Truth Machine

The Contentious History of DNA Fingerprinting

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Contents

Preface

During the premier season of the immensely popular American television series *CSI: Crime Scene Investigation,* the program's hero Gil Grissom frequently made pithy remarks that conveyed a characteristic attitude toward forensic evidence:

> Grissom *(admonishing Warrick Brown, a coinvestigator)*: Forget about the assumptions. Forget about your promotion. These things will only confuse you. Concentrate on what cannot lie—the evidence. . . . There is no room for subjectivity in this department, Warrick. You know that. We handle each case objectively without presupposition regardless of race, color, creed, or bubble gum flavor. (Episode 1.01: "Pilot")
>
> Grissom: We're crime scene analysts. We're trained to ignore verbal accounts and rely instead on the evidence a scene sets before us. (Episode 1.02: "Cool Change")
>
> Grissom *(later in the same episode, to another CSI investigator)*: People leave us clues, Nick. They speak to us in thousands of different ways. It's our job to make sure that we've heard everything they've said. Anything less is reasonable doubt.
>
> Grissom *(in the same episode)*: I tend not to believe people. People lie. The evidence doesn't lie.

The ideal investigator personified by Grissom distrusts human testimony, and by heroically acting without prejudice, he allows physical evi-

dence to speak for itself.[1] People lie, but the evidence speaks the truth—that is, as long as one listens to it "without presuppositions." In *CSI* and its numerous spin-off shows, forensic analysts are depicted as highly skilled scientists who distrust mere testimony and circumstantial evidence in favor of crucial bits of physical trace evidence. Whenever such evidence is allowed to speak for itself, criminal identifications reach a conclusive end. The heroes and heroines in *CSI* not only collect and analyze physical evidence, like police detectives they also risk their lives by directly confronting criminals with the truth (Nolan, 2007; Mopas, 2007; Valverde, 2006).

Experienced forensic scientists are quick to point out that such fictional portrayals are notoriously inaccurate, and that the reality of forensic science is far less clear, certain, and glamorous than portrayed on television. For example, according to forensic scientist Thomas Mauriello, "40 percent of the forensic science shown on *CSI* does not exist" (Klein, 2004). Another forensic scientist, Max Houck, adds that DNA evidence is used in a minority of cases. He also points out that few forensic analysts have advanced degrees in a science; much forensic science involves older forms of judgmental comparison; and even when "scientific" evidence is available and uncontested, ordinary forms of circumstantial evidence may be crucial for the judgment of guilt or innocence (Houck, 2006: 87).

The popular impact of these television shows is such that a term "the *CSI* effect" has been coined to describe an alleged effect on jurors' expectations. Supposedly, jurors in criminal trials who had followed the shows expect prosecutors to adduce clear physical evidence; otherwise they will not convict. Although the media reports "the *CSI* effect" as a full-fledged sociolegal problem, further examination yields no empirical evidence of any such effect (Cole and Dioso, 2005; Cole and Dioso-Villa, 2007; Tyler, 2006; Podlas, 2006; Forthcoming). The evidence for the alleged effect in most media accounts is actually evidence of the effect of prosecutor, not juror, behavior. "The *CSI* effect" appears to be more of a media panic (a media panic about the pernicious effects of media) than anything else. At a deeper level, however, "the *CSI* effect" may be viewed as indicative of awe and anxiety about the perceived power of

1. Grissom exemplifies a modern conception of scientific objectivity that ascribes inherent properties to an object that can be ascertained only by means of a disciplined struggle against the prejudices of sense and mind (Daston & Galison, 1992; Megill, 1994).

scientific evidence, particularly DNA evidence. Indeed, some scholars have suggested that the "effect" may not be, as "*CSI* effect" proponents claim, the effect of the distorted view of reality portrayed on television, but rather the effect of reality itself: the effect of the increasing use of science and technology in criminal justice for building legal cases. This broader "tech effect" (Shelton et al., 2006) may be viewed as a reflection of the assumption that DNA evidence has attained a level of "mathematical certainty" that transcends the merely subjective status of eyewitness testimony, confessions, and older forms of forensic evidence that purportedly generated "moral certainty."[2] Naïve jurors are by no means the only persons who hold such an assumption.

Although professional analysts remind us that the current state of the art in criminal investigation is far removed from the hi-tech science portrayed in *CSI,* they often make exception for DNA analysis. It is common to read that DNA fingerprinting is the "gold standard" in forensics; that it is a "truth machine"; that it represents "God's signature"; and that it produces "unassailable" or "infallible" evidence (McCartney, 2006: xii). These characterizations are not only made by enthusiasts for popular television shows in which crime scene investigators snare criminals with "science"; they are also made by science journalists, knowledgeable lawyers, and legal scholars. For example, in an article in the *New Yorker,* Jeffrey Toobin disabuses readers of the impression from *CSI* that trace evidence such as hairs or toenail clippings can be linked to individual suspects with absolute certainty. Toobin (2007: 33) mentions a forensic conference presentation in which Houck and FBI analyst Bruce Budowle reported results from a study that compared visual hair comparisons with the results from mitochondrial DNA (mtDNA) tests (Houck and Budowle, 2002). The mtDNA analysis indicated that in nine out of eighty cases in which forensic analysts using microscopes reported matching hairs, the samples could not have come from the same person. When Michael Risinger and Michael Saks (2003) first used that

2. Laudan (2003: 297) points out that the contrast between "mathematical" and "moral" certainty traces back to John Locke and John Wilkins (also see Shapiro, 1986). According to this distinction, mathematical certainty is subject to rigorous proof, beyond all doubt, whereas moral certainty *can* be doubted by a skeptic, but is nevertheless strongly supported by evidence and practically reliable. It is associated with an older, nonmathematical, conception of probability. Shapin (1994) elaborates how moral certainty was no less intrinsic to the role of witnessing in experimental natural philosophy than it was to the rise of standards of proof in law.

study to impugn the accuracy of microscopic hair comparison, Houck attempted to neutralize the criticism by refusing to accord epistemic priority to DNA: "Microscopical and mitochondrial DNA analyses of human hairs yield very different but complementary results, and one method should not be seen as 'screening for' or 'confirming' the other" (Houck, 2004). This response provoked some incredulity precisely because it has become virtually unthinkable to exhibit any confusion as to whether hair comparison is more trustworthy than DNA analysis.[3] For example, Toobin (2007: 34) quotes a comment about Houck and Budowle's study by law professor Margaret Berger suggesting that hair evidence should be excluded from criminal trials when there is "no mtDNA to back it up." Though used to counteract an alleged popular belief in the infallibility of forensic comparisons, such statements treat DNA analysis as itself infallible.

It is notable that such language is reminiscent of what commentators ascribed to the original form of fingerprinting nearly a century ago. At that time, the fingerprint was proclaimed to be "a God-given seal" (*Finger Print & Identification Magazine,* 1925: 10), "God's fingerprint language" (Brayley, 1909: 7), and "voiceless evidence" (International Association of Identification, 1920: 26). It also was declared that "until further research is made on the subject we shall assume that the Creator placed them there as a means of positive identification" (Murphy & Murphy, 1922: 9).[4] Such statements compound mathematical certainty with theological certitude. However, as we discuss below, the transcendent evidential quality once assigned to fingerprinting is now more often attributed to "DNA," as the older form of evidence has begun to lose its luster.

In this book, we question how DNA became a "truth machine." We chronicle a series of technological and legal changes starting in the mid-1980s, and focus on controversies in the courts and the science press. We also question the basis for the truth ascribed to the machine, and sug-

3. More recently, Houck and Budowle have invoked subtler arguments. They contend that the failure of hair comparison in these cases represents poor discrimination, not errors. This is perhaps an example of the "sociology of error" (Cole, 2005b): an account that presumes correct access to reality and reserves social and psychological explanations for instances in which that reality is obscured, misrecognized, or resisted by others (Bloor, 1976).

4. The religious terminology seems reminiscent of theological rather than mathematical certainty, but the two can be merged in a Platonic view of mathematics as a transcendent language of the gods.

gest that the probative value of DNA evidence rests on practices, circumstantial knowledge, and administrative assurances that are not essentially different from those that support other, less glamorous, forms of evidence. Finally, we examine the social and legal implications of the widespread belief that DNA evidence produces ground truth in the contentious world of criminal justice.

