; they carefully reviewed the relevant scientific literature; and they published their report. The Report could not be said to have given a good report card on the state of voiceprint analysis.

Upon publication of the report, the FBI ceased performing voice identification for the purpose of offering testimony in court, and it was expected that the courts would stop admitting voice identification expert testimony, at least until the scientific support for it improved. Of the eleven judicial opinions written since release of the NAS report, however, half admitted the expert testimony. Still more troubling, most of those opinions make no mention whatever of the NAS review, and only one of the two that did cite it appears actually to have read it. In short, for the most part, the courts decided the post-NAS cases just as they decided the pre-NAS cases, that

is, as if the report did not exist.

### **Bite Marks**

Bite mark identification is one of the newest areas of forensic identification. The courts have been virtually unanimous in admitting such testimony. Bite mark expert opinion has now been admitted in most jurisdictions of the United States, the great majority of those occurring since 1980. Ironically, fundamental research on the proficiency of bite mark experts did not appear until a quarter century later and that shows remarkably high error rates - in the neighborhood of 60 percent false positive errors.

Several ironies accompany this legal history. One is that forensic odontologists, perhaps reflecting a grounding in scientific skepticism that is absent from the traditional forensic identification sciences, were more doubtful about whether the state of their knowledge permitted them to successfully accomplish the challenging task of identifying a perpetrator "to the exclusion of all others." The history of other areas of forensic identification reveals few with similar self doubts. Second, in spite of the existence of these profound doubts, the courts began admitting expert testimony on bite marks even under the *Frye* test. Third, rather than the field convincing the courts of the sufficiency of its knowledge and skills, admission by the courts apparently convinced the forensic odontology community that, despite their doubts, they really were able to perform bite mark identifications.

The cornerstone case on the admissibility of bite mark identification was decided in 1975, in California, in *People v. Marx*. At trial, three odontologists testified that in their opinion the defendant's dentition matched the bite wound. One of those experts took pains to note that in many other cases he had refused to offer a firm opinion or even to testify about an identification. This case, however, was an exception in that the dentition at issue was unusual and the bite mark was exceptionally well defined. The witness characterized these bite impressions as the clearest he had ever seen, either personally or in the literature. Despite the expert's caution, and unusual case facts emphasizing the rarity of both the dentition and the bite marks, once the courtroom door was pried open, and affirmed on appeal, *Marx* became the admission ticket for a far wider and more dubious array of dentition in many subsequent cases.

The following year, in 1976, Illinois had its first occasion to consider the admissibility of bite mark evidence. In *People v. Milone*, the Court of Appeals held it admissible as "a logical extension of the accepted principle that each person's dentition is unique." The court based this on its earlier recognition of the identification of dentition of accident victims from dental records. In *Milone* expert witnesses disagreed sharply on the question of the validity and utility of bite mark identifications. The testimony of three forensic dentists was offered by the prosecution and four by the defense. The defense experts testified and cited odontological literature showing, at the least, considerable disagreement among forensic odontologists as to whether offenders could be uniquely identified from bites left in the flesh of victims. Notwithstanding the controversy in the trial record and in the literature the court still managed to find that the general acceptance standard had been met. As virtually always happened, and contrary to the approach of *Daubert*, the *Milone* court held that questions about the truth of the proposition quoted above - of infinite variation and unique identifiability - went to the weight of the expert testimony, not to its admissibility.

California law professor who wrote "Laboratory of Justice: The Supreme Court's 200-year Struggle to Integrate Science And The Law." He said there are no required standards for fingerprint analysis, and labs often declare a match between two prints based on years of examining fingerprints rather than a clearly spelled-out methodology. "From a scientific standpoint," he said, "that is the voodoo part."

Faigman and other critics argue that science has a long and checkered history in the courtroom. Lawyers once used body characteristics, such the lengths of people's arms or shape of their heads, to prove a defendant's propensity to commit a crime. In 1927, a phrenologist was called into court to "read" a woman accused of murdering her husband; the phrenologist declared that the suspect's chin "tapered like the lower face of a cat," demonstrating "treachery."

