Toward a Sociology of *Forensic* Knowledge? A (Supplementary) Response to Cole

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> [The "sociology of *scientific* knowledge" (SSK)] set out . . . to develop an anti-individualistic and anti-empiricist framework for the sociology of knowledge in which "social factors" counted not as contaminants but as constitutive of the very idea of scientific knowledge. . . . SSK developed in opposition to philosophical rationalism, foundationalism, essentialism, and, to a lesser extent, realism. The resources of sociology [were] necessary to understand . . . how it was that scientists came to recognize something as a "fact" or as "evidence" . . . .[1]

I. INTRODUCTION

It is a privilege to be invited to contribute to this symposium, alongside so many leading evidence scholars, honoring Professor Michael Risinger. And it is significant that the symposium opened with Professor Simon Cole's reflections on forensic science, insofar as he is not a lawyer or evidence professor, but a science-studies scholar. Ten years ago, Professor Risinger

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[1] Steven Shapin, *Here and Everywhere: Sociology of Scientific Knowledge*, 21 ANN. REV. SOC. 289, 297 (1995).

came to Villanova to participate in a symposium about my book, *No Magic Wand* (co-authored with L.H. LaRue[2]), which (i) argued that *Daubert*[3] idealized science, and (ii) recommended an engagement by those in law with the discipline of science studies. As to the latter, Risinger (after extensively familiarizing himself with science studies and its "wars" with conventional philosophers of science) was doubtful:

> It seems to me that science studies in its various manifestations have suffered from a self-inflicted wound. It has made itself easy to associate with the worst excesses of post-modernism as post-modernism played out across various academic disciplines. In the main, perhaps science studies practitioners did not go so far in that direction as their critics later charged, at least not consistently. But if they were not card carrying post-modernists, they were at least sometimes opportunistic fellow travelers, and there was still plenty of excessive rhetoric to choose from. . . .[4]

The relativism associated with science studies, in particular, was too excessive for law—courts did not want or need to hear an account of the instability of scientific knowledge. However, Risinger noticed some common ground between science studies and its critics, namely that few scholars in the "science wars" were willing to take either of the exaggerated positions—strong relativism or complete idealization—in the debate over the nature of science.[5] By 2007, few critics of science studies idealized science, and few science-studies scholars denied scientific progress.

Indeed, on this twenty-fifth anniversary of *Daubert*, if I may generalize, I would argue that a sort of skepticism (due to the forensic science debacle) has, over those twenty-five years, slowly replaced the idealism that LaRue and I identified; and over the same period, science studies has been on a trend from relativism to a sort of idealism concerning science, focusing nowadays on reliability in science![6] In law, we have moved from Judge Kozinski's "twin errors" (on remand in *Daubert*)—roughly, (i) that money does not corrupt pharmaceutical science, and (ii) that forensic science requires no particular scrutiny[7]—to Judge Edwards' skepticism:

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[2] *See* DAVID S. CAUDILL & LEWIS H. LARUE, NO MAGIC WAND: THE IDEALIZATION OF SCIENCE IN LAW (2006).

[3] Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579 (1993).

[4] D. Michael Risinger, *The Irrelevance, and Central Relevance, of the Boundary Between Science and Non-Science in the Evaluation of Expert Witness Reliability*, 52 VILL. L. REV. 679, 682 (2007).

[5] *See id.* at 684.

[6] See discussion *infra* notes 46–66 and accompanying text.

[7] *See* David S. Caudill, *Expertise, Lab Lit, and the Fantasy of Science Free from*

> I started the NAS project with no skepticism regarding the forensic science community. Rather, I assumed, as I suspect many of my judicial colleagues do, that the forensic disciplines are well grounded in scientific methodology and that crime laboratories follow proven practices that ensure the validity and reliability of forensic evidence offered in court. I was surprisingly mistaken in what I assumed.[8]

And in science studies, we have moved from a singular focus on the determinative social aspects of the scientific enterprise—its institutions, authority structures, influential theories, methodological preferences, specialized languages (and governing metaphors), experimental conventions (limited by measurement and instrument technologies), consensus-building strategies, and even negotiated scientific papers—to make room for Nature (the so-called "naturalist turn" in science studies; in Bruno Latour's words: "Yes, the scientific facts are indeed constructed but they cannot be reduced to the social dimension because this dimension is populated by objects mobilized to construct it").[9] So it is not a surprise to include Professor Simon Cole in a law and expertise symposium honoring an evidence scholar.

