

What's Scientific About Forensic Science? Three Versions of American Forensics, 1903-1965, and One Modest Proposal

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ABSTRACT

Growing attention to the philosophy of forensic science in recent decades has sometimes included the question: "what kind of science is forensic science"? Yet there has been little discussion of how that question has been differently construed in terms of period, place, and prevailing anxieties. Following an examination of the unique character this question must have in an American legal context, this article reviews three modes/phases of response, rooted successively in individual authority, comprehensive method, and institutions of flexible problem-solving. The conclusion applies this complex legacy in two ways: first to clarify areas of incoherence and tension in recent attempts to underwrite forensic sciences, and second to supply a fuller framework for Max Houck's argument for the essentially historical character of forensic science.

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INFORMATION

ACADEMIC FORENSIC PATHOLOGY: THE PUBLICATION OF THE NATIONAL ASSOCIATION OF MEDICAL EXAMINERS FOUNDATION

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INTRODUCTION

With the possible exception of physics, it is hard to find a modern science whose practitioners are more alert to the philosophy of their enterprise than are forensic scientists. Among many reasons are the serious consequences of getting it right—I titled another treatment of this issue, the “Guts of Rights”; that is what forensic science has long been (1).

Yet introspection within professional communities is rare: more common is a united front. And remarkably that critical appraisal comes despite great popular faith: forensic science is commonly seen as resolving conflict rather than being subject to it.

While the most recent wave of reflection may have been triggered by the emergence of DNA-based identification, it goes beyond, to involve concerns ranging from training and oversight to practices of inference and structures for ensuring impartiality (2). At issue is scientificity itself, conceived by Crispino et al. as the articulation of “simple principles,” that “minimum of fundamental, primary, or general laws or truths, possibly axiomatic, from which others are derived.” They seek a pedigree like that of other efforts of “the human mind . . . to produce simple, unified and coherent representations of nature since at least the scientific revolution” (3). Similarly, a chapter on “The Principles of Science and Interpreting Scientific Data” of the 2009 report of the National Academy of Sciences/National Research Council Committee on Identifying the Needs of the Forensic Sciences Community opened with an epigraph from Isaac Newton’s “Rules of Philosophizing” (not in fact a quotation, but a paraphrase, emphasizing empiricism, reason, and acceptance of the tentativeness of scientific knowledge) (4). The prominent practitioner Max Houck (one of the Crispino coauthors) made the philosophy of forensic science subject of his doctoral dissertation (5).

The enlistment of Newton and search for axioms suggests reliance on the deductive-nomological (or D-N) model of the classical physical sciences as the model of scientificity: general principles will subsume both

observations and mid-level theories (6). Thus the NAS/NRC authors: “Ultimately, the goal [of science generally] is to construct explanations (‘theories’) of phenomena . . . consistent with broad scientific principles, such as the laws of thermodynamics or of natural selection.” Yet applying these canons to forensics is a stretch. While the authors assert that “the same principles hold when applying known processes or knowledge,” their analogue to “formulating and testing hypotheses,” is care—in “preparation and analysis of samples and the interpretation of results” (7). What survives the translation is simply a common gesture toward rigor.

I come to these issues from the history and philosophy of science, having, for four decades, studied expert disagreement in various settings (8). I sympathize with the concerns, but have misgivings about the approach. The best way to capture the distinctiveness of forensic science will be to avoid statements—for example, “Forensic science is an applied science based on the laws of physics and chemistry”—that seem to reduce it to a generic science or to other fields (9). The presumed unity of science is itself illusory. The D-N model is characteristic only of some domains and is not well suited to an enterprise that produces composite inductions (probable accounts) in particular cases. Finally, it seems unwise (and futile) to insulate the search for philosophical authority from the contexts of application, the legal institutions that are, after all, its reason for being. Structural matters peculiar to the law-science interface, calling for institutional reform, will require approaches grounded neither solely in science nor in law, but in what Cole has called the “epistemic culture” of their interface. For Cole recognizes that the problems—anxieties about status, concerns about performance, and frustration with institutions—are less philosophical than sociological (10).

They are historical too. Recent changes in the substance of forensic knowledge should not hide the fact that issues involving the relations of expert authority, investigative practice, and public decision-making are not new, despite having been underexplored by historians of law, or of science and medicine (11). And, as in other fields, what scant histories we have are often

used to consolidate knowledge rather than problematize it. Thus, celebrated predecessors are enlisted to illustrate maxims or act as exemplars. Edmond Locard's "exchange principle" motivates investigation and guides interpretation. Hans Gross is the prophet of crime scenes; pathologists have Bernard Spilsbury. Usually such persons are stick figures, makers of pithy statements. Context is unimportant; the most famous (Sherlock Holmes) is fictional.

Here, I use history differently—to focus on sites of tension in the use of expert knowledge and of ambiguity in performing the role of expert. The next section provides background, in combining a review of key differences between legal and scientific institutions as sites of knowledge-making with a very brief sketch of the coevolution of adversarial common-law legal systems (i.e., those in use in England and America), with prosecutorial and policing institutions, and with the enlistment into that nexus of a largely foreign (German and Italian) institution of specialist expertise, medical jurisprudence.

The bulk of the article treats the institutionalization of forensics into modern American jurisprudence, roughly from the progressive age around 1900 to the 1960s, in terms of three successive models of expertise: "Embodied Authority," "the Magic of Method," and "Institutionalized Artistry." At issue is not so much technical abilities as representations of what good forensic authority looks like. While I hope to show why these models succeeded one another as they did, we would be wrong to imagine a simple story of progress. Rather, as I hope readers will see, each perspective persists.

An historical framing of contemporary forensic reform discussion, then, unites underrecognized tensions stemming from the complicated history of jurisprudential institutions themselves, and especially their struggles to incorporate expertise with the modes of authority experts embodied. In the last section 4 I use the final vision of expertise, that is, as an integration of all applicable science to reveal a unique contingent event, as a departure point for a descriptive and prescriptive philosophy of forensic science. There I take

up Houck's call to see forensic science is an historical inquiry—a far cry from the deductive-nomological model.