As phrenology was being dismissed as quackery, the early 20th century saw the birth of forensic science as a specialized profession, with laboratories and experts who aimed to link suspects definitively to crime scenes. Eventually, handwriting, fingerprints, photographs and blood samples became regularly introduced into evidence, and the belief that "every criminal leaves a trace" became a cornerstone of police investigations.

By the late 1980s, DNA testing had been widely adopted, and today technology is still marching on: A new technique called "brain fingerprinting", a kind of lie detector based on brain signals, was admitted into court in Iowa in 2003 in order to help free a man in prison for murdering a retired police officer. (The man was freed by the Iowa Supreme Court, although the judges did not refer to the technology in their decision.)

If history is any lesson, however, today's certainty is tomorrow's question mark. For example, the rise of DNA testing has revealed enormous failings in the microscopic hair analysis that was considered reliable a generation ago. In 2002, DNA analysis helped free a Montana man who had spent 15 years in prison for rape based in large part on faulty expert analysis of pubic hair found at the crime scene. According to the Innocence Project, which works to free wrongfully convicted people, in 25 of the first 82 DNA exonerations around the country, scientists and prosecutors presented bad or tainted science to convict a defendant.

So concerned was the US Supreme Court about the growing role of science in the courtroom in the 1990s that the court instructed judges to act as gatekeepers for scientific evidence, scrutinizing experts and procedures to be sure scientific techniques were peer-reviewed or tested, with known and acceptable error rates.

That instruction led to the first major court decision questioning fingerprint evidence. Two years ago, a Philadelphia judge ruled an expert could not link fingerprints found at a crime scene to a defendant because the matching technique used by fingerprint experts had never been proven valid. There was no proof, the judge said, that fingerprint analysis had been scientifically tested or its error rates calculated. The judge later reversed his decision after the FBI testified about training, procedure and error rates, but the challenge opened up the floodgates for other defense attorneys protesting fingerprint analysis.

"It has never been demonstrated that fingerprint examiners use a proven methodology," said Lyn Haber, a California forensic researcher.

Department of Justice formal solicitation of research proposals "to determine the scientific validity of handwriting identification" was a binding admission by the government that "in its present state, handwriting analysis cannot pass muster under Daubert/Kumho Tire." Directly after this ruling, the court proceeds to make "a few general observations" before it gets down to the business of applying "the Daubert factors to assess the admissibility of Storer's testimony." *Id.* at 1209. It is at this point that the opinion rolls off the edge of the table. The court says, essentially, that Daubert and Kumho Tire were directed at "novel theories in civil cases," and not at "time-tested techniques used almost universally by law enforcement," and further, where "a novel theory is presented to a court, it makes sense to demand proof of statistically significant results and strict compliance with scientific methods. However, where a technique has been repeatedly applied and tested by law enforcement and the courts for over a century, the Court does not believe that the absence of scientific data, without more, should be the death knell for such testimony," citing Judge Pollak's second Llera-Plaza opinion. In making these observations, the court comes as close to formally embracing the "civil plaintiff proffer vs. prosecution proffer" double standard as one can imagine a court explicitly doing, and does it without noting the difference between the use of a technique to generate investigatory leads and its use as evidence in a criminal trial. After these "preliminary observations" the result of the case seems a forgone conclusion.

Perhaps even more startling is the way that the court then deals with the requirements of Kumho Tire, which in the first part of the opinion it rightly noted requires some appropriate standards of reliability applied to the actual task the expert is performing in the case before the court, and which requirement it repeats at length at 1210-1211. How does Judge Lasnik define that task (or more properly, those tasks, since we have already noted that even Ms. Storer treated her comparison in this case as involving many discreet tasks applied to different parts of different writings with different amounts and types of writing)? Well, at first he makes no attempt. He begins his analysis with a description of the government's claims about data on handwriting uniqueness generally, but he says that he need not decide the general question of uniqueness [The court concedes in footnote 5 that the data cannot establish uniqueness.], given the extensive nature of the "known documents" available to the document examiner, whose credentials are impeccable. (If the reader cannot follow this logic, he is no worse off than the writer). He then says that the Kam data establish (globally) that Questioned Document Examiners are generally more accurate than lay persons (ignoring the problems of the application of those data to handwriting identification practice on the ground), that QDEs have a method, and that they have "broad acceptance" among law enforcement. Hence it is sufficiently reliable and properly admitted.