The Study

The research that went into this book took place over a fifteen-year period and involved a series of collaborations. The study began in the United States in the late 1980s and early 1990s, at a time when forensic DNA testing was being actively debated in the courts and in the pages of *Science* and *Nature*. It continued through the mid-1990s, at a time when the U.K. Home Office and Forensic Science Service were constructing a national DNA database (NDNAD). During that phase of the study, the televised O. J. Simpson trial in 1994–95 yielded a mother lode of videotaped testimony, transcripts, interviews, news coverage, and scholarly writing. Though the *Simpson* trial itself was less significant for our purposes than many of the other cases we covered, the mass of material greatly informed our understanding of the issues.[5] Our research continued through the late 1990s, when DNA testing was established as a new "gold standard" in forensic science. Finally, starting in 1999, we followed the story of how DNA testing provided leverage for challenging *other* forms of evidence, including fingerprinting.[6]

Because our research covered an extensive period while running concurrently with many of the events described, it combined historical and ethnographic methods of analysis. As historians of recent events, we collected and examined a substantial amount of documentary material, including published articles in forensic science and law journals and popular sources, unpublished records such as written forensic witness

5. For a series of studies on the O. J. Simpson trial and related issues, see the special issue of *Social Studies of Science* guest edited by Lynch & Jasanoff (1998).

6. Simon Cole, who had studied the history of fingerprinting, acted as an expert witness for the defense in several challenges to the admissibility of fingerprint evidence, and we draw upon his experience as a scholar and witness (see especially chapter 9). For a dialogue on the dilemmas raised by Cole's participation as an expert witness, see Lynch & Cole (2005).

statements and administrative reports, court transcripts, and written judicial summaries, and (in the *Simpson* case) videotapes of testimony.[7]

The ethnographic aspect of the study was "multi-sited" (Marcus & Fischer, 1999) and "multi-timed," in the sense that it involved many sites of investigation—principally in North America and Britain—over an extended period of time. We relied upon interviews, observations, and variable degrees of participation at laboratories and courtrooms, and also performed a significant amount of documentary reconstruction. Given limits of time, technical competence, and access to forensic laboratories, we relied upon interviews with laboratory staff at different levels of administrative hierarchy, ranging from research administrators, midlevel officials, and staff scientists, to various grades of technician. We made repeat visits to selected facilities in the United States and United Kingdom, where, in most cases, we were permitted to tape-record and transcribe the interviews. For example, during several visits to the Forensic Science Service facility in Lambeth, South London, we were permitted to take photographs (some of these are included in chapter 7). We were given tours and demonstrations of equipment and laboratory results, as well as reprints and references to technical publications. We also interviewed attorneys and expert witnesses involved in the cases we studied, and we examined court transcripts, and (in the *Simpson* case) video recordings of live television coverage. Some of our analyses of trial transcripts and videotapes were informed by conversation analytic procedures, but we did not aim to make a technical contribution to that field.[8]

To a large extent, we tailored our methods to the specific themes presented in the chapters that follow. We borrowed liberally from our own

7. Some of these materials were collected with support from the National Science Foundation, for building an archive at Cornell University on the forensic issues surrounding the O. J. Simpson case. The O. J. Simpson Murder Trial and DNA Typing Archive, 1988–1996 (collection number 53-12-3037), is housed at the Division of Rare and Manuscript Collections, Carl A. Kroch Library, Cornell University.

8. There is a large literature in conversation analysis. The two-volume set of transcribed lectures by Harvey Sacks (1992), the founder of the field, remains the single best source of insight into the workings of ordinary conversation. Still useful is the anthology by Atkinson and Heritage (1984). For conversation analytic studies of court discourse, see Atkinson & Drew (1979); Pomerantz (1987); and Drew (1992). For studies of credibility in tribunals see Brannigan & Lynch (1987), and for studies on uses of textual materials to leverage testimony, see Lynch & Bogen (1996), and Gonzalez (2006).

and others' earlier studies of laboratory discourse, the visualization and mathematization of scientific results, and the organization of testimony.[9] Although focused on specific legal decisions, legal briefs, trial transcripts, and selected videotapes of broadcasts of the O. J. Simpson trial, our study attempted to encompass a much broader scope than usually is the case for detailed studies of text and talk. The study focused on practices in different national contexts—principally the United States and United Kingdom, and to a lesser extent other European nations, the European Union, and Australia—over a twenty-year period.

We discuss cases from different national legal systems, and mention some differences between legal and criminal justice systems in connection with particular cases and technical developments. Our focus is mainly on the role of expert evidence in adversary legal systems, mainly in the United States and United Kingdom, but many of the developments we discuss are international in scope. For example, we noticed during the study that forensic scientists, criminal justice administrators, and lawyers were attuned to key cases and technical developments in other nations. International news coverage of notable cases and networks of forensic scientists transcend national borders. In addition, deliberate efforts were underway to "harmonize" DNA profiling systems in Europe and throughout the world. Consequently, while we have not made a systematic effort to compare national DNA profiling systems and practices, we believe that the developments described in our study apply broadly, if not universally.

The research required us to read technical publications, and to seek explanations from practitioners. One of us is trained in biology; two of us were given an elementary hands-on lesson in laboratory routines; one took part in a training session on crime scene investigations involving DNA evidence; and one of us was an expert witness in court cases involving fingerprint evidence.[10] Discussions among ourselves and with colleagues and informants helped upgrade our understanding of legal

9. See Lynch & Woolgar (1990) for a collection of studies on visualization in science, Goodwin (1994) on visual rhetoric used in court, and Jasanoff (1998) for an interpretation of courtroom materials informed by such studies.

10. Patrick Daly, an inspector with the British Transport Police, was a part-time PhD student at Brunel University when Lynch and McNally were there, and he was a good source of information and insight about police investigations and police views of DNA evidence.

and technical issues. The issues—particularly the probability issues—are widely acknowledged to be difficult, and indeed they were difficult for us as well as for judges and jurors. However, our aim in this book is not to give technical advice on legal, statistical, biological, or forensic matters, nor is it to present a comprehensive historical narrative of the controversy over forensic DNA profiling (for such a history, see Aronson, 2007); instead, our aim is to narrate a sociological history in which this controversy illuminates the fraught relationship between science and law. Each of the associated practices can be explored in much greater technical depth, but we shall not do so here.

Organization of the Book

The book is organized into ten chapters, which are interspersed with five "interludes" that present relatively compact accounts of some of the legal and technical issues featured at various points in the discussion. The overall structure is that of a narrative that runs through phases, starting in the mid-1980s when DNA "fingerprinting" was invented and continuing until the present time. At a more abstract level, the narrative is framed by phases of legal-scientific controversy about forensic DNA evidence. The controversy began in the late 1980s and ran for a few years before winding down in the mid-1990s and entering a "postclosure" phase in which "DNA" was invoked as a gold standard with which to challenge all other forms of criminal evidence, including fingerprinting.

Themes discussed in each chapter summarize issues that were brought into focus by particular cases and phases of controversy, as well as questions of general interest for science and technology studies (S&TS) and legal research:

- How do controversies open and close in a hybrid legal-scientific field?
- How do innovations disperse in time and place, and how is technical stability maintained?
- How does expert evidence relate to ordinary "commonsense" evidence?
- How are the credibility and scientific status of evidence presented and undermined in adversary discourse?
- How do quantitative measures influence the credibility of evidence?
- How is the probative value of evidence constructed and deconstructed?

These questions relate to topics that are often considered epistemological—objectivity, representation, quantification, and scientific truth—but they are addressed in a case-specific way that draws our attention to historical contingencies and circumstantial judgments.[11]

The concluding chapter critically reflects on the story of how DNA evidence was granted such extraordinary truth status. We recognize the delicacy of such criticism: the currently popular view of "DNA" as a "truth machine"—an unassailable basis for exposing the fallibility of all other forms of criminal evidence—has been crucial for postconviction exonerations of death row inmates. While we applaud such exonerations, and recognize the immense effort that has gone into their achievement, we also believe that uncritical assumptions about the objectivity and certainty of DNA evidence also are encouraged by those who would lend scientific certainty and legal finality to death penalty convictions. The chapters that follow underline a lesson that is, or should be, familiar to students of criminal justice if not to *CSI* enthusiasts: expert evidence does not *itself* determine guilt or innocence; instead, its probative value depends upon circumstantial judgments that place the evidence within a story of the case at hand. The exceptional credibility assigned to DNA evidence does not exempt it from the judgments and contingencies that surround its use in criminal investigations. This is not to say that DNA evidence is somehow less certain or more error-prone than "real" science. Rather, the lesson from a large body of research on the history and social organization of the natural sciences is that the credibility of research results in *every* empirical science rests upon judgments and contingencies that can never be fully explicated. In addition to exploring how that lesson applies to a self-proclaimed science that is implanted within a legal domain, we believe that our study of protracted disputes and deliberations about DNA evidence sheds light on how scientific credibility is achieved as a practical, rhetorical, and administrative matter.