FORENSIC SCIENCES: HERITAGE AND CHARACTER

The empirical foundation for this article is an ongoing survey of programmatic statements in forensic science and its predecessor domain, forensic medicine, a term generally synonymous with "legal medicine" or "medical jurisprudence." Though our modern "forensic science" is narrower, it remains imprecise. Considered as an institution it is an ancillary to policing; as a set of ways of knowing, it is applicable in civil and administrative as well as criminal matters. The statements come from textbooks, exhortatory pamphlets, and editors' justifications for new journals. But even in these settings, reflections are rare—philosophies are treated as implicit, familiar, and unproblematic. Texts, after all, are not to question knowledge but transmit it; reliable rule-following will be vital for regular jurisprudence. Change, evident in evolving technique, is presented as incremental, its magnitude and implications visible only at a macroscale.

The Distinctiveness of Forensic Science: Elements of Application

As Cole points out, decision-making in law (here American law primarily) differs significantly from that in academic science (10). Though I have altered terms, the following are versions of some of the differences Cole identifies:

Proceduralism

As is not the case in the open commons of science, in forensic settings who can say what, how, and when, is rule-bound, as in the notorious Daubert test, which qualifies experts and expertise.

Definitions and burdens of proof

The precedents, definitions, or burdens of proof that operate in legal decision-making are foreign to science, and likely to seem imprecise or arbitrary. On

matters like cause, complex or continuum phenomena may become binary or formulaic.

Adversarialism

In science, contestation of claims will be incidental; the structured adversarialism of legal proceedings imposes it. What claims actually are contested depends on advocates' strategies, but what is in science understood as process of clarification may be obfuscation; not better knowledge but no knowledge. What philosophers of science label "agnotology," the purposeful cultivation of ignorance, is a vital and virtuous tactic in such settings (8,12,13).

Artificial closure and formal resolution

Within science an issue may remain open indefinitely, while a trial is a means of timely and permanent closure (14). Scientists may accept claims with varying degrees of confidence; laboratories, it is noted, are places to make mistakes (15). Here, the NAS/NRC committee, reflecting the profundity of the DNA revolution, challenged jurisprudence to accommodate tentativeness. "Scientists continually observe, test, and *modify* the body of knowledge," it declared. "Rather than claiming absolute truth, science *approaches* truth" (7, p112). Yet the welcoming of tentativeness in policy settings has been seen, and rightly, as open season for the perpetual postponing of resolution (16,17).

Lay decision-making

As is not the case in science, persuasion of nonexperts will be important; plausibility may be more important than rigor (16). Particularly in conjunction with adversarial advocacy, this last point is the most peculiar. Realms of competence distinguish professional from lay judgment. A premise of professionalism is that only professionals are qualified to judge their peers. Yet the adversarial system leaves the finding of fact to incompetent persons, responding to contrived contestation.

Institutional Evolution

These features are products of a long evolution. The common law heritage celebrates adaptiveness; yet improvement of parts may be maladaptive for the whole, leading to practices that are not only absurd—like employing authority and then making it nonauthoritative—but that endanger impartiality. Such may be the case with the asymmetrical control of forensic knowledge-creation, as when investigations are managed for prosecution-oriented policing.

Central here is the absorption into English law at the end of the 18th century of a bastard version of continental forensic medicine. Though it oversimplifies, European legal systems may be divided between custom-based systems of common law (as in England, and later the United States), and code-based systems as in France, Italy, and the German lands. In these latter, "inquisitorial," systems judges judged, but only when investigators had brought a compelling case that met explicit standards of proof. The institutions and professions of medical jurisprudence that arose chiefly in Germany and Austria between the 16th and the 18th centuries did not merely advise these judges. On issues within their purview they found fact, constituting judicial authority. Whether a wound caused a later death or an infant had been born alive was for them to say. Often, too, there were specialist appellate institutions—boards of higher experts for the hardest cases, often university professors or, later, members of state-funded medicolegal research institutes (the first was in Vienna in 1818), tasked with improving knowledge in their specialized areas (18–20).

English criminal law too evolved rapidly during the 18th century. What John H. Langbein has called "the accused speaks trial," a communal altercation-resolution in which an accused person responded to accusers before a moderator judge and a jury of neighbors (leading, ideally, to resolution or compensation), had given way by 1800 to "lawyerization," first of the defense and then of a prosecution. In this change, notes Langbein, victory in a "combat" of orchestrated impressions biased by wealth replaced the goal of reconciliation. Moreover, he notes, in such trials, "none of the . . . participants . . . [was]

responsible for truth-seeking”; none, in effect, deserved the designation, “finder of fact” (21). Still, because prosecution remained private in England until 1879, there was potential for symmetry. A public prosecutor, first appointed and later elected, was mainly an American innovation.

State-based detection had been an anathema to English civil liberties. Other than the minimal efforts of magistrates or grand juries, no institution existed to find physical fact. In America, the coupling of the public prosecutor with “scientific policing” over the first half of the 20th century would bring about the concentration of power that currently exists. Commanding moral, popular, and cognitive authority, with absolute discretion over investigating, charging, and resolving cases, the public prosecutor held “more control over life, liberty and reputation than any person in America,” noted Attorney General Jackson in 1940 (22,23). Growing forensic means helped justify ends, which were, as in common law private prosecution, victory in single combat. Representing a union of truth and justice, that victory would register as electoral success, with structural bias reinforcing self-interest.

The flurry of English and American textbooks of medical jurisprudence after 1788 reflect enlistment of science in this combat. While some authors looked to science to rationalize jurisprudence, others emphasized the risk to reputation doctors faced unless they appeared to know something of the field. If authors express envy at continental medicolegal research process, or reflect on how much better it would be to present the whole truth to a single judge, generally they downplayed differences in legal systems. It would do no good to admit that they were pretenders, lacking both the substantive authority of specialized training, and (with exceptions like psychiatric commitment, and later, cause of death (24)) any explicit legal authority. Authorities without authority, they acted as the playthings of attorneys.

BEING AN EXPERT

The three episodes described in following section both reflect that heritage and were part of its ongoing

evolution in America during a period in which forensic science became institutionalized within legal institutions.

Embodied Authority: the Expert as Skeptic 1903

My first version of forensic expertise is founded on a curious admission of expert vulnerability, occupying four pages of the seven-page introductory chapter, “On Expert Evidence,” in the two-volume *Text-Book of Legal Medicine and Toxicology* (1903) edited by Frederick Peterson (psychiatrist) and Walter S Haines (toxicologist). This compendium of specialist-authored chapters reflects the growing complexity of the many fields relevant to forensic questions, and the absence of any profession or institution to unite them: the police laboratory staffed by trained criminologists was decades away.