Just over thirty years ago, Harry Collins, discussing the "new" sociology of scientific knowledge, expressed his disappointment that, "though the field has only begun to fulfill its potential, disagreements are now taking up more space than substantive contributions."[10] Indeed, as

Economics, 33 CARDOZO L. REV. 2471, 2481–82 (2012) (discussing *Daubert* on remand). Science-studies literature, of which Judge Kozinski seemed unaware, includes a critique of funding bias. *See, e.g.*, Sheldon Krimsky, *Do Financial Conflicts of Interest Bias Research? An Inquiry into the "Funding Effect" Hypothesis*, 38 SCI., TECH. & HUM. VALUES 566, 569–70 (2013). As to legal critiques, *see* THOMAS O. MCGARITY & WENDY E. WAGNER, BENDING SCIENCE: HOW SPECIAL INTERESTS CORRUPT PUBLIC HEALTH RESEARCH 4–6 (2008).

[8] Harry T. Edwards, Senior Circuit Judge and C.J. Emeritus, U.S. Court of Appeals for the D.C. Circuit, Co-Chair, Comm. on Identifying the Needs of the Forensic Sci. Cmty. the Nat'l Acad. of Sci., *The National Academy of Sciences Report on Forensic Sciences: What it Means for the Bench and Bar*, Presentation at the Superior Court of the District of Columbia Conference on the Role of the Court in an Age of Developing Sci. & Tech. (May 6, 2010), https://www.cadc.uscourts.gov/internet/home.nsf/AttachmentsByTitle/NAS+Report+on+Forensic+Science/$FILE/Edwards,+The+NAS+Report+on+Forensic+Science.pdf.

[9] BRUNO LATOUR, WE HAVE NEVER BEEN MODERN 6 (Catherine Porter trans., Harvard Uni. Press 1993) (1991).

[10] H.M. Collins, *The Sociology of Scientific Knowledge: Studies of Contemporary Science*, 9 ANN. REV. SOC. 265, 265 (1983). Collins distinguishes the "largely American" sociology of science from the "largely British" sociology of scientific knowledge. In the former, the sociologist does not focus on the content of science, which is presumed to be determined by Nature and not by people, but on the "normative and other institutional arrangements that enable science . . . to exist and function efficiently." *See id*. at 266. Hence it is often called a "sociology of error," insofar as human influences are not properly part of scientific knowledge. *See* Shapin, *supra* note 1, at 291 (citing JOSEPH BEN-DAVID, THE SCIENTIST'S ROLE IN SOCIETY: A COMPARATIVE STUDY 11–13 (1971)) ("[T]here might be a

suggested above, the effort "to explain the content of scientific knowledge as far as possible in social terms"[11] invited disagreement, as did "explanations of the outcomes of [scientific controversies] . . . by reference to wider social and political factors."[12] While the social *aspects* of science are hardly in doubt (there are scientific *communities*, experimental *conventions*, identifiable *cultural* values—e.g., honesty—and so forth), SSK was a challenge to traditional notions that the *content* of science should *not* be affected by society, and, consequently, that scientific controversies should be settled by Nature. Reflecting that ideal, the mid-twentieth-century sociology of science associated with Robert K. Merton, now called the "first wave" of science studies, assumed that "sociological accounting had to stop at the door of scientific method and scientific knowledge."[13] SSK, having opened that door, is often now referred to as the "second wave" of science studies; it is variously characterized as (i) breaking down the distinction between science and society, (ii) highlighting the constitutive, and not merely influential, role of "the social" in the production of scientific knowledge,[14] and (iii) developing a less idealized view of science and scientists—the scientific enterprise is part of, and not above, culture.

A feature of the latter development—the development of a more modest view of science—was the proposal that ordinary citizens could and should play a role in scientific decision-making For example, an elite scientist helping a community suffering from an environmental crisis may not know as much about the problem (and workable solutions) as a local farmer.[15] It is this phenomenon, this proposal to increase citizen participation in science, which inspired the so-called "third wave" of science studies. Indeed, the third wave was a reaction *against* the notion of "citizen scientists":

legitimate sociological understanding of scientific error . . . but . . . there could be no such thing as a sociology of authentically scientific knowledge.").