11. Peter Dear (2001) uses the term "epistemography" to describe a detailed historiographic or ethnographic orientation to the traditional topics of epistemology. This idea has some kinship with a long-standing ethnomethodological program for "respecifying" the recurrent themes in social theory and methodology (Garfinkel, 1991). Lynch speaks of these themes as "epistopics" (recurrent topics of epistemological discussion and debate) that tend to appear again and again in philosophy, history, and sociology of science (Lynch, 1993). Also see Garfinkel (2002: 181–82) for a discussion of "perspicuous settings."

CHAPTER ONE

A Revolution in Forensic Science?

This book is about a "scientific revolution" in forensic science. As with *the* scientific revolution of the seventeenth century, there may be some reason to doubt that it actually was a revolution or even that it ever occurred, but nevertheless it remains a compelling subject to write about (Shapin, 1996: 1). The revolution we chronicle has been widely heralded, and its banner is "DNA." In the context of forensic science, "DNA" is shorthand for a family of techniques and their analytical products. Common names for these techniques include DNA fingerprinting, DNA profiling, DNA typing, and DNA testing.[1] Technical names include multilocus probe (MLP), single-locus probe (SLP), and short tandem repeat (STR) systems. DNA fingerprinting, like its traditional namesake and other forensic identification techniques,[2] is used for comparing trace evidence found at crime scenes with a suspect's (or sometimes a victim's) evidence. But unlike comparisons of fingerprints, handwriting samples, threads, hairs, or bite marks, DNA profile comparisons use laboratory

1. The most widespread name, especially early in the history of the techniques, was "DNA fingerprinting." For reasons we shall explain, the analogy with fingerprinting later was downplayed, and other terms such as "DNA profiling," "DNA typing," and "DNA testing" were preferred. Sometimes, DNA profiling and DNA typing are distinguished, with the former referring to earlier techniques making use of restriction enzymes, Southern blotting, and autoradiography, and the latter referring to more recently developed techniques using the polymerase chain reaction as a key constituent. Several techniques have been used in the past twenty years, and there have been numerous variants on each technique. We briefly review some of the main techniques in interlude A.

2. Throughout this book, we use "forensic science" to describe practices performed by various public and private organizations, such as the Forensic Science Service in the United Kingdom, to assist police and, less often, defendants in criminal investigations. Forensic science is involved in other kinds of investigation, and more precise terms (such as "identification sciences") can be given to fingerprinting and DNA profiling.

methods and equipment that have widespread use in biological research and clinical testing. In public discourse about DNA evidence, laboratory science is associated with truth. In recent years "DNA" has become emblematic of a level of objectivity and certainty unmatched by any other mode of criminal evidence. In this book, we chronicle how forensic DNA testing attained such extraordinary status, and work out some of its implications for criminal justice. Of primary concern for us is how the attribution of scientific status to DNA evidence seems to have lifted it above and beyond the contestable status of other forms of expert and nonexpert evidence. By paying close attention to arguments by scientist-critics and lines of attack used by savvy defense lawyers during key controversies and legal disputes, we show that the credibility of DNA evidence largely rests on a fallible combination of technical, administrative, and legal practices. We argue that, in many respects, its credibility, as well as its occasional vulnerability to attack, arises from institutional practices that support or undermine the credibility of less glamorous forms of criminal evidence.

Although we focus on legal matters, our orientation largely draws from the field of science and technology studies. Sometimes called "science studies" or "science, technology and society," S&TS is a transdisciplinary field that combines the history, social study, and philosophy of science. In recent decades, research in the field has become notorious for its critical, often skeptical, approach to the "construction" of scientific facts, laws, and entities. Constructionist S&TS is frequently caricatured as a form of philosophical relativism that holds that everything under the sun (and, for that matter, the sun itself) is a figment of cultural imagination, not essentially different from fairies and goblins. Without going into the frequently rehashed arguments on the subject,[3] we can say, simply, that our orientation to the "deconstruction" of scientific evidence follows paths that have been blazed by scientists and lawyers who have had prominent roles in disputes about DNA evidence. In this respect, we pursue an ethnomethodological line of research (Garfinkel, 1967; 2002), meaning literally a line of research that investigates the varieties of "people's practices" (ethno-methods) that establish facts on the ground. Rather than seeking to establish that DNA evidence is, in the

3. Opinionated attacks on constructionism are found in Gross & Levitt (1994), Gross et al. (1996), and Sokal and Bricmont (1998). See Hacking (1999) for a broad and well-informed philosophical critique of constructionism, and Lynch (1993) for a more focused, critical examination of social constructionism in S&TS.

final analysis, a social construction, we examine the interactional and administrative practices through which lawyers, experts, and nonexperts build up and break down its credibility. In this introductory chapter we set up our chronicle of the recent history of forensic science by critically discussing a prominent conception of a "paradigm" shift in that field. This allows us to introduce a distinct view of the construction and deconstruction of expert knowledge in criminal justice systems.

A Paradigm Shift?

DNA fingerprinting has been said to mark a "paradigm shift" in forensic science. Though misleading in many respects, the analogy with a new scientific paradigm is interesting for those of us in the field of science and technology studies who grew up with Thomas Kuhn's (1970 [1962]) *Structure of Scientific Revolutions* as our canonical text. Kuhn's historicophilosophical account of scientific revolutions itself had historic, revolutionary significance for the S&TS field. Especially significant was his conception of paradigm shifts in the basic or "pure" natural sciences: grand conceptual sea changes in astronomy, physics, and chemistry. According to Kuhn, a new paradigm is no mere novelty: discoveries that cumulatively build upon prior discoveries are emblematic of "normal science," whereas a new paradigm requires a much more radical rupture with the past—a level of novelty that is at first resisted, because it defies conventional wisdom. A new paradigm overthrows the old: heliocentrism overthrows geocentrism; chemical theory overthrows phlogiston and caloric; relativity replaces classical mechanics. Importantly, and controversially, Kuhn (1970 [1962]) argued that competing paradigms were "incommensurable." Referring to the much-abused alternating figure of the duck-rabbit in gestalt psychology (also see Hanson [1961] for a similar argument using an antelope-bird alternating figure), Kuhn suggests that a scientist's alignment with one paradigm rather than another is not a rational choice; instead, it is more like the way a viewer sees the alternating figure *as* a duck or *as* a rabbit. But, unlike a viewer who can freely alternate between seeing the figure as a duck and then as a rabbit, scientists tend to have their feet firmly planted in a single paradigm. The nexus of theoretical commitments and the communal networks that make up and support the paradigm commit subscribers to a standpoint from which they simply cannot see the sense of the alternative universe.

The transition between paradigms is not a sudden "switch," but more of a gradual migration or generational change within a fractiously divided community of practitioners. Once established, a new paradigm generates its own "normal science" tradition, in which novelty is framed by no-longer-questioned presumptions that were controversial during the prior period of revolutionary change.

To speak of DNA profiling in forensic science as a new paradigm is hyperbolic. For all its advantages as a method of criminal identification, it does not represent a new paradigm in Kuhn's sense of a grand conceptual shift that overthrows existing theory and normal science. It does not surpass a prior normal science tradition with something completely different. Instead, as Saks and Koehler (2005: 893) would have it, the revolution is a matter of rising *to,* rather than rising *above,* normal scientific status. Speaking of pre-existing craft traditions of forensic science, they observe,

> In normal science, academically gifted students receive four or more years of doctoral training where much of the socialization into the culture of science takes place. This culture emphasizes methodological rigor, openness, and cautious interpretation of data. In forensic science, 96% of positions are held by persons with bachelor's degrees (or less), 3% master's degrees, and 1% Ph.D.s. . . . When individuals who are not steeped in the culture of science work in an adversarial, crime-fighting culture, there is a substantial risk that a different set of norms will prevail. (Saks & Koehler, 2005: 893)

By treating normal scientific status as a positive historical goal, Saks and Koehler imply that, until now, forensic science has been a *subnormal* science.[4] It has subnormal status, not so much in comparison with Kuhn's normal science, but with an idealized conception of "real" science according to which some forensic disciplines appear to be a loose array of police crafts graced with an aura of science.[5] According to a

4. Kuhnian normal science is an established, stable, and cumulative research tradition that develops in the aftermath of a scientific revolution. Saks and Koehler's normal science is a general status attained by a specialized practice. Their view is akin to general conceptions of science expressed by the U.S. Supreme Court in rulings about the admissibility of expert evidence. Normality in this sense is an aspect of public credibility (Shapin, 1995). It is also subject to legal stipulations and public administration.