The closest the judge comes to defining the "task(s) at hand" occurs in two footnotes. In footnote 7 he rejects the idea that he should analyze handprinting separately as a separate task, relying in part on the testimony of Professor Kam described below, and further in footnote 5 he says his opinion is limited to situations where there are extensive "questioned and known" documents for the document examiner to work with. [It is easy to see how the extent of the "known" documents in this case might be taken to enhance accuracy, but in reality the "questioned" documents were not "extensive" in the sense that the 3 page ransom note in the JonBenet Ramsey case was extensive, that is, displaying lots of writing in a single document clearly by the same person. Instead, it is as if the document examiner had been given 78, or 150

or 300 different tests to do (depending on the separable sub-parts, which were clearly present given her actual results). How the extensive nature of her multiple tasks enhanced generally her accuracy is a mystery. The only document that actually appears to have been extensive was a three page document which was hand-printed, raising again the propriety of lumping printing and cursive together in making reliability judgments under the approach mandated by Kumho Tire.] In essence, Prime defines the Kumho Tire task-specific issue as "identifying handwriting as to authorship when there is extensive known writing," which is almost inevitably true in every criminal case, given the power to compel the creation of exemplars. In the end, Prime becomes a standard-issue global opinion in Kumho Tire clothing.

Prime is also the first case in which the government called Dr. Moshe Kam to testify explicitly on the issue of handprinting. The limitations of Kam's first two studies are analyzed at length in the main text at §28-2.3.6. The reader will recall from those sections that Dr. Kam refuses to share the data from those studies in contravention of the usual norms of science. In spite of this, courts have neither ordered that he produce those data for reanalysis as part of criminal discovery, nor have they barred him from testifying as a result of the failure to produce the data for defense inspection. It seems unthinkable that a court would follow a similar course in regard to a proffered plaintiffs expert who relied on such research in any competently litigated civil case. Be that as it may, however, in Prime things reached a new low. As indicated above, the court in Saelee excluded document examiner testimony on handprinting, concluding that handprinting identification was a task which the document examiner literature indicated was more difficult than cursive identification, and which, whatever the status of the research record in regard to cursive, there were no data indicating that document examiners could assign authorship to handprinting reliably. In Prime the prosecution called Kam to fill that gap. He testified, apparently [The court references his testimony at 220 F. Supp. 1203, 1213 n. 7 but does not describe it. The presumed tenor is taken from the fuller summary of the testimony in U.S. v. Hidalgo], that he had gone back to the original raw materials of those studies and discovered a subset of exemplars in the original materials that were (in his judgment) not in cursive script but handprinted, that he broke out that subset and analyzed it, and that document examiner performance in regard to the handprinting subset was not significantly different from performance in regard to cursive. [As if to confirm the elementary point of both science and law - that truth and accuracy are enhanced by making data public and accessible to scrutiny - it emerged at the Daubert hearing in Hidalgo that Kam's new analysis made the wrong comparison. It compared examiners' performance on handprinting to their performance on writing that was both script and a mixture of script and handprinting. Once that fact becomes known, it should be obvious even to nonscientists that what was tested was not the question that was at issue. Such a comparison between apples versus oranges with some apples mixed in will reduce and tend to obscure any true performance gap that might actually exist between examiner performance on handprinting compared to cursive writing. Why the simplest, most obvious and most appropriate comparison was not made, we cannot know. The procedural point here is that it, was handled by the Prime court in a way that prevented anyone from learning of the problem and by the Hidalgo court in a way that made it possible for the error to become known.] The court allowed this testimony despite the fact that the data and raw materials still were not produced for inspection and the newly discovered handprinting subset was never mentioned in the original publication of the study. In the end, however, despite formally recognizing the particularizing requirements of Kumho Tire, the court explicitly treated handwriting identification including handprinting as a

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Identifying Weaknesses of the Forensic Individualization Science (Especially Handwriting)

Michael J. Saks Arizona State University

Forensic Science and Daubert Generally

Saks, *Forensic Identification Science: A Summary of the Theory, the Science, and the Legal History* (printed below)

Daley, *Foolproof Forensics? Even Science May Not Make Verdicts Infallible*, Boston Globe Tuesday, June 8, 2004. (printed below)

Saks, *Merlin and Solomon: Lessons from the Law's Formative Encounters with Forensic Identification Science*, 49 HASTINGS L. 1. 1069 (1998).