[11] Collins, *supra* note 10, at 272.

[12] Collins, *supra* note 10, at 275.

[13] *See* Shapin, *supra* note 1, at 294–95.

[14] *See generally id.* at 301 ("Signaling the sensibility that sought to remove 'the social' from its status as a 'factor,' the second (1986) edition of Latour & Woolgar's *Laboratory Life* deleted the word 'social' from its original (1979) subtitle: *The Social Construction of Scientific Facts*."). That is, the "social" is not *merely* a "factor" in the construction of scientific knowledge, but is rather something more constitutive—for example, the social is co-productive alongside Nature.

[15] *See generally* Brian Wynne, *Sheep Farming after Chernobyl: A Case Study in Communicating Scientific Information*, 31 ENV'T: SCI. & POL. FOR SUSTAINABLE DEV. 10, 34–35 (1989); Brian Wynne, *May the Sheep Safely Graze? A Reflexive View of the Expert–Lay Knowledge Divide*, *in* RISK, ENVIRONMENT & MODERNITY: TOWARDS A NEW ECOLOGY 44–83 (Scott M. Lash, Bronislaw Szerszynski & Brian Wynne eds., 1996); Brian Wynne, *Misunderstood Misunderstanding: Social Identities and Public Uptake of Science*, *in* MISUNDERSTANDING SCIENCE? THE PUBLIC RECONSTRUCTION OF SCIENCE & TECHNOLOGY 19–46 (Alan Irwin & Brian Wynne eds., 1996).

> Though science studies has [shown] that the basis of technical decision-making can and should be widened beyond the core of certified experts, it has failed to [answer the question:] "How far should participation in technical decision-making extend?" In other words, science studies has shown that there is more to scientific and technical expertise than is encompassed in the work of formally accredited scientists and technologists, but it has not told us how much more.[16]

Of particular relevance to the ongoing criticism of forensic science, the third wave project is focused on who should participate in scientific decision-making, which becomes a question of who is a credible expert. Briefly, in third wave terminology, experts include (i) those who are trained and credentialed in the consensus science of the relevant field, as well as (ii) those who have sufficient experience (even without formal training) in the field to interact productively with trained experts and thereby contribute to the task at hand. While an "ordinary" citizen has no business influencing scientific decisions, an experienced farmer (and therefore not an "ordinary" citizen with respect to farming) without scientific training can help a trained scientist (with no farming experience) understand and solve a problem. Anticipating my discussion of arson investigation below, for example, a former firefighter might be of help to a fire chemist. The key to expertise is not, therefore, *solely* training and credentials—it can also include expertise based on experience, a notion that is consistent with *Kumho Tire*'s clarifications of the *Daubert* standard for admissibility of expert testimony.[17]

I mention the third wave in science studies because I think it offers a minor but helpful re-orientation, or supplement, to Professor Simon Cole's Article (in this symposium) on the regulation of forensic science, which includes a brief analysis of scientific cultures and the supposed need for forensic scientists to join one.[18] My own orientation in this Article is not toward SSK, the sociology of *scientific* knowledge, but rather, as suggested

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[16] *See* H.M. Collins & Robert Evans, *The Third Wave of Science Studies: Studies of Expertise and Experience*, 32 SOC. STUD. SCI. 235, 237 (2002).

[17] *See* Daubert v. Merrell Dow Pharm., Inc., 509 U.S. 579, 592–93 (1993); Kumho Tire Co. v. Carmichael, 526 U.S. 137, 147 (1999) (stating although *Daubert* referred to scientific knowledge as the basis of admissible expert testimony, Federal Rule of Evidence 702 makes no relevant distinction between "scientific" knowledge and "technical" or "other specialized" knowledge).