5. Kuhn sometimes spoke of a preparadigm phase in the history of a science, and he noted that some disciplines (such as many in the social sciences) remain stuck in a preparadigmatic phase. These differ from the forensic sciences in that the latter are often

conventional (and rather whiggish) history,[6] for more than a century practitioners of various criminological crafts for identifying individuals (commonly called forensic sciences today) have called themselves "scientists," and novel techniques such as fingerprinting and tool mark and bite mark analysis were heralded as scientific breakthroughs. But, as Saks and Koehler argue, these traditional practices have not yet attained (and may never attain) the normal scientific standing enjoyed by DNA typing. Accordingly, DNA typing rose to the status of normal science because it applies knowledge "derived from core scientific disciplines," and offers "data-based, probabilistic assessments of the meaning of evidentiary 'matches'" (Saks & Koehler, 2005: 893).[7]

In support of Saks & Koehler's view, it can be said that, notwithstanding dramatic portrayals in currently popular television shows, forensic science has long held secondary, and even dubious, standing when compared with "high" science. More than forty years ago, Paul Kirk, a prominent forensic analyst, wrote (referring to "criminalistics," the specialized forensic practices used in criminal investigations), "It seems fair to state that criminalistics may now be considered a science in its own right, but that it lacks at this time the full development that will allow general recognition" (Kirk, 1963: 238). According to a much more recent account, this characterization still holds:

"mature," stable practices, and some of them (such as latent fingerprint examination) are characterized by a high degree of consensus (see Cole, 1998). From the point of view of university-based sciences, they are subnormal because of their lack of connection to scientific disciplines, and scientific culture more generally, and their strong association with police work and police organizations.

6. The "whig interpretation of history" (Butterfield, 1931) is a critical label affixed to historical accounts that treat the past as a condition for the emergence of present-day democratic institutions and religious tolerance. Kuhn and many others criticize histories of science that treat present-day textbook knowledge as a basis for understanding and evaluating earlier eras of science and natural philosophy. In this sense, Saks and Koehler present a whiggish (or, in Wilson & Ashplant's [1988a, b] terms, a "present-centered"]) interpretation of forensic science that forgets the extent to which claims to scientific truth and associations with established academic sciences have characterized many earlier eras in the history of forensic and identification science (Golan, 1999, 2004a).

7. This differs from saying that the older forensic sciences are preparadigmatic in Kuhn's sense of having yet to attain a consensual paradigm that unifies the discipline and overrides intractable conceptual difficulties and interminable disputes. Some forensic science professions (exemplified by latent fingerprint examiners [Cole, 1998]) have long enjoyed impressive internal consensus and credibility in the courts. Saks and Koehler use an idealized image of DNA profiling—its use of probability measures and its adoption of molecular biological techniques—as a basis for making invidious comparisons with older forensic sciences.

> The situation, unfortunately, hasn't changed much since Kirk described it 40 years ago. Technical innovations, especially in instrumentation, have far outdistanced any attempts to establish a theoretical framework for criminalistics as an autonomous discipline. The dearth of, and recent alarming decrease in, academic programs in forensic science only serve to underscore the dangers of analysis without a framework for thoughtful interpretation. (Inman & Rudin, 2001: 64)

Unlike the discovering sciences (still epitomized by physics, though with molecular biology in the ascendancy), forensic science involves routine procedures, limited objectives, and little or no orientation to discovery. It is not rocket science. It is related to what Steven Shapin (2004) has called "sciences of the particular": practical research geared to tangible objectives set by other social institutions.[8] More conventionally understood, it is an "applied" science:

8. Routine clinical trials of pharmaceuticals can be viewed as a "science of the particular" that is subjected to regulatory "scientific" norms articulated, administered, and arguably enforced, by government and corporate organizations. Kuhn was sometimes criticized for portraying normal science as unexciting and routine work, but the routines of the clinical trial or forensic laboratory are far less creative than anything Kuhn deigned to include in his conception of normal science. Increasingly, clinical trials are performed by contract research organizations (Angell & Relman, 2002; Mirowski & Van Horn, 2005) and sponsored by pharmaceutical companies. They are regulated (perhaps not heavily enough) by government agencies, and their production is highly normalized. Even the scandalous practice of ghostwriting in pharmaceutical research—so vividly exposed by David Healy (2004)—points to a high degree of normalization through which the authorship function becomes reduced to an endorsement separate from the arts of writing (a perverse twist on the trends discussed by Biagioli & Galison, 2003). They are not internally governed normal sciences in Kuhn's sense, but are instead monitored and regulated by government agencies. Steven Epstein (1996) uses the term "impure science" to describe clinical trials, but he means more than that they represent a corruption of the ideals of "pure science." Epstein describes how AIDS activist groups effectively lobbied regulatory agencies and pharmaceutical firms to accelerate clinical trial protocols in the interest of getting experimental drugs to patients. The fact that standard methodological steps and controls were bypassed was not, for Epstein, indicative of shoddy science; rather, it pointed to how the methodology of clinical trials answered to multiple social interests and agendas. Epstein's conception of "impure science" is relevant to the way DNA evidence is handled, presented, and evaluated by a range of agents and agencies besides scientists. However, the "subnormal" status of (some) forensic science has to do with an invidious comparison with a "paradigm" that is supposedly rooted in accepted scientific concepts and procedures. Another partial cognate is Silvio Funtowicz and Jerome Ravetz's (1992) term "post-normal science" (see also Ravetz, 2006). This term describes sciences, such as those associated with assessing possible causes of global climate change, that address urgent problems under contentious and uncertain conditions. Although forensic sciences can involve uncer-

> Little of what goes on in forensic science resembles the classical description of how science develops theories, tests hypotheses and revises its ideas and understandings. This is partly because the scientific method is a description of pure, or basic, science (knowledge building), while forensic science is an *applied* science. (Thornton & Peterson, 2002: 13; emphasis in original)

However, even its status as an applied, as opposed to basic, science can be questioned. After all, which branches of basic research do the fingerprint examiner, tool mark examiner, and handwriting analyst apply? Although latent print examiners' visual comparisons of fingerprints may be informed by self-tutelage in embryology, it is difficult to see how "science" governs the comparison and evaluation process itself. At their best, these "sciences" are diligent practices of collecting, reading, and comparing trace evidence. In some disciplines forensic scientists have historically risen from the police ranks, and their higher education credentials are limited. Their skills are learned on the job, rather than acquired through advanced education.

Although its nominal status as science may give forensics some authority in courts of law, its association with the natural sciences also is a burden. More conspicuously than university research, or even some corporate research, forensic science lacks autonomy: it is answerable to initiatives and evaluations that are not controlled by the community of practitioners.[9] Administrative quality assurance/quality control (QA/QC) regimes are designed by associations of lawyers and scientists who advise government agencies on how to normalize a science that is not trusted to stand on its own feet. Layers of internal staff and various external agencies and institutions are empowered to review and evaluate the research and its products. Such QA/QC recommendations incorporate scientific ideals (double-blind testing, peer review, and the characteristic format of research reports) that are enacted and supervised

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tainty in high-stakes situations of decision making, notwithstanding *CSI* drama they tend to be routinized and performed by anonymous agents.

9. Some forensic laboratory directors, for example, report to nonscientists in the law enforcement chain of command, a practice that can raise questions about how scientific autonomy is meant to function in such contexts. Forensic science is not heavily regulated when compared with industries such as clinical laboratory testing, or even pet food manufacture. With the increased salience of intellectual property concerns, human subjects committees, and protections against charges of intellectual misconduct, university scientists also are becoming subject to legal and regulatory norms, formal regulations, and reviews (Myers, 1995; Packer & Webster, 1996).

within bureaucratic administrations.[10] The regulations and standards often mechanically invoke popularized Popperian (pop-Popperian, we might say) notions of testing, testability, and falsifiability.[11] By implication, when forensic science adheres to such standards (peer review, reliability, testability, etc.) it becomes "normal," though as we shall see, the legitimating function of such standards in courts of law is independent of the extent to which they are actually followed, in practice.