The authors open by outlining the plight of expert judgment—cognitive, epistemic, and institutional.

The phenomena we are called upon to consider in forensic medicine are often indefinite, shadowy, and illusory. The observer . . . is hampered by the uncertain evidence of his more or less imperfect senses, sometimes by . . . undisciplined intellect, by the perversions of hazy memory, by limitations of . . . general knowledge and experience, perhaps by the modifying influence of emotions, and, very rarely, . . . by a tendency to deliberate deception and misrepresentation.

A later summary of “chief sources of error” adds “deliberate fraud as in all species of quackery” and “willful perversion of facts by pseudo scientists” (25).

The first issue—the “indefinite, shadowy, and illusory” thing to be studied—is distinctively forensic. In most sciences, the observer dictates the subject of study, yet the clever criminal may disguise the crime itself so that there appears to be nothing to study.

The next items address general human failings. Some, like the mechanics of perception itself (“narrow and dim avenues”), its disciplining, and the library of experience which supplies the basis for judging perceptions, affect cognition itself. Yet the authors raise

troublesome questions about experience and authority: witnesses with “limited horizons” in “cognate sciences” will simply err with confidence.

Beyond fullness of memory and breadth of training is the “imaginative faculty”—ability to see what is absent as well as what is present, and to convert facts into narrative. Here, under emotions, arise further problems. One, arising in pride, is over-valuing the fruits of one’s own imagination. Another, rooted in habit, is reliance on a familiar dramatic form (something like letting the mean stand for the whole population). Last is a problem philosophers rarely encounter: interlocutors—here the authors worry about fellow professionals, not witnesses or criminals—who dissemble.

I call this introduction “curious” because we expect professionals to exude self-confidence. By contrast this deconstructive lament might make one despair of any application of expertise to public decision-making. Modern forensic credibility is also tied to laboratory evidence. Here, though Peterson-Haines include sections on X-rays, semen, blood, and hair assessment as well as toxicology, there is no sense that tests or methods can displace judgment.

Period and author go far to explain this stance. Considered in context, this testament of ignorance is in fact an advertisement. Late 19th-century American medicine suffered a crisis of authority. Given low standards of training and licensing, a mere degree meant nothing. Confidence was the mark of the sectarian or quack. As Rosenberg (26) and Mohr (27) have demonstrated, ambitious prosecutors were fully prepared to exploit that false certainty, often playing the cards of race and radicalism, both to promote and to assuage public fears. At the same time, European clinical researchers were showing that many interventions were ineffective or harmful. Illnesses would resolve or not. Within this “medical nihilism,” progress, if it came, would come from research. Elite American physicians successfully marketed that skepticism as responsible authority. It was, however, an embodied expertise, immensely valuable to the public if never reducible to the error rates a Daubert test now requires.

One such physician was Peterson himself (1859-1938), a Park Avenue neurologist, likely author of this skeptical manifesto. Neurology, a new specialty, itself represented the challenge of European research culture to contemporary warehouse psychiatry. Neurologists’ expertise was especially valuable in medicolegal matters. They claimed authority both to diagnose and to explain on empirical and somatic grounds. Even if they could not treat, their out-patient practices helped destigmatize mental illness.

Peterson was a star. Following his 1879 Buffalo MD, he spent three years in Germany, returning to appointments as pathology professor at Buffalo and pathologist in the local asylum. Then, after three years as assistant at the Hudson River Asylum (and more European experience) he began private practice in New York. His research focused on electrotherapeutics, but he was an expert on cerebral palsy, and a pioneer in asylum reform and provision for persons with epilepsy. He would become patron of/and expert in many fields: child welfare, neurological research, Chinese paintings, and nature conservation. He was a significant poet. The legal medicine textbook was his third, following an American edition of the Viennese professor Eduard von Hofmann’s *Atlas of Legal Medicine* (1898), and *Nervous and Mental Diseases* (with Archibald Church of Northwestern) (1899). It appeared during his presidency of the New York State Lunacy Commission (1901-4); he was also Professor of Neurology at Columbia from 1903 to 1915 (28).

One of such broad learning and manifest achievement could readily admit uncertainty in principle precisely because he was in practice so patently authoritative. Thus, against the categorical oversimplification so prevalent in American medical jurisprudence, Peterson was representing the depth of European experience. The Ritter-Peterson *Atlas* is a visual proof-text: its pages, photographs of parts or conditions of human bodies represent the record of human variability laboriously accumulated in the forensic institutes. Ritter’s Austria was not only the capital of medical nihilism but home to other sciences of the unique, like psychoanalysis and the art of detection itself, which have been recognized by Ginzburg (29). Peterson’s theme of

remaining critical of one's inductions echoes the Austrian magistrate Hans Gross's 1893 famous *Handbuch of Kriminalistik* (30). Gross, however, was writing of "self-discipline" in *detection* itself, not in *individual* branches of forensic expertise.

The Magic of Method, 1913-1930

The "science" and "methods" absent from Peterson's and Haines's 1903 introduction are plentiful in the second edition (1923, retitled *Legal Medicine and Toxicology by Many Specialists*). The skeptical language remained, though scattered among celebrations of new technologies (31).

A "scientific policing" was coming to America. It involved adopting European methods, though which and how were less clear. In Europe, noted the first editor of the journal of The American Institute of Criminal Law and Criminology, the flagship reform organization established in 1909 and headquartered at Northwestern University, "the value of cooperation among lawyers and scientists in promoting improvement in the criminal law has long been recognized." The Institute's concern was procedural reform, including penology, much more than expert investigation or policing, though its founders included among seven committee topics, "establishing commissions of specialists for giving expert testimony." But the most immediate need was to learn from Europe, especially England (32). That made sense with regard to legal heritage, less so for forensics. The journal's third volume, however, carried a brief piece on the "Scientific Police" by Salvatore Ottolenghi, Professor of Legal Medicine at Rome. Here, Ottolenghi described two training courses, one for lawyers and police administrators and another for front line officers, that he had been developing since 1894. His rhetoric anticipates Crispino et al. and the NAS/NRC. Not Newton, but Galileo, was the hero. "Galilei's experimental, objective, and rational method" had "made experimental science possible"; it would, "applied to the police, serve as a safeguard against errors." With it, he would "teach the officers and the judges how to observe, to reason, and to be absolutely impartial in investigations and reports." They would use the "scientific method"

in inquests, in investigations of "accused persons," and in examining witnesses (33).