[18] *See* Simon A. Cole, *Who Will Regulate American Forensic Science?*, 48 SETON HALL L. REV. 563, 567, 569–70 (2018) ("Most forensic laboratories are controlled by law enforcement agencies. This arguably creates potential pro-government bias and interferes with forensic scientists' allegiance to 'science.' . . . [Moreover,] many of the self-regulatory features thought to apply to science seem less applicable to forensic science. . . . This situation means that the regulation of forensic science cannot come about merely through what is often called 'scientific culture.'").

in my subtitle, toward a sociology of *forensic* knowledge—where such knowledge may not be "scientific" in the conventional sense of that term. The pejorative sense of the descriptor "non-scientific culture" becomes less relevant in my analysis, because reliable, admissible expertise can obviously be offered by non-scientists. To the extent that a field of consensus science is linked to a non-scientist's expertise, however, the expert's testimony just as obviously should be consistent with that science. In this supplementary response, I argue for a relatively modest extension of Cole's analysis, which draws extensively on second wave SSK, into a third wave focus on "expertise" rather than "science" as the most helpful category in assessing the shortcomings and promise of forensic science.

In Part II below, I summarize Cole's recent views on forensic science, and his proposal that a focus on *standards* could help solve the problems that plague forensic expertise. In Part III, I imply that Cole, employing the discourse of second wave science studies, recasts the problems of forensic science in terms of third wave science studies. In short, the questions about science and non-science (or scientific and unscientific cultures) become questions of expertise and lack-of-expertise. In Part IV, to illustrate my re-orientation, I use the example (mentioned by Cole in this symposium) of the recent critique of, and reform efforts with respect to, arson expertise. The problem in that field is not the lack of scientific training on the part of arson investigators and experts, but rather the gap between the science of fire chemistry and the "mythologies" adopted by poorly trained fire investigators. In Part V, I conclude that Cole's recommendation of standards would be enhanced by third wave categories of "expertise," as it helps explain why some technical experts do not need to become scientists, even though they do need to be practicing within a framework of scientific consensus.

II. FORENSIC CULTURE

> [M]ost forensic disciplines weren't invented in labs, then subjected to peer review in scientific journals. Instead, most were invented by people in law enforcement, not in the quest for knowledge, but as an aide to help them solve crimes. Scientists within the same field have strong incentives to poke holes in others' theories, [but this] isn't the case in forensics. A fingerprint analyst testifying for the defense might disagree with a fingerprint analyst for the prosecution, but he isn't going to call into question the premises on which the entire field of fingerprint analysis is based. He'd be undermining his own legitimacy.[19]

[19] Radley Balko, *A Brief History of Forensics*, WASH. POST (Apr. 21, 2015), https://www.washingtonpost.com/news/the-watch/wp/2015/04/21/a-brief-history-of-forensi

In his insightful contribution to this symposium, entitled *Who Will Regulate American Forensic Science?*,[20] Cole (after identifying the perceived shortcomings of forensic science, the past efforts to reform forensic science, and the current opportunity for regulation) highlights the differences between scientific culture and *forensic* science culture. The latter not only lacks "many of the self-regulatory features thought to apply to science," but operates under an economy that is not based on publication and reputation.[21] There seems to be little likelihood that the forensic science community will adopt a "scientific culture" any time soon, as recommended in the U.S. National Academy of Sciences Report on forensic science ("NAS Report").[22] Cole suggests that standard-setting may be "a plausible means of regulating forensic science," but notes that those standards should probably not be set by "members of the same guild group with group commitment to the general validity of the enterprise as practiced by the best practitioners," especially in the absence of input from the public, from representatives of consumers (courts and criminal defendants), and from "neutral scientists who were committed to the notion of validity generally."[23] Finally, even if a standardization process succeeds, "implementation is an open question."[24]

Elsewhere, Cole has written extensively on, and criticized as incomplete and not very helpful, the recommendation that forensic scientists adopt a scientific culture.[25] In 2010, Cole raised the question of why there

cs/?utm_term=.bf39e5c95377.

[20] *See* Cole, *supra* note 18, at 563. Cole identifies as shortcomings, in forensic science, its lack of standards, inadequate resources, logical fallacies, over-claiming testimony, lack of validation, confirmation bias, control by law enforcement agencies, insufficient self-regulation, insufficient training, insufficient quality controls, lack of intellectual curiosity, insufficient commitment to open inquiry, and over-defensiveness to criticism. *Id.* at 566-67.