Earlier "Paradigms"

The current tendency to treat DNA typing as an exceptional and strongly scientific form of evidence obscures the fact that to a large extent its institutionalization has followed a path set by earlier techniques which are now deemed to be subjective and prescientific. When Alphonse Bertillon's "signaletic" system was adopted by the Paris Prefecture of Police, it was used to profile criminals who were in police custody. A filing system was set up in which standardized information was presented on cards. It was possible to search the cards to find individuals with identical measurements. This property was held to be a crucial feature of campaigns to apprehend "recidivists"—repeat offenders who often literally and figuratively masked their identities. Bertillon argued that his measures were more trustworthy than photographs, and in France and many other nations the system he established lasted well into the twentieth century, decades after fingerprinting had become the preferred identifi-

10. Robert K. Merton's (1973) famous schema for the norms of science (universalism, disinterestedness, communal property rights, and organized skepticism) supported the argument that scientific objectivity can be optimized only under conditions of autonomy from political, religious, and economic interests and authorities. The schema is often criticized for offering rhetorical support for the view that science is cognitively exceptional, free from politics, and uncorrupted by commercial interests. What such criticisms tend to miss is that Merton's scheme borrows heavily from Weber's (1968: 956ff.) ideal type of bureaucracy, with its emphasis on conduct in accordance with rules (rationality), impersonal criteria of evaluation, specialization of function and circumscribed expertise. These ideal features support a conception of what might be called "administrative objectivity"—the claim that administrative decisions follow from legitimate rules rather than personal biases.

11. This is, of course, a reference to Sir Karl Popper's philosophy of science (see, for example, Popper [1963]), though as Susan Haack (2005) argues, the popularized versions of falsification that legal actors (including U.S. Supreme Court justices) invoke tend to be watered down and conflated with incompatible philosophical ideals.

cation method in Britain, India, and Argentina. Although Bertillonage is now archaic as a method of identification, it often is credited with initiating a scientific era of criminal forensics. Ginzburg (1989) links it to an emergent "evidential paradigm," which ranged across numerous fields of investigation and was characterized by an effort to reconstruct individual identities from mundane signs and traces, rather than merely inferring character from them. Nonetheless, efforts to discern character from somatic markers remained—and still remain today through popular and professional interest in a genetic basis for crime (Allen, 2004)—closely related to identification.

The crucial advantage of fingerprinting was not so much the individuality of fingerprints or the accuracy of the system, but rather its ability to lower and defer costs by enabling identification data to be recorded by relatively unskilled labor, while requiring skilled labor (the indexing of the prints) at a centralized "back end." Moreover, fingerprint examination not only could be used to counteract a recidivist's aliases and disguises, it also could be used to connect an individual to a crime scene. Newly professionalized "fingerprint experts" promoted the idea that matches between latent marks (recoverable traces left at a crime scene) and rolled prints (inked prints of each finger taken under controlled conditions) could be determined with absolute certainty.

Until very recently, dactyloscopy was not a digital technology, except in the punning sense of being about fingers.[12] However, the older meaning of "digit" also signaled an advantage, as many crimes involve acts of touching. Much in the way that the human face is a meaningful surface of identity in social interaction (as noted by sociologist Erving Goffman (1959), in his classic essay on "face work"), the ends of the fingers are an interface with the material surfaces with which a person interacts. As Ginzburg (1989) observes, fingerprints are usually inscribed inadvertently, as byproducts of action rather than expressions of intention, and this enhances their forensic credibility. In Goffman's (1959: 2ff.) terms, they are signs "given off" rather than "given," which are legible for an indefinite time after they have been inscribed. Prior to their use for purposes of criminal identification, fingerprints were sometimes used

12. Although fingerprints are now recorded and filed in digital form, latent fingerprint analysis continues to require a human examiner to assess possible matches turned up by a computerized database search.

as equivalent to signatures: direct bodily impressions onto a paper surface. What is so useful about fingerprints for purposes of criminal identification is that, for the most part, "finger writing" is inscribed invisibly and unintentionally, whenever the tips of the fingers contact a surface leaving oily traces of ridge and pore patterns. The technique of exposing and collecting the latent fingerprint sets up their codification and comparison. Footprints are an alternative form of bodily trace, and forensics makes ample use of them, but the practice of wearing shoes tends to blunt the discriminatory power of the footprint. In modern urban life a footprint is a trace of a removable manufactured object, which may show unique patterns of wear, but it is a different category of trace than a fingerprint or handprint.[13]

The technology of latent fingerprint analysis is aided immeasurably by features of the built environment in the modern world. Our fingers continually come into contact with flat surfaces composed of, or covered with, glossy paint, plastic, glass, polished metal, and other substrates that collect and maintain the integrity of the oily imprint. Moreover, our environments, and our ways of life within them, have standardized features that facilitate inferences about the places and circumstances of normal and suspicious entry and exit.

As Paul Rabinow (1996b) observes, Francis Galton, who coined the word and founded the program of "eugenics," initially hoped that fingerprint patterns would provide a meaningful index of hereditary lineages. Galton failed to find significant ethnic correlations for fingerprint patterns, although other researchers were able to establish such links. Despite his own disappointment, Galton, together with Edward R. Henry and Juan Vucetich, made a significant contribution to fingerprinting by developing a rudimentary classification system for fingerprint patterns, which proved useful not for tracing heredity but for individual identification. The widespread adoption of fingerprinting as a method of criminal investigation was based on the dogma that no two individuals have the same dermal ridge patterns, together with the assumption that professional examiners were capable of making error-free fingerprint identifications. Fingerprint evidence was contested in trials early in the twentieth century, and fingerprint experts sometimes had to conduct in-court

13. Shoe prints can be highly significant, as viewers of the O. J. Simpson trial may recall. The prosecution presented evidence of bloody footprints recovered from the crime scene that matched a rare kind of shoe. Simpson denied owning such shoes, but during the trial he was shown wearing them in a televised broadcast.

demonstrations to persuade juries of their ability to distinguish matching from nonmatching fingerprints (Cole, 1997; 1998), but by the 1930s the practice had become nearly unassailable in the courtroom.

Fingerprint identification bureaus were developed during the 1890s in India and Argentina, and in Britain in 1901. Scotland Yard made the first latent identification in Europe in 1902 in the Denmark Hill burglary case. It made its first fingerprint conviction in a murder case in 1905, fingerprinted its first corpse in 1906, and established its first legal precedent ("stated case") in the *Castleton* case in 1909, in which the only evidence offered by the prosecution was that fingerprints on a candle matched those of the accused (Lambourne, 1984: 67–86). Fingerprint evidence and the experts who vouched for such evidence were uncertain and contentious matters in these early trials (Joseph and Wilson, 1996), but by the 1930s it was widely accepted that when a fingerprint expert declared a match between two prints, such testimony provided unambiguous evidence of identity. Courts were willing to accept that no two sets of fingerprints are exactly alike, and they reasoned from this assumption that latent print identification must be "reliable." Subsequently, juries were willing to convict on the basis of fingerprint evidence alone. Fingerprinting became a routine part of police procedure, and administrative systems were devised for collecting, filing, and analyzing fingerprints. In 1948, the first mass fingerprint screen in Britain was undertaken, and in the same year police powers to take fingerprints were extended under the Criminal Justice Act (section 40), which permitted the police to take fingerprints under court order from a person charged before a magistrate with a criminal offence without his or her consent (Lambourne, 1984: 123–29). Police were granted the power take fingerprints from suspects without a court order under the Police and Criminal Evidence Act 1984.

The Credibility of Latent Fingerprint Examination

Even if one assumes that no two fingerprints are alike, latent print identification is really more about likeness than about unlikeness. The question is not whether individuals have unique friction ridge skin on their fingertips; the question is whether latent print examiners can accurately determine whether two unlike prints (for example, a "latent print" or "mark" found at the scene of a crime, and an inked print taken from a suspect) derive from a common source finger. The two prints will not

be identical: the latent print is likely to be partial, less distinct, and distorted by variations in substrate and pressure. Similarly, even if one assumes that prints from different persons will never be alike, this does not guarantee that the differences will be correctly detected in every case. Consider, for example, handwriting: one may assume that no two people have exactly the same handwriting, but this does not necessarily lead us to trust the handwriting expert. It took a few decades for fingerprinting to be accepted in the justice systems in Europe, North America, South America, India, and elsewhere, but eventually the courts accepted the dogma that no two individuals (even identical twins) have the same fingerprint patterns, and that fingerprint analysis was a reliable expertise. This dogma alone was not sufficient for establishing the power of fingerprinting. At least as important was the development of a distinct forensic profession of latent fingerprint examiners whom the courts trusted to declare whether or not two prints matched.