The "scientific police" that would arise in America came as the establishment of a few scattered municipal police laboratories in the 1920s. Again, Northwestern was a center. Its Scientific Crime Detection Laboratory (est. 1929, and independent from the University) would publish *The American Journal of Police Science* (1930). Its first director, Calvin Goddard (1891-1955), was a Johns Hopkins-trained physician who had moved into ballistics analysis (34,35). Again, the emphasis was Europe; the journal would be an organ for translations (36). (In 1932, the two Northwestern journals merged with "Police science" being a separate section at the end of each issue of the *Journal of Criminal Law and Criminology*. From 1951 to 1971, however, "Police Science" would be included in the journal's title.)

Yet Goddard's "police science," concentrating on objective means of assessing physical evidence, was quite different from Ottolenghi's "Scientific Police." Where Ottolenghi's approach was dynamic, criminal-centered, and preventive, Goddard's was analytic, crime-centered, and reactive.

Ottolenghi's science included techniques like photography and fingerprinting, but they served the program of his mentor Cesare Lombroso (or equally of his French colleague Alphonse Bertillon), of making criminology a predictive social science. Central to this effort were programs of anthropometrics. Crimes were not random; they had social, anthropological, and ultimately "biological" determinants. Criminology was thus a matter of "moral hygiene," an offshoot of public health. For Ottolenghi the foundation of "scientific police" was clinical epidemiology practiced in prisons not hospitals, leading to generalizations that would guide surveillance and apprehension. They would guide therapy too. Criminals (or would-be criminals) were patients: where brutal police tactics encouraged crime, "kindly" science might prevent it. The impetus for Goddard's laboratory was the St. Valentine's Day massacre of 1927. Goddard's firearms analysis convicted the shooters (36).

Burney and Pemberton have seen these contrasting versions of scientific policing as reflecting the impact of Gross's *Handbuch of Kriminalistik* (30). It had effected the splitting off of what would become criminalistics (our modern forensics) from criminology. Given the abhorrence many historians have felt for the eugenic, racist, and totalitarian implications of the anthropometrists' program of classification and surveillance, the science of post hoc crime reconstruction can seem refreshingly liberal. That such approaches flourished in Britain and America may seem to reflect constitutional guarantees, like the presumption of innocence.

Constitutions did matter. Reporting in 1915 on his two-year study of European policing for the Rockefeller-funded Bureau of Social Hygiene, the lawyer-reformer Raymond Fosdick (1883-1972) (later head of the Rockefeller Foundation), noted that differing views of civil liberties were reflected in differing purposes of policing—keeping the peace in England and America as opposed to managing the population on the Continent—and differing practices. English detection was indirect and circumspect; a German detective might enter a dwelling on minimal pretext or round up the usual suspects to be “sweated,” allowing elimination of some or even generating a confession, the primary goal in inquisitorial systems (37).

Yet differing constitutions were far from the whole story. Differences reflected professions and cultures, organizations and education, as well as social customs and identities. Nor was the split between criminology and criminalistics sharp or always welcome. Gross himself had written more about criminology—sociology and psychology—than about investigation. August Vollmer, the Berkeley California police chief credited with giving “birth to modern [American] criminalistics” (38) was a follower of Gross, but initially of Gross's criminological work, to which he had been introduced by the radical Berkeley biologist Jacques Loeb. For roughly a decade, the scientific policing he promoted in Berkeley was the Ottolenghian version. He only discovered the potential of scientific criminalistics serendipitously around 1915, through meeting a local botanist (Albert Schneider, 1863-

1928), who found investigation more interesting than teaching. Vollmer would then set up the Los Angeles Police Laboratory in 1923, the first significant American lab (39).

Skills (and authority) were in turn tied to status. Fosdick was struck by the high stature of European policing, especially in Austria and Italy. Leadership positions required success in a competitive examination following five years' doctoral training as a jurist (40). He marveled at Ottolenghi's schools with their rigorous admission standards, at the 27 district police laboratories in Italy staffed by “trained investigators” with the “the latest apparatus.” Yet only 7 of the 53 pages in his chapter on “Methods of Crime Detection” concern physical evidence. More important were the surveillance sciences—means of identification, classification, and data management that went beyond “crimes and crime methods” to the “pathology and psychology of criminals” (40, p192,361-367). Poorly educated American officers could not aspire to the learned sensitivity needed for effective interviewing of suspects (or witnesses), Fosdick thought; reconstructing crime scenes from physical evidence might be a fallback.

Early graftings of European approaches onto American policing can be risible, as in the 1935 *Modern Criminal Investigation*, a collaboration of Dr. Harry Söderman, head of the Stockholm Institute of Police Science and John Joseph O'Connell, a New York City inspector and Dean of its Police Academy (41). One can easily separate the European from the American sections, the scholarly psychological criminology (including Freud) from the mean streets of Prohibition-era America. While Söderman-O'Connell might work as an elementary text, an American reader who wanted to follow-up the footnotes would need several European languages—among the 200 plus items in the bibliography, roughly a third are in German, another third in French and Scandinavian languages. Including translated works would skew the ratio further. The conspicuous American expertise was in firearms.

Some of the anthropometrists' techniques would live on in America, but as aids to detection more than

means of surveillance—they are evident in the “mug” shot and the wanted poster. Yet their utility would be hampered by the chaos of multiple jurisdictions. A reading of the last volume, on “Police Conditions,” of the 1930 Wickersham Commission on Law Observation and Enforcement reveals the primitiveness of local policing. Laboratories were not even on the list of equipment needs (42).

Institutionalized Artistry, 1947-1964

What struck Fosdick about the new European laboratories was facts. Detection was undertaken “as medicine and biology have been approached—from the standpoint of facts. A burglary is a fact, the tools with which it was accomplished are facts, every incident surrounding its commission is a fact.” A “science of crime detection,” he continued, “springs from the analysis and systematization of these facts” (43). Perhaps, but who would make the facts? Texts did not ensure skills, much less experienced judgment on a wide range of issues that might arise in a police laboratory.