[21] *See* Cole, *supra* note 18, at 569. Self-regulation, courts, and government regulations have not worked to overcome these deficiencies. *See id.* at 570.

[22] NAT'L RES. COUNCIL OF THE NAT'L ACAD. OF SCIENCES, STRENGTHENING FORENSIC SCIENCE IN THE UNITED STATES: A PATH FORWARD 125 (2009), https://www.ncjrs.gov/pdffiles1/nij/grants/228091.pdf [hereinafter NAS REPORT].

[23] *See* Cole, *supra* note 18, at 574, 577.

[24] *See* Cole, *supra* note 18, at 580. *See also id.* (alteration in original) ("Sociologists note that '[s]tandards often require an auxiliary system that provides internal or external incentives, audits, and certification. Standards may fail implementation for countless reasons, including lack of knowledge, lack of compliance, immediate conversion of standards, resistance, adaptation, or usurpation. Very few standards work as intended by the designers of standards because they are tinkered with, whether slightly or fundamentally.'" (quoting Stefan Timmermans & Steven Epstein, *A World of Standards But Not a Standard World: Toward a Sociology of Standards and Standardization*, 36 ANN. REV. SOC. 69, 81 (2010))).

[25] *See* Simon A. Cole, *Forensic Culture As Epistemic Culture: The Sociology of Forensic Science*, 44 STUDIES IN HIST. & PHIL. BIOLOGICAL & BIOMEDICAL SCIENCES 36, 37 (2013);

needed to be an intervention and critique by the National Academy of Sciences, and then explained:

> The NAS Report does not focus very much on the "why" questions, but, to the extent that it does, its discussion is centered [on] the "culture of science." The Report describes the "culture of science" as an important missing ingredient in at least parts of forensic science. It states that "some . . . activities" that fall under the broad rubric of "'forensic science' . . . are not developed within the culture of science." Further, it touts "scientific culture" as a potential antidote to . . . "The Problems That Plague the Forensic Science Community" [and claims that the] "forensic science disciplines will profit enormously by full adoption of this scientific culture."[26]

Validation studies have also been recommended for the forensic sciences, both in the NAS Report[27] and by Cole himself.[28] And yet, while there "are vigorous debates about what 'validation' means," it is at least a "concrete" thing

> compared to the nebulous notion of "scientific culture." What is this "scientific culture" that the NAS Committee says is both missing and needed in at least some parts of forensic science? The Report never explicitly defines scientific culture[—]a rather vague and contested term that is used to mean a variety of different things . . . . I will argue, however, that . . . it is important, and perhaps indispensable, that we can articulate precise meanings for the term, and that forensic science adopt something called "scientific culture" if any of the commonly desired responses to the NAS Report . . . are to occur.[29]

Cole goes on to criticize the "definitional confusion around" the terms scientific culture, science, and scientific method, a "confusion that [is]

Simon A. Cole, *Acculturating Forensic Science: What Is 'Scientific Culture,' and How Can Forensic Science Adopt It?*, 38 FORDHAM URBAN L.J. 435, 438–40 (2010).

[26] Cole, *Acculturating Forensic Science*, *supra* note 25, at 438 (citing NAS REPORT, *supra* note 22, at 39, 114, 125); *id.* (citing Harry T. Edwards, Senior Circuit Judge & Chief Judge Emeritus, U.S. Court of Appeals for the D.C. Circuit, Keynote Address at the Conference on Forensic Science for the 21st Century: The National Academy of Sciences Report and Beyond, Solving the Problems That Plague the Forensic Science Community (Apr. 3, 2009)).

[27] *See* NAS REPORT, *supra* note 22, at 144, 154, 161, 166, 174.

[28] *See* Simon A. Cole, *Comment on "Scientific Validation of Fingerprint Evidence Under* Daubert*,"* 7 L. PROBABILITY & RISK 119, 124 (2007).