In most countries, a "points system" was devised for declaring matches. Although quantitative, in the sense that a specific number of points (16, 12, 8, etc.) provided a threshold for declaring a match, the number was not probabilistic, since examiners claimed that it was *impossible,* and not highly *improbable,* that fingerprints from different persons would match on the requisite number of points. This threshold system would mark a significant difference between fingerprint analysis and forensic techniques such as blood group analysis and DNA profiling which employed probability estimates. Scotland Yard abandoned its points system in the year 2000, partly because of its lack of probabilistic meaning, and for some time before that points had not been consistently used in the United States. Fingerprint experts who appeared in court continued to declare whether or not two fingerprints matched, without giving probability calculations. This was an either-or judgment, excluding the middle. To avoid ambiguity, the fingerprint examiner profession publicized the idea that examiners would deem "inconclusive" any and all fingerprint evidence that lacked sufficient clarity to make an absolutely certain judgment. Whether or not they actually adhered to this practice, examiners had such credibility as expert witnesses that, in the words of Anne Rafferty QC, chairman of the Criminal Bar Association in the United Kingdom, "If fingerprint evidence emerges when you are defending a client, then you tend to put your head in your hands. There is not really a question mark over it" (quoted in Grey, 1997). The testimony of a fingerprint expert was rarely challenged.

An Inversion of Credibility

When DNA evidence first was used in criminal investigation in the late 1980s, analysts spoke enviously of the "absolute" identifications provided by fingerprints. DNA "fingerprinting" borrowed the name of the established technique, but in the courtroom DNA matches could not be declared with the same absolute certainty—it was necessary to give probability figures. However, in the past several years, fingerprinting has begun to undergo a crisis. Two trends seem to be responsible for this crisis: a kind of feedback effect from DNA profiling, and (in U.S. federal law) changes in recommended legal standards for admitting expert evidence (Mnookin, 2001).

The legal history of DNA profiling in England and the United States shows a pattern of rise, fall, and rise again: rapid acceptance in the late 1980s, followed by challenges in the courts and science press from 1989 through the mid-1990s, followed by renewed acceptance at a stronger level. The initial credibility of DNA profiling can be attributed to an uncritical acceptance by scientists and the courts, while the later acceptance is often justified by noting that DNA profiling had "passed the test" and become a reliable scientific technique.[14] "DNA" passed the test so well that it became a standard for assessing the probative value of all other forms of criminal evidence, including its former namesake, "fingerprinting." Borrowing terms for describing controversies in science and technology studies, we use the term "postclosure" to describe this late phase in the establishment of a once-controversial technical innovation. Not only was the controversy settled, it set up a sequel in which "DNA" was used as an unquestioned model for challenging the epistemic status of *other* previously settled forms of evidence (particularly, and perhaps most interestingly, fingerprinting).

In the mid-1990s, just as the controversy about DNA "fingerprinting" was winding down, fingerprinting began to seem less "scientific" or even "old-fashioned" (McCartney 2006: 185). The relative credibility of the two technologies seemed to be undergoing inversion. The unquestioned credibility once assigned to fingerprinting was now assigned to "DNA," and the credibility of fingerprinting suffered by comparison.[15]

14. For popular accounts that herald the acceptance of "DNA," see Levy (1997) and Dawkins (1998). The eventuality of acceptance was widely forecast, even before it occurred, and announced tendentiously, most famously by Lander & Budowle (1994).

15. *Credibility* is a key legal concept that has deep resonances with a sociological approach to truth and truthfulness (Shapin, 1995). Credibility is commonly distinguished

Law, Science, and Society

In recent years there has been a convergence between science and technology studies (S&TS) and law and society (L&S) studies. This convergence reflects the fact that scientific and technical experts are increasingly prominent in legal disputes about health, safety, and environmental regulation. New forms of biotechnology challenge existing legal distinctions in areas such as intellectual property law, raising questions about whether genetically engineered organisms, or genes themselves, can be patented. Scientific knowledge and novel technologies challenge legal definitions of life, death, and viability. Such innovations sometimes reinforce traditional distinctions and regulations, but they also can be used to challenge or circumvent them. Even the practice of science itself is deeply penetrated by legal norms, regulations, and sanctions (Myers, 1995; Packer & Webster, 1996; Kevles, 1998). At a more abstract level, law and science offer distinct, but historically intertwined, procedures and traditions for determining facts, testing truth claims, and resolving disagreements (Jasanoff, 1995; 2005). When law turns to science or science turns to law, we have the opportunity to examine how these two powerful systems work out their differences.

In addition to offering a fund of topics for investigating the entanglements of science, technology, and law in modern (or not-so-modern) societies, research on law in science and science in law offers distinct methodological advantages. Mariana Valverde (2003b) articulates one such advantage very well, when she observes that ethnographic "science studies" of day-to-day laboratory practices enable sociologists and cultural anthropologists to gain insight into the unsettled arena of "science in action" (Latour, 1987), and to develop a more dynamic, contingent picture of research practices than was ever conveyed in philosophical and methodological writings.[16] Valverde then observes that the formal, public record of legal disputes yields a fund of material that is comparable to what the ethnographer turns up through extensive on-site investigation:

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from truth, but in actual situations of judgment there may be no practical means available to distinguish the "appearance" of truth and truthfulness from the actual truth of what is said or conveyed. Nevertheless, as Shapin observes, various "proxies" are used in actual situations as guides for assessing truth and truthfulness.

16. Ethnographic studies of laboratories include Latour & Woolgar (1986 [1979]); Knorr (1981); Lynch (1985a) and Traweek (1988).

> First, questions of evidence and of authority are often explicitly contested, with the contestations often forming part of a court's public record and/or going on in the public setting of the courtroom. Thus, unlike science studies scholars, who must gain access to social interactions that are not mentioned in scientific papers and that do not take place in public view, legal studies scholars have vast amounts of material—affidavits, trial transcripts, etc.—that can readily be analyzed, and we have automatic access to at least some of the struggles about what counts as evidence and who counts as an authority waged in legal settings. While recognizing that interviews and ethnographic methods can offer very important insights, and acknowledging that "the public record" is the product of a whole set of prior practices which are either black boxed or simply invisible, nevertheless, it seems to me that sociologists and anthropologists of knowledge should not neglect to explore processes that are readily accessible either through court observation or through the written public record of legal proceedings. (Valverde, 2003b: 1)[17]

From the side of S&TS, legal arenas not only offer the opportunity to *apply* an existing fund of concepts and research strategies to a significant institutional site, they enable us to *explicate* central concepts (including "science" and "technology") that are distinctly problematic. The very establishment of a field of S&TS might suggest that its members have specialized expertise about science, technology, and expertise itself (Collins & Evans, 2002). No doubt, members of the field are likely to be widely read on those topics, and to have thought deeply about them. And, in some cases, they can produce unusual insights and engage in sophisticated arguments about what is or is not a science, how technology develops, and what counts as expertise. However, those of us who turn away from the dream of a (social) science of science have less interest in developing better glossaries of the key words in our discipline than of understanding why and how those words prove so troublesome, not only for us, but more importantly for the parties whose actions and interactions constitute the institutions we study. As we shall see throughout this book, "science" and "expert" are contested terms in the law courts. In this study we are not interested in settling what those words

17. This quotation was taken from a conference paper (Valverde, 2003b) that was published under the same title (Valverde, 2003c). The published version did not include the quoted passage. For related discussions, also see Valverde (2003a; 2005).

mean in a context-independent sense. Instead, we are interested in how participants in the adversary system of the Anglo-American courts construe science and expertise.[18] American and English courts have a long history of wrestling with how to handle expert evidence (Golan 2004a, b). Such pragmatic construals may or may not compare well with philosophical or sociological conceptions of science, but the important point is that they *constitute what counts* in the legal system as science and expertise. Such constitutive work and its pragmatic effects are phenomena for this study, as we chronicle how DNA profiling was established as a supralegal form of scientific evidence.