Chapter 1 (and various other chapters specifically on forensic sciences) in Faigman, Kaye, Saks & Sanders, MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (2005 edition due out soon; earlier editions in the library).

Look at the courts' treatment of non forensic sciences (mostly in civil litigation). The screening is much more rigorous and exclusion of expert testimony far more common. See the discussion of cases in Faigman et al., MODERN SCIENTIFIC EVIDENCE (2005) and/or in Saks, Faigman, Kaye & Sanders, ANNOTATED REFERENCE MANUAL ON SCIENTIFIC EVIDENCE, SECOND (2004).

Also, consider the laboratory procedure of failing to "blind" examiners to extraneous evidence when making their subjective judgments about the evidence. See, Risinger, Saks, Rosenthal, & Thompson 2002. *The Daubert/Kumho Implications of Observer Effects in Forensic Science: Hidden Problems of Expectation and Suggestion*, 90 CALIFORNIA L. REV. 1 (2002).

Handwriting

Risinger, *Handwriting Identification*, in Faigman, Kaye, Saks & Sanders, MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (2005 or 2002 & 2003 pocket part).

Risinger, excerpt from above analyzing U.S. v. Prime (printed below).

## Fingerprints

David Stoney (and the Editors), *Fingerprint Identification*, in Faigman, Kaye, Saks & Sanders, MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (2005 or 2002 and 2003 pocket part). See especially the review of judicial district court opinions involving challenges to admission under Daubert.

United States v. Mitchell, 365 F.3d 215 (3d Cir. 2004) (testability saves lack of scientific testing; peer review outside of publication; error rates; generally acceptable by commercial use; "most factors support (or at least do not disfavor) admitting the government's latent fingerprint identification evidence. There are good grounds for its admission.")

Epstein, *Fingerprints Meet Daubert: The Myth of Fingerprint "Science" Is Revealed*, 75 S. CAL. L. REV. 605 (2002).

United States v. Llera-Plaza, 2002 WL 27305 (E.D.Pa.) (Llera-Plaza I).

United States v. Crisp, 324 F.3d 261 (4th Cir. 2003) (compare dissent to majority opinion).

## Microscopic Hair Identification

Saks, (Comment on) Johnson v. Commonwealth: How Dependable Is Identification by Microscopic Hair Comparison?, 26 The Advocate [Journal of Criminal Justice Education and Research] 14 (Jan. 2004). (printed below).

## Microscopic Toolmark and Firearms Identification

Biasotti & Murdock (and the Editors), *Firearms and Toolmark Identification*, in Faigman, Kaye, Saks & Sanders, MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (2002; 2003 pocket part).

Writings by Adina Schwartz, John Jay College of Criminal Justice.

## Bullet Lead Comparison

National Academy of Science, Forensic Analysis: Weighing Bullet Lead Evidence (2004) (available from the National Academies Press, 500 Fifth Street, NW, Washington, DC 20001; 800-624-6242 or 202-334-3313 (in the Washington area); [www.nap.edu](http://www.nap.edu)).

U.S. v. Mikos (N.D. Illinois, 2003) (partially excluding and partially admitting bullet lead comparison expert testimony).

## Behavioral "Profiling"

Risinger & Loop (2002). *Three Card Monte, Monty Hall, Modus Operandi and "Offender Profiling": Some Lessons of Modern Cognitive Science for the Law of Evidence*, 24 Cardozo Law Review 193 (2002).

Saks, Comment on data purporting to demonstrate expertise in criminal profiling (printed below).