[29] Cole, *Acculturating Forensic Science*, *supra* note 25, at 439.

exacerbated by a tendency to equate 'science' with what [Cole calls] 'discovery science.'"[30] Routine laboratory work, industrial science, engineering, and "much of medicine" is not captured by that latter, popular conception; sociologists of science, however, *have* focused on scientific practices, the mundane "work" that must be considered when evaluating the activities of forensic scientists.[31] In the end, however, it is not necessary to define "science" after *Kumho Tire*, which "relieved courts of the responsibility to decide whether various forms of expert evidence should constitute 'science' or 'non-science.'"[32] Likewise, any attempt to define the scientific method is problematic because there is no single method "employed by all areas of knowledge production that we generally call science."[33]

Cole recommends that the NAS Report's call for the adoption of "scientific culture" by forensic scientists would make more sense if the tasks or practices—the "work"—of forensic scientists were identified and integrated into any notion of the type of "scientific culture" to which they might aspire.[34] These tasks include (i) basic research (including validation experiments), (ii) evidence collection, (iii) technical management, (iv) analysis (deploying forensic techniques), and (v) interpretation.[35] The "scientific culture" that we want for these five tasks would include (i) research similar to a university or industrial research laboratory, (ii) careful and honest collectors of evidence, (iii) open-mindedness and critical thinking on the part of technical managers, (iv) meticulous adherence to protocols and documentation on the part of analysts, and (v) logical reasoning and self-restraint (to avoid exaggeration of results) in interpretation.[36] Cole's point is that an orientation to *discovery* science as the image of scientific culture is less than helpful in our evaluation of forensic science. Cole's evaluation of

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[30] Cole, *Acculturating Forensic Science*, *supra* note 25, at 444 (stating discovery science is "scientific activity designed to create new, generalizable knowledge about the natural world").

[31] Cole, *Acculturating Forensic Science*, *supra* note 25, at 445 ("[We] cannot coherently apply such notions as 'science,' 'scientific method,' and 'scientific culture' to forensic science without thinking seriously about what sort of scientific activity forensic science purports to be").

[32] Cole, *Acculturating Forensic Science*, *supra* note 25, at 446–47; refer to note 17 *supra* and accompanying text.

[33] *See id.* at 448. In defining the "scientific method . . . we run into the same problem we encountered in seeking to demarcate "science." Popular parlance may believe that there is a single unitary "scientific method" involving the same experimentation and hypothesis testing that all areas of science utilize. Philosophers of science, however, are in broad agreement that there is no single method employed by all areas of knowledge production that we generally call science. *Id.*

[34] *See* Cole, *Acculturating Forensic Science*, *supra* note 25, at 454.

[35] *See* Cole, *Acculturating Forensic Science*, *supra* note 25, at 454–56.

[36] *See* Cole, *Acculturating Forensic Science*, *supra* note 25, at 457–59.

forensic science, briefly, is that basic research (especially validation research) is lacking; evidence collection, analysis, and interpretation are often performed by the same individual; and there is little technical management by a scientist.[37]

> Understood in this light, we can see that the failure of scientific culture that the NAS Report implies cannot be understood as a unitary failure that applies equally to all task-roles in forensic science. Nor is it a failure to adhere to a single method, principle, or virtue. Instead, the implied failure of scientific culture should be understood as a much more variegated thing. For instance, the failure to validate . . . can be understood as a cultural failure of intellectual curiosity among Basic Researchers. The use of non-validated technologies can be understood as a cultural failure to engage in organized skepticism, or . . . "critical questioning." The creation of reporting regimes which mandated the reporting of conclusions in terms that were illogical and unsupported . . . can be understood as a cultural failure of epistemological modesty, a failure [to discourage] "statements that go beyond established facts." And, the defensiveness with which the forensic community reacted to the challenges posed by outsiders . . . can be understood as a cultural failure of "openness to new ideas. . . ."[38]

For Cole, the field of medicine provides a useful model, analogous to forensic science, insofar as there are biomedical researchers who never see patients, clinical physicians who do no research, and laboratory technicians who perform medical procedures.[39]

> [I]n forensic science, we would want a cadre of basic researchers, who develop and validate new methods and techniques. We would also want a much larger cadre of technicians with manual and interpretive skills. These individuals would need to know very little about the validity of the techniques that they use. . . . Mediating between these groups would be a cadre of individuals

[37] *See* Cole, *Acculturating Forensic Science*, *supra* note 25, at 459–60. *See also id.* at 461 ("An intellectually valid answer to the question "Where is the study showing this technique is valid?" requires some familiarity with scientific reasoning and probably some understanding of philosophy of science as well. Historically, Analysts had been drawn either from the ranks of police or civilian law enforcement employees. They did not have the kinds of scientific training that would allow one to function as a Basic Researcher in a university, industrial, or government laboratory, and some had no scientific training at all.").