Deconstruction

In S&TS writings about legal disputes, the word "deconstruction" is sometimes used to convey a sense of what happens to technical claims in an adversary system. "Deconstruction" in this context has only a superficial resemblance to the literary-philosophical method originated by Jacques Derrida.[19] The S&TS version of deconstruction tends to be more mundane,[20] as it refers to argumentative efforts to undermine scientific evidence by questioning the competence of the relevant scientists and the adequacy of their methods. Such arguments can occur among co-

18. Arguments in favor of treating social science concepts and variables as, in the first (and sometimes last) instance, ordinary words used in the social situations studied have a long history that is associated with pragmatist and interactionist approaches. See Mills (1940), Winch (1958), and Sharrock & Watson (1984) on the concept of "motives" in social psychological explanations of human actions. Also see Garfinkel (1967; 1991), and Garfinkel & Sacks (1970), and Blumer (1969) for general outlines of the sociological implications of such a reorientation toward basic concepts.

19. Jasanoff (1992: 348) provides a succinct definition of the mundane version as "nothing more arcane than the pulling apart of socially constructed facts during a controversy." Latour & Woolgar (1986 [1979]: 179) first used the term "deconstruction" to describe the way disputing scientists tend to take apart rival factual claims and methodological procedures: "The 'reality out there' . . . melts back into a statement, the conditions of production of which are again made explicit." See Jasanoff (1995: 211–15) for an example of how the term tends to be used in S&TS studies of science in law; for a critical exchange about the relation (if any) between the more mundane sociological application and Derrida's original, see Fuchs & Ward (1994) and Agger (1994).

20. To say they are "mundane" is not to suggest that they are uninteresting. Melvin Pollner's (1974; 1975; 1987) conception of "mundane reason" in traffic courts, psychiatric clinics, and other organizational settings illuminates how ordinary discourse about what happened (or *can* have happened) in a mundane world provides an empirical counterpoint to erudite metaphysical debates about the "problem" of reality.

workers in a laboratory, between rival scientists at a conference, or through an exchange of letters in *Nature*. The arguments can become extremely elaborate when they delve into previously unmentioned details of laboratory technique and possible sources of noise or artifact. Harry Collins likens scientific controversies to "breaching experiments," such as those deployed by ethnomethodologist Harold Garfinkel (1963; 1967) to illuminate taken-for-granted features of ordinary social activities. Like a "breaching experiment" in which Garfinkel's (1967: 42ff.) students persistently question intimates about the meaning of their routine actions and expressions, a scientific controversy in which routine laboratory practices are called to account can momentarily "explode" the quiet agreement that operates in a settled field of action. For sociologists, a breaching experiment is a research tool and didactic device for exposing taken-for-granted norms, background knowledge, and tacit practices. Studying a scientific controversy does not require an intervention on the part of the sociologist to "explode" the quiet agreement characterizing a form of life; instead, in heated exchanges among participants minor flare-ups, and sometimes major explosions, enable us to gain a vivid appreciation of the extent to which contrasting assumptions, leaps of faith, previously undocumented skills, and competing interests are featured in scientific practice but in a way that tends to be hidden when members of a field present a unified front. Two, partly related, senses of the word "explode" are relevant in this context: one is a noisy spectacle that disrupts quiet routine and peaceful coexistence, and the other an "exploded view" of inner workings, akin to diagrammatic conventions for showing the parts of a machine or the anatomy of a body. Of the two, the second explosion, more analytical and less spectacular, is more salient for the present study.

The metaphor of "opening the black box" expresses one of the principal analytical aims in studying contentious science. The expression derives from cybernetics. In a technical variation on the gestalt theme of figure-ground, an engineer can draw a "black box" around certain components in a circuit diagram, in order to place the details of those components in the background. In the S&TS field, "black box" has become a common term of trade with diverse uses. Often it refers to taken-for-granted, presumed, or collectively "forgotten" aspects of the social history of a scientific or engineering innovation. A different sense of "black box" has to do with the transparency or opacity of a device for a specific community of users. The components under the hood of a contemporary

automobile, for example, are far less transparent (both conceptually and mechanically) to an average owner, or even garage mechanic, than were, for example, the "guts" of a 1960s-vintage Volkswagen Beetle. Similarly, the electronic components of a contemporary radio not only are inaccessible to view, they are inaccessible to repair, unlike the tubes and wires of a vintage radio. Although black boxing holds many advantages for producers and users alike, it also can be problematic when things do not work as planned, or when facts and artifacts are incorporated into nonstandard settings.

Bruno Latour (1987) and his large following in S&TS have developed a theoretical agenda around the theme of opening up or deconstructing black boxes. This theoretical interest differs from the practical aim of reverse engineering in which an intact artifact is disassembled in order to reconstruct how it was put together. For Latour and others the scope of such reverse engineering is unusually broad and multifaceted. In addition to delving into how facts and artifacts are technically constructed, the S&TS deconstructionist explores broader horizons, interests, competencies, and networks. The didactic aim is to demonstrate how "heterogeneous engineering"[21] tightly weaves together social networks and material components, to the point of challenging the salience of the initial social-technical distinction. The lesson aims both to elucidate broader historical and social horizons of science and engineering, *and* to reveal how scientific and engineering innovations embody social actions and social forces in ways that escape the conceptual apparatus of standard social theories. In addition to being an analytical tool for S&TS, deconstruction also identifies what scientists and engineers *do* to their rivals' ontological claims during technical disputes, and thus it is a fine topic for social and historical studies of "technoscience."

In an esoteric field of science, the deconstruction of rival claims tends to occur in discourse among specialists. In an adversary system of interrogation, such as in the United States or United Kingdom, experts are explicitly (and often publicly) asked to explain, defend, and justify factual claims and technical choices.[22] It is a situation of "pure institutionalized mistrust" in Brian Wynne's (1989b: 33) memorable phrase. Social scientists rarely have the authority (or gall) to interrogate experts in

21. The theme of "heterogeneous engineering" was introduced by John Law (1986).

22. Just how such public inquiries are organized is itself an interesting issue. For an illuminating study of the play between public disclosure and closed inquiry in a science advisory panel, see Hilgartner (2000).

a sustained and adversary way, and so public interrogations of experts hold special interest for scholars who aim to delve into the "construction" of facts and artifacts.

It may be argued that "deconstruction" has become second nature to legal actors in adversarial systems of justice, and, indeed, the deconstruction of expert witnesses has a long and colorful history (Golan 2004a: 58). Unlike literary theorists and constructionist social scientists, courtroom adversaries rarely, if ever, call into question fundamental assumptions about truth, nature, and science. Courtroom deconstruction is practical and circumscribed. In jury trials in the Anglo-American tradition, the juror's "common sense"—nonspecialist understandings established as part of communal life—is assigned decisive importance. Contemporary courts resort to juries less and less frequently, but in trials and hearings in which no jury is present the presiding judge takes the role of the commonsense fact-finder. What is or is not a matter of "common sense" is itself subject to adversarial dispute and rhetorical play. "Common sense" is a wonderfully protean concept—sometimes upheld as a valuable public resource, and at other times condemned as a source of illusion and superstition—and in adversary courtrooms "science" and "common sense" are discursive categories that are actively worked into arguments and counterarguments.

Occasions in which expert evidence is deconstructed hold special interest for S&TS scholars,[23] because the adversary questioning skeptically explores the contingent production of the evidence. When successful, such questioning can explode just-so statements about objective facts to reveal hidden or actively suppressed sources of contingency and uncertainty. As Sheila Jasanoff (1995: 215) points out, these occasions can provide public tutorials (or "civic education") on the construction of expert evidence, thus encouraging reflection about expert authority and promoting a picture of science at odds with a realist or positivist version.[24] Jasanoff also notes, however, that courtroom "deconstruc-

23. What S&TS specialists *are*—(social) scientists, scholars, academics—is not easy to specify. The term "scholar" does not carry the weight (or baggage) of the term "scientist," and it can signal a lack of interest (or even disdain) for empirical detail. Our research is oriented to such detail, though it is not empiricist in the philosophical sense of claiming to derive propositions strictly from sensory observation (see Lynch [1993] for programmatic arguments).

24. A somewhat darker picture of the "civics lesson" promoted by the "applied deconstruction" of the State's evidence in a public tribunal is provided by Lynch & Bogen (1996: 14), in an analysis of testimony at the 1987 Iran-Contra Hearings.

tion" cannot be taken on its own terms as an instrument for exposing actual uncertainties and contingencies. Participants in a legal dispute rarely question the idealized conceptions of scientific method and fact to which they hold expert witnesses accountable,[25] and interrogators have been known to dramatize and exaggerate the consequences of minor departures from idealized conceptions of scientific knowledge and method (Oteri et al., 1982). Consequently, courtroom deconstruction cannot be taken as a reliable basis for probing and exposing the construction of expert evidence. Instead, consistent with the research policies in the field of ethnomethodology, it is necessary to view courtroom deconstruction as an interactional production, and not as a method for unmasking a reality that supposedly lies beneath the construction of expert evidence. In other words, courtroom "deconstruction" cannot be relied upon as a method for "laying bare" underlying contingencies and inherent uncertainties; instead, it is itself a contestable (and frequently contested) source of evidential claims and counterclaims.