It is worth noting that the initial faith in fact rested on the fortuitous and nearly contemporaneous recognition of several means of differentiation useful in investigations: Bertillon’s standardized characterization of appearances, documented photographically, the apparently unique fingerprint, and, most counterintuitive (because firearms were products of precision machinery), the signature each gun left on a bullet. Early American police laboratories were mainly photography studios, with provisions for microphotography. Detectives, not technicians should staff them, Söderman and O’Connell believed, but a gap remained between making the facts one could make and creatively applying science to answer questions about a crime (41, p426-429). Thus, what forensic science was, was tied to the limited skills of its practitioners and the circumstances of their practice. According to Kirk and Bradford (38, p5), skills still were minimal in the mid-1960s.

My subject in this last section is Paul Leland Kirk (1902-70), University of California-Berkeley professor of technical criminology and rock star forensic

expert of the 1950s, owing to his intervention in the Sheppard murder case (44). Kirk, though controversial during his career, has recently attracted notice from those interested in the philosophy of forensics for his 1963 articulation of “individuation” as a key principle. I focus on Kirk here with regard to his distinctive representation of that central problem and for his associated views of the professional identity and requisite skill set of a general criminologist. Briefly, Kirk sought to steer the forensic enterprise away from routine data-collection and toward creative hypothesis-testing. In doing so, he was departing from the Peterson-Haines model of a stable of professionals whose experience might have some bearing on the circumstances of a case, and equally from the routine grinding out of fact that Fosdick had admired. Instead Kirk worked backwards from the particulars of crime to the means needed to reconstruct it. Obituarists saw him as a forensic artist adept at finding technical means to realize a narrative vision (45).

Kirk entered forensics accidentally, lacking background in ancillary domains like medicine, social hygiene, policing, or even toxicology. But nor was he a typical biochemist, his home discipline. His specialty was analytical instrumentation for ultramicroanalysis. A reputation for technical expertise based mainly on a series of papers in the German journal *Mikrochemie* led to his recruitment by the Manhattan Project where his task was to measure minute amounts of plutonium (46,47). A slow move into technical criminology began about 1937. Vollmer, who had become professor of Police Administration at Berkeley, was probably key (39,48,49). By the late 1940s, Kirk and his students were working on a variety of means of differentiation suited to use in police labs. Some, like him, divided their research between analytical biochemistry and forensics, publishing in *Mikrochemica Acta* and in the Northwestern *Journal of Criminal Law and Criminology* (which had absorbed *The Journal of Police Science*). The breadth of research is remarkable: hair, soil, glass, fibers, tool marks, typewriter ribbons, and ball point pen ink, but also instrument-based studies—of gas and paper chromatography, and of density gradients, which might be used to determine the sources of ash (47,50–58). That

extraordinary variety was the basis of Kirk's unique approach, one tied to his articulation of individuation: that there is in principle no kind of evidence having potential probative value in individuation, that one (i.e., a single, well-trained and properly equipped *scientific* criminalist) cannot isolate and measure.

With respect to the philosophy of science, three points stand out. First, many of Kirk's explorations of modes of differentiation were theory-driven. From physical and chemical principles, he deduced what ought to be observable with well-designed instruments. Besides density, these included volatility and refractive indices. His famous work on blood spatter was predicated on Newtonian physics: trajectory, viscosity, and angles of impact gave droplet shape. Rarely was it possible actually to match theory with practice but theory-guided investigation and warranted interpretation of findings.

Second, Kirk was acutely aware how far "police science" differed from ordinary research. In police science, qualitative analysis of mixed samples was more important than quantitative analysis of pure extractions (58). Remarkably, he continued to combine pure and applied research, publishing in short order single-authored books on *Quantitative Ultramicroanalysis* (1950) and *Crime Investigation: Physical Evidence and the Police Laboratory* (1953) (47,59). The distinction remains important; the NAS/NRC Committee worried that forensic sciences had become too isolated from conventional scientific institutions.

Third, Kirk addressed metascientific issues regularly and well. He went beyond the distinctiveness-of-forensics issue toward a reconceptualization of science-society relations. His first focused treatment was "Standardization of Criminological Nomenclature" (1947) (60). Others I shall discuss are "Progress in Criminal Investigation" (1954), published in the *Annals of the American Academy of Political and Social Science* (61) and four papers from 1963-64: the well-known "Ontogeny of Criminalistics" (62), as well as "Criminalistics" (published in *Science*) (63), "The Hybridization of Science" (64), and "Evidence Evaluation and Problems in General Criminalistics" (with

Charles Kingston) (65). His books too address such issues: *Crime Investigation* (1953) and *The Crime Laboratory* (with Lowell Bradford) (1965).

Initially Kirk's chief concern was to bring order to the forensic enterprise. Already in 1954 he was worrying about the severing of mere technique from "fundamental philosophy and balanced understanding" (61). That frustration persisted a decade later, in the famous "Ontogeny" paper. Kirk names no names, but complains of "great divergence of philosophy . . . we often travel separate roads; the goal is not always clearly recognized." He asks "Where is criminalistics, forensic science, or whatever it may be called, going?" There is need to "define a goal, so that we may all talk about the same thing and move in similar directions" (62)

Rejecting general versions of scientific method, Kirk declared that the essence of forensic science, "in brief but truly fundamental terms," was "individuation." While Gross and Locard had perhaps implied that, Kirk made it a theorem ("A thing can be identical only to itself . . . since all objects in the universe are unique"), and then a justification. Were this "not true, there could be no identification in the sense used by the criminalist" (62).

Of course, nothing links these statements, or guarantees an ability to trace the singular, and Kirk's example of uniqueness—hydrogen atoms, utterly alike, differing only in place—was hardly helpful (62,63). A premiere ultramicroanalyst like himself might follow many tiny traces, yet in large domains of nature, like radioactive decay, individuation had already been given up: regularity was stochastic. Hence the "real aim" of criminalistics could not be to achieve absolute individuation, but "to approach . . . as closely as the present state of science allows" (62).