The chapters that follow examine courtroom dialogues and judicial summaries of trials and admissibility hearings in which DNA profile evidence (and fingerprint evidence) was challenged. The challenges are of interest for the way the dialogues explicate unresolved tensions about science, expertise, and laboratory practice. The overall frame of the study is that of a history of a technical-legal controversy that began in the mid-1980s, was resolved a decade later, and was then followed by controversies about *other* forms of evidence. Analysis of selected excerpts from key cases will provide materials for specifying the phases and articulating the thematic elements of the controversy.

The Problem of Expertise

The problem of expertise is a thematic element that pervades the DNA fingerprinting controversy and also is a topic of lively debate in the S&TS field.[26] Briefly, it is a problem of how to reconcile the role of expert

25. For a critical account on the general theme of the idealization of science in law, see Caudill & LaRue (2006).

26. As Burney (2000: 8), has noted, "'Experts' are key figures in the history and historiography of the modern state." See also Golan (1999, 2004a). Turner (2001) provides a summary and critique of the "problem" as it is often posed in social and political theory. The problem of jury understanding of expert evidence (Edmond & Mercer, 1997) is a circumscribed variant of that problem. Empirical studies of "lay expertise" developed, for example, by patient and activist groups (Epstein, 1995; Rabeharisoa & Callon, 2002), or of local

knowledge (which is often assumed to be beyond the critical comprehension of the "lay" public) with the ideals of public participation in a liberal democracy. In a courtroom inquiry, the nonexperts have limited acquaintance with, and very little opportunity to learn about, the technical matters that expert witnesses are asked to present. Consequently, jurors (as well as many judges, prosecutors, and defense attorneys) can be at the mercy of experts who make uncontested assertions about what the evidence shows. And, when the experts disagree, the lay participants have no technical basis for deciding which claims to believe. Legal scholars and social scientists disagree about the extent to which jurors are capable of understanding expert evidence. Many argue that judges and jurors are ill equipped to distinguish genuine expertise from "junk science." However, others argue that the difficulties jurors experience with expert evidence can be relieved by more effective presentation of expert evidence in court. According to this view, juries are capable (or, at least, no less capable than judges are) of grasping what they need to know to decide a case.[27]

In the present study, we do not take up the empirical question of what jurors actually understand, or can be brought to understand. Instead, we address how actions and pronouncements by courtroom participants (principally, attorneys, judges, and expert witnesses) implicate what a silent jury can understand or appreciate. Although jury trials occur in a small, and decreasing, proportion of criminal and civil cases in the Anglo-American courts, the jury continues to stand proxy for the common citizen's place in the justice system.[28] The jury is also a "virtual" presence: even when they settle cases out of court, litigants frequently argue about how a jury would or could react to elements of the case at hand (Burns, 2001). Legal precedents and rules of evidence traditionally

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knowledge grounded in practical experience (Wynne, 1989b), provide dramatic examples of the limits of expert knowledge and the leading role in knowledge production that is (or can be) assumed by groups with no claims to scientific credentials. In contrast, Collins and Evans (2002) credit the substantive reality of expertise, and try to distinguish among various degrees and kinds of expertise that have a role in social and political life.

27. For discussions of the general problems associated with the role of expert evidence in legal and legislative decisions, see Smith & Wynne (1989), Wynne (1989a), and Jasanoff (1995). Contrasting views of jury competence are expressed by Huber (1991), who emphasizes that jurors and other lay participants in trials cannot tell the difference between genuine and "junk" science, and Gigerenzer & Hoffrage (1995), who claim that jury understanding of probabilistic evidence depends upon how it is presented.

28. The *New York Times* reported that in the past twenty years, jury trials ending with verdicts in the United States had dropped from 2 to 1 percent of civil cases and from 10 to less than 5 percent of criminal trials (Glaberson, 2001: A-1, 17).

reserve the "ultimate issue"—the judgment about guilt, innocence, or liability—to the trier of fact and restrict the role of experts to "informing" the court.[29] Procedures of cross-examination and the institution of jury deliberation build in social-interactional and documentary mechanisms for "testing" evidentiary claims, exposing and dramatizing inconsistencies, and airing opposing views. In traditional jurisprudence, such procedures for openly presenting and testing evidence before a lay audience are likened to powerful machineries for exposing "the truth."[30]

Court cases and public debates about DNA testing and other forensic techniques frequently raise questions about the scientific status of expert evidence. By equating DNA evidence with science, proponents draw upon a powerful source of authority for legitimating their stories and arguments, while undermining the credibility of evidence presented by their adversaries.[31] The possibility that such authority can be exaggerated or otherwise misused was not lost on the courts.

Conclusion

This chapter outlined how the history of technical/legal controversy about DNA testing is an apt subject for studying how an important social institution relies upon, incorporates, and translates scientific evidence. The chapters and interludes that follow take up the story in its various phases and use specific legal cases and technical disputes to il-

29. "Ultimate issue" restrictions have been greatly lessened in many jurisdictions today (see, for example, U.S. Federal Rule of Evidence 704a). Cole (2001a: 209) discusses appeal cases in the United States in the late 1920s and early '30s in which the issue of jury usurpation pertained to the testimony of fingerprint examiners. In some cases the appeal courts held that fingerprint examiners encroached upon the jury's province when they described matching latent and rolled prints as matters of "fact" rather than expert "opinion." See *Iowa v. Steffen* (1930).

30. John Henry Wigmore (1940: 29) famously stated that the presentation and testing of evidence in the trial court is "the greatest legal engine ever invented for the discovery of truth."

31. The quotation marks we place around "science" are not meant to signal skepticism about science, as such, though we recognize that scare quotes are frequently used in the science studies field to "neutralize success words" (Stove, 1984: 23) such as "discovery," "fact," and "truth." We simply mean to denote that the word "science" (along with its grammatical variants and synonyms) was deployed and appealed to in texts and speeches by judges, lawyers, legal scholars, advisory panels, and the science press. The quotation marks are intended to signal that the quoted item is a significant term with recurrent use in the institutions we are studying.

luminate the issues and problems at stake. Our chronicle begins in the mid-1980s, when DNA fingerprinting was first developed for forensic investigations. We then move to the late 1980s and early '90s, when forensic DNA testing was challenged in the courts, the international forensic community, and the science press. The hyperbolic label "DNA wars" headlined a particularly intense flare-up that attracted widespread media attention.[32] Opposing views were aired in popular articles, technical publications, and legal testimony. Public displays of hostility among stereotypically sedate and studious scientists and the "culture clash" between scientists and lawyers provided newsworthy topics for the science press.[33] Aside from the entertainment value of the spectacle, the controversy highlighted key issues for scholarship on the role of science in law. The public discord helped to undermine the quiet agreement among legal and scientific experts about the effectiveness of a new criminal justice tool, and the trials, admissibility hearings, and official evaluations of DNA testing provided insight into a contentious relationship between scientific and forensic practices. More generally, the trials and investigations raised questions about the relationship between legal and scientific conceptions of evidence and methodological adequacy.

The controversy began to wind down in the mid-1990s, but then a very interesting phase began in which an "inversion of credibility" occurred. DNA evidence became an unquestioned ground for challenging all other forms of criminal evidence. We focus particularly on fingerprinting, because as noted earlier it had long held a status as a nearly unassailable form of criminal evidence, but starting in 1999, that status was challenged in a series of admissibility hearings. This postclosure phase of controversies about forensic DNA testing takes us to the present time, when "DNA" is equated with truth, and DNA databases grow ever larger. Recalling a theme that runs throughout this book, the concluding chapter emphasizes that this seemingly unassailable, transcendent form of criminal evidence remains bound up with stories that are infused with contingent judgments about the mundane meaning and significance of evidence.

32. For use of the expression "DNA war," see Thompson (1993). War analogies were also used by science journalist Leslie Roberts (1991) in her coverage of the *New York v. Castro*.

33. The theme of "culture clash" was headlined in an article by Roberts (1992).