But what was "the sense used by the criminalist"? Recent critics of individualization claims (e.g., Saks and Koehler; Cole) have focused on particular techniques, like fingerprinting (66,67). They have, in various ways, challenged the view that probability of error can be reduced to zero. Insisting that "all laboratory

findings are related to a probability,” Kirk anticipated them. “The Problem of Identity,” the first chapter of *Crime Investigation*, was followed by a 20-page chapter on “The Relation of Probability to Physical Evidence,” which included an introduction to the statistics of variability (59, p6, 20-37).

In 1953 Kirk saw no practical problem in the failure of law enforcement professionals to admit fingerprint identification as probabilistic: the likelihood of duplication was “so minute as to be negligible”; “the almost universal modern use of fingerprints . . . testifie[d] to their reliability and utility”(59, p13-14). A decade later, however, in asserting the *scientific* status of forensics, he acknowledged inadequate mathematization as a serious general issue. “Until this area of science . . . is reduced to reasonable mathematical exactness, the administration of justice will be correspondingly hampered,” he noted in “Criminalistics,” published in April 1963, adding that the effect of this inadequacy “on the accused person, is sometimes tragic”—possibly he was thinking of Dr. Sam Sheppard (63).

But his main treatment came in “Evidence Evaluation,” a think-piece coauthored with his student Charles R. Kingston given at the American Academy of Forensic Sciences in February 1964 (65). In “Criminalistics” (63), Kirk had expressed hope for the future; here he condemned the dismal present. He savages colleagues, representing the “so-called forensic sciences” as “mostly subjective” and “arbitrary,” and they and he as “delinquent” and “amateurish.” Evidence interpretation was “deficient in mathematical exactness and philosophical understanding,” he asserted. “At this time, no actual mathematics of criminalistics even exists.” In claiming scientific status, “we must be embarrassed, for no science is without some mathematical background.” While the chief problem was accurate reference data, that is, the populations that were the denominators in calculations of probability, there were others: the quantification of pattern comparison methods (e.g., hair comparison) and the factoring in of contingent environmental effects on evidence. Uncritical assumptions of the independence of variables were another problem,

though determining degrees of linkage might be difficult (65,68).

These were symptoms of a pathology of professionalization and disciplinarity—of criminalists’ ignorance of statistics and statisticians’ disinterest in criminalists’ problems. Re-educating working criminalists, who were “almost invariably poorly trained in mathematical areas,” would be futile, they thought, but statisticians, who had tended to privilege general characteristics of populations over particulars, might be converted. They would then bring skills of experimental design to the construction of reference data sets. The new computers would crunch the numbers to solve “our unique problems.” Criminalists had only to collect the data (65).

During these years Kirk saw himself not only as giving criminalistics a scientific pedigree but giving it the best one. What might seem merely an amalgam of useful techniques actually epitomized rational decision-making. “Criminalistics,” he told the broad readership of *Science*, the flagship journal of the American Association for the Advancement of Science, was “A New Discipline to Meet an Old Need.” Renarrating the history of science, Kirk told of ancient holism giving way to experiment and disciplinarity (“Archimedes, Copernicus, Galileo, Leuwenhoek, Newton, and Lavoisier”), which had in turn given way to a revolution of application, or “reversed” disciplinarity. The results were evident in modern medicine and in engineering, in atomic energy and space travel. But the need for trustworthy crime detection was greater (63).

In his October address on “Hybridization of Science,” given to the Association of Official Agricultural Chemists, Kirk was even bolder. Like criminalists, agricultural chemists creatively adapted techniques to a range of public problems. Elitists might impugn such work as the “hybridization, mongrelization, or even degradation of science,” but it was they, locked into narrow disciplines, who were doing “unproductive,” even “possibly corrupted” science. Not only did their self-indulgence threaten public support, it ignored the public’s demand for integration and meaning. “Our real and basic interest lies in the realm of human

values,” Kirk reflected. Specialization was “a futile exercise of pure intellect, of interest only to a race of intellectual robots” (64).

The slapdown comments betray a defensiveness: Kirk justifying his own radical path from a prestigious science into a mode of professional existence that he needed to invent, not merely justify. For it would be wrong to read Kirk’s career as the inevitable path toward the fact-based forensic deliverance we now celebrate. Hybridization was one thing; what hybrid to make quite another. Notably, to the agricultural chemists, he identified himself not as a professor but as a consultant: “Paul L. Kirk Ph.D. and Associates, Berkeley, Calif.,” specializing in “Technical Investigations, Civil and Criminal” (64).

If Kirk was aggressive toward science, he was usually supplicatory toward policing—after all, his major market. His focus in the late 1940s was the development of cheap and simple techniques for small departments. But he was marketing skill too. With these, a single master of methods, the general criminalist (analogue of a local GP, the general medical practitioner) would reconstruct each unique crime, while avoiding costly routine analyses of crime scene residues (52).

Waste, based in ignorance, had been the thrust of his 1954 assessment of the “Progress of Criminal Investigation.” Police laboratories were staffed by persons ignorant of the workings of instruments and the interpretation of results. Or municipalities were seduced by gadgetry: one electron microscope or two? No one in the force in question knew how to operate such a device, nor was there a use for them (61,59, v-vi p3-4,9). By the early 1960s, however, Kirk himself was being seduced by the magnificent precision of costly new methods—electron probes, Nuclear Magnetic Resonance, but especially neutron activation analysis. A nuclear reactor would supply ample neutrons. It would measure the large trace element variations in human hairs; would be what DNA is now, the godsend of identity (63,64).

Throughout, however, Kirk was seeking a seat at the detectives’ table, ideally at its head. For forensics was

less about supplying facts to detection than about detection itself. His rejoinder to worries about the probabilistic status of particular types of evidence was that “a single piece of evidence” was “rarely sufficient . . . to establish proof of guilt or innocence.” Instead, the assembly of multiple forms of convergent physical evidence constituted individuation, the reconstructing of a contingent unique event. Mostly he was politic about this ambition, yet his representations of the criminalist as servant of law enforcement come with reminders of how foolish it would be to ignore that service. For physical evidence did not merely complement human testimony, it superseded it: it did “not forget . . . [was] not confused by the excitement of the moment. . . cannot be wrong; . . . cannot perjure itself.” We could, however, fail to gather it and fail to interpret it properly. Hence “to this study must be brought all the resources of science and human understanding if the message is to be clear, complete, and unequivocal” (59, p4-6).

A “clear” and “complete” message based in “unequivocal” knowledge would seem to imply that an advocacy trial before a lay jury was irrational as well as superfluous. In *Crime Investigation*, Kirk had railed against defense advocacy. Cross-examination did “not serve the ends of justice”; it existed only to “cast doubt.” Yet there need be “no . . . loophole . . . [for] a clever attorney” if a well-prepared forensic case was well-presented, by reciting each test, “the reasons for performing it, . . . the results obtained, . . . interpretations . . . and the objections to alternative interpretations” (59, p517-520). In *The Crime Laboratory* (1965) he and Bradford overlooked defense advocacy altogether. The criminalist would coordinate directly with the state’s attorney in making a case (38, p110-114).

Ideally then, accurate assessment of physical evidence would be so well integrated into the iterative hypothesis-testing of detection that no charge would ever be made without sound forensics. Justice itself was at stake, as in Kirk’s most famous engagement on behalf of Dr. Sam Sheppard, convicted for his wife’s 1954 murder. A competent forensic reconstruction by the Cleveland Police would have pointed to someone other than Sheppard (44).

Here too Kirk's perspective anticipates recent faith in forensic deliverance. A common plot line has eccentric forensic detectives displaying their creativity in correcting grumpy gumshoes. The view of forensic evidence as chief guarantor not only of prosecution, but of conviction, effectively makes the expert the finder of fact, even if a "moral" determination is made elsewhere (69,70). Hence, authors like Kirk operate *as if* they are in a continental legal system, rather than simply adding incidental bits of technical information to the process of moving from suspicion to conviction. And with no discussion of how the maxim will be implemented, Kirk and Bradford remind readers to keep in mind "*that all criminalistics examinations are made as much in behalf of the defendant or suspect as for the enforcement agency*" (38, pvii, 6,91-93).

These issues frame Kirk's confrontation, in "Evidence Evaluation," with the unseemly topic of expert disagreement. That issue rarely surfaces in the works I have examined. When it does, it is usually in the guise of casting stones at incompetent or mendacious "pseudo-experts" testifying against the author. Kirk and Kingston, on the other hand, argued that in the current state of criminalistics, subjectivity was inescapable. It was only a "practical problem," however, when "another expert disagrees." There being "no higher court of appeal," the result was "an impasse." Anticipating later forensic reformers, they urged colleagues to "abandon the idea of absolute certainty."; Echoing Peterson, they listed common bases of error: "inadequate or atypical experience, . . . lack of understanding of fundamentals, or even a mental bias of which its possessor may be totally unaware" (65). Mainly, however, expert-disagreement was another reflection of the appalling state of mathematics.

Though Kirk's 1963 "Ontogeny" essay is usually cited for its individuation assertion, its broader concern was the role and authority of the criminalist. Kirk had been considering the question: "So you're a criminalist, What's that?" since the 1947 "Nomenclature" essay (60). There he had rejected "criminologist" (too broad); "forensic" or "legal" (vacuous in the context); "police chemist," "police scientist" or "police technician" (misleading on substantive grounds and in

implying dependent status). Only the European "criminalist" and "criminalistics" remained. In "Ontogeny," he declared that criminalistics had a unique nobility, combining "the responsibility of medicine, the intricacy of the law, and the universality of science." He represented it simultaneously as science, profession, and skilled occupation. "Occupation" signified work done well by a person trained in the principles of the techniques being used. "Science," understood as growing analytical prowess (e.g., neutron activation), was transgressive. Yet "higher penalties for error" gave criminalistics a unique gravity and made "profession," guarantor of the ethical, its primary identity (62). Returning to the issue in 1965 Kirk and Bradford represented forensics as a "ministry" of service, with "authority" granted by the "citizenry" (38, pvii, viii). Remarkably, status issues—proper "respect" and "pride"—in Kirk's terms, remain an explicit issue in contemporary the nature-of-forensic-science discussions.

FACT OR NARRATIVE? WHAT IS FORENSIC SCIENTIFICITY AND WHERE TO FIND IT?

Compared to Kirk's day, contemporary criminalistics seems a success story. Why then the persistent anxieties and continuing ambiguities? Three factors are important I think, all having to do with the failure to bring the history of the interplay of roles, institutions, methods, and expectations fully into the discussion. These are:

- 1) Failure to acknowledge the full impact of the workplace—the institutions of jurisprudence—on the work.
- 2) Failure to acknowledge the depth of differences between the mission/context of science and that of jurisprudence.
- 3) Failure to extract from the philosophy of science the analyses most apposite to the nature and legitimacy of forensic science

Institutions first. "Forensics" derives from matters of the forum. It would be naïve to think that the rules of a forum had no effect on statements made within it—in making a point in debate (still one meaning of "forensic"), one is not

making a Solomonic judgment. Hence, while there is ample precedent for the recent call of Biedermann et al. for standardized interpretation of forensic results, the issue is less forensic practice than the role of forensic authority within a legal system (71). Eighteenth-century medical jurisprudence textbooks from inquisitorial legal systems were compendia of such rules. Their concern was to stabilize jurisprudence. But in Anglo-American adversary systems, the premium was on destabilizing, exploration of the exceptional. Most of the many American medicolegal journals launched in the postwar years were for personal injury law, for novel ways to destabilize (72). Often, one won by overclaiming. Given that background, the Kirk-Bradford representation of the criminalist as minister to a congregation of humanity is strikingly discordant.

As I have noted, it was not in the interest of Anglo-American forensic writers to advertise differences between the continental expert's role as finder of fact, and their role as instrument of persuasion (a rare and ambiguous exception was Stanford Chaillé, addressing the *Origin and Progress of Medical Jurisprudence, 1776-1876* (27,73)). No one was asking them to reinvent jurisprudential institutions; their status was beneath that of lawyers, and their professional ethics put client over public interest. Moreover, though the texts emphasize criminal proceedings, experts' skills were valuable in civil actions and consulting (74,75)

More subtle than the neglect of legal context is the slippery slope between metaphysics and public decision-making that comes with bringing natural philosophy (Newton and Galileo) into the courtroom as a way to secure philosophical dignity to forensic findings. On one level Kirk had clearly distinguished the "major goal" of most other sciences—"equations, formulas, and generalizations summarizing the *normal behavior* of any system" (befitting a D-N model) from the forensic focus on "the unlikely and the unusual" (63), ital. mine. His specialty, microanalysis, did share with

criminalistics this pursuit of the unusual, yet as the hydrogen-atom analogy indicates, he had not reckoned fully with the difference between a metaphysical debate, where there are always plausible avenues of conjecture about ulterior levels of distinction, and a forensic context. Both involved imaginative exploration, yet consequences differed: conceivable possibility versus practical decision-making.

Kirk had avoided that confrontation by moving between the individuation of things and of events. As chemist, he was guarantor of the single analytical result. However difficult the probability calculation needed to determine its actual probative value, the problem was plain enough. As detective, Kirk followed Gross, who represented detection not as simple induction, but as a "campaign," an iterative achievement that required an imaginative transcendence of single pieces of evidence in seeking a compelling narrative. Both were in a loose sense Bayesians, though Bayesianism had yet to enter forensics in a formal way (38, p5, 76).

As the NAS/NRC committee recognized, these two forms of individuation are at the heart of the particular philosophical problems that arose in forensic science. With regard to the former, it worried about false individuation. "Cognitive biases" in assessing data were "common features of decision making," reflecting "the common desire to please" or to avoid "cognitive dissonance." With regard to reconstructing unique events, it worried about "anchoring," becoming so "wedded to a preliminary conclusion, . . . that it becomes difficult to accept new information fairly" (77).

Correcting the former by insulating fact-making may hamper the critical thinking needed to correct the latter. In reviewing proposals for the appropriate relation of fact-making to evaluation, William C. Thompson proposes an organizational solution for trading off the objectivity that comes from isolation from context with the utility that comes from judging evidence in context (78). But the problem should be recognized as a philosophical one too.

It is here that Max Houck's invitation to focus individuation on narrative- rather than fact-making becomes important as a continuation of Kirk's integrative forensics. Houck argues that forensic science is fundamentally an historical science. Its familiar elements, like the Locardian exchange principle or probability determinations, are tools for reconstructing a contingent matter of fact, the crime (5).

Some past philosopher-scientists have likewise recognized features of a unique class of historical sciences. In 1840, the Cambridge polymath William Whewell coined the term "paleoetiological" for sciences which inferred past causes from residual evidence (79). The new science of stratigraphic geology was his (and Houck's) exemplar. Yet while classifying a rock formation by index fossils might be straightforward—akin to identifying a tire track—testing hypotheses of origins was trickier. Here, the work of the later geologist T.C. Chamberlin has been seminal. Recognizing how easily one might accentuate confirmatory evidence while ignoring anomalies, Chamberlin, writing in the 1890s, proposed the "method of multiple working hypotheses." To avoid cherry-picking, one consciously opposed, to the probability-reducing agenda of individuation, its opposite: the consideration of alternative narratives, letting each hypothesis guide inquiry, and continually reassessing its compatibility with accumulating evidence. The paper has regularly been republished and revisited by workers in other disciplines, but it seems particularly apt to forensics (80,81). Whewell had also, under the phrase "consilience of inductions," recognized, in general terms, the value of multiplying probabilities: convergence of many lines of inquiry gives stronger reasons for belief, but *only* if the lines really are independent (79).

A focus on the history of other historical sciences—epidemiology, for example—would bring to the fore other anticipations of reasoning modes conspicuous in forensic contexts. These would take us further from the D-N model. Kirk's substitution of "criminalistics" for "forensic science" was one step; in *The Crime Laboratory* he and Bradford went so far as to offer a new, neutral term, "criminalistics

operations," for any "scientific support" of law enforcement (60,82).

If we follow Kirk and Houck, the recondite features of forensics will be in techniques not reasonings. No other members of the jury of which I was a member some years ago had, I presume, read Whewell or Chamberlin, yet we did what they suggest: we seriously considered multiple hypotheses and were swayed by converging lines of evidence. Other jury members were better at that than I, who had read them. We should not be surprised: sociologists of trust have noted that a canny assessment of narratives is the key-stone of ordinary decision-making (83).

Thus, the modest proposal. If forensics is an historical science, why not call it "history"? Houck's view of archeology as the closest analogue to forensic science privileges commonalities in evidence—material traces—and methods, like the law of superposition, over goals (5). Yet, as is conspicuous in geology, the goal even of historical sciences may be a general truth rather than reconstruction of a contingent event, the goal of the detective and in most cases, the historian. (Archeology is ambiguous in this respect—reconstruction of a site may be a means to address general issues of social science or an end in itself, in which case archeology is a mode of history.)

More conspicuous than reliance on types of evidence (which, in the case of document examination, significantly overlap) has been the differing cultures of narration of historians and forensic scientists. On one side, fact is a check to narrative, which will come as elimination—"it couldn't have happened otherwise." The other, accepting the inescapability of story-making, seeks the *likeliest story* of how it might have happened. Even preferred terms differ—the criminalist finds "*traces*"; the historian chooses "*sources*" and sees confirmation not as bias but as coherence. Both approaches operate in detection—the *good* investigator, Hans Gross observed, had "a slight leaning toward the fictitious" (84).

Like forensic scientists, historians have agonized over status (85). In the 19th century securing a place among

the disciplines—known in Germany as “sciences” (*wissenschaften*)—required demarcating domain and methods. While claim to a scientific status remains important to historians’ professional status in many parts of the world, in the United States the field is usually understood as a critical humanistic discipline. Goals do not differ: one still seeks to draw conclusions from multiple modes of evidence. Nor has the centrality of source criticism changed. Inference must be guided by knowledge of the creation and possible alteration of types of evidence, which may include red herrings dangled to deceive or distort. But reward structures have changed. Thus, (as for Peterson) authority often manifests as skepticism: we admire those who show us why we should doubt sources. Sometimes there are moral reasons for under-claiming. Aware how easy will be the construction of self-serving narratives, historians may prefer to deconstruct or to warrant their own products as single slices through the stuff of the past.

As to the modest proposal, however much I like the term “crime historian,” forensic scientists will not and should not reinvent themselves as forensic story-tellers. Rather, the value of the excursus is recognition of a dialectic Gross explores, of an essential interplay between fact-making and story-making that necessarily takes place within a distinct institutional framework.

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