[38] Cole, *Acculturating Forensic Science*, *supra* note 25, at 463 (quoting NAS REPORT, *supra* note 22, at 113, 125).

[39] Cole, *Acculturating Forensic Science*, *supra* note 25, at 464–65.

> with more scientific training and knowledge than technicians. These individuals would . . . not necessarily need the set of skills necessary to be independent basic researchers themselves. These individuals would presumably function as laboratory technical managers. They would know whether certain techniques are valid or not, not because they had validated them themselves or even seen it done, but because they would understand what a validation consists of. . . .[40]

Cole explains that there is an alternative proposal, quite the opposite of his own proposal, termed the "California School":

> The California School might . . . agree that it was a mistake to expect technicians without significant scientific training to defend, or even talk or think coherently about the validation of techniques like latent print or firearms and toolmark identification, they might argue that the answer is not to keep those non-scientifically trained individuals in the technician role. Rather, they might argue that the goal should be to turn all persons occupying the role of "forensic scientist" into true scientists with a scientific approach to empirical questions—in short, a "scientific culture."[41]

Here, in this debate between Cole and the California School, is where I think an intervention of third wave science studies might be helpful. Cole explains that laboratory technicians

> rarely, if ever, hold M.D.'s or Ph.D.'s. Although they do not have the knowledge that clinicians have acquired by attending medical school . . . [,] these individuals often have very sophisticated knowledge that takes other forms. . . . Some laboratory technicians may be more competent than physicians at performing certain laboratory procedures, and some radiological technicians may be more skilled at reading images than their clinician supervisors. Crucially, however, even when technicians possess superior skills, we do not generally expect them to make

[40] Cole, *Acculturating Forensic Science*, *supra* note 25, at 466–67. *See also id.* at 470 ("This is a view that I associate most closely with generalist forensic scientists with research focused scientific educations like Inman, Rudin, and DeForest. This view is also loosely associated with the University of California, Berkeley forensic science program run by Professor Paul Kirk. For convenience, we can refer to them as the "California School." The California School might argue that, rather than deskilling and segmenting the profession, we should be uplifting it.").

[41] Cole, *Acculturating Forensic Science*, *supra* note 25, at 470.

diagnoses.[42]

I think Cole is getting close here to third wave insights. His reference to the field of medicine breaks up the notion of science into experts, some of whom are not scientists. One does not have to deal with whether forensic science is science at all—the key question is: is it expertise, in accord with consensus science?

In 2013, Cole again published an article on forensic science addressing the question of whether "forensic culture" could become anything like "scientific culture."[43]

> [Cole's argument was] not merely that forensic science differs from research science; such a claim would be trivial and tell us little about what is distinct about forensic science. After all, much 'science' that goes on in a university, such as routine laboratory assays, is also distinct from "research science." Rather, my argument is that "forensic culture" can be characterized as a culture that is distinct even from other epistemic cultures that are not research science: regulatory science, industrial science, medical science, engineering science, routine laboratory procedures, and so on.[44]

The unique characteristics of forensic science include the facts (i) that it does not "seek to develop general knowledge claims," (ii) that reproducibility is not a goal, (iii) that its reward structure is not oriented to publication and prestige, (iv) that it is more adversarial and less transparent than conventional science, (v) that its audience is the criminal justice system (and not the scientific academy), which sets its research agenda, and (vi) that its claims lack modesty.[45] Cole concludes by suggesting that the solution (to the shortcomings of forensic science)

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[42] Cole, *Acculturating Forensic Science*, *supra* note 25, at 465–66.

[43] *See* Cole, *Forensic Culture*, *supra* note 25.

[44] *See* Cole, *Forensic Culture*, *supra* note 25, at 39.

[45] *See* Cole, *Forensic Culture*, *supra* note 25, at 39–43; *id.* at 43 ("[As to the last (vi) characteristic, Cole notes that] [f]orensic science is structured in such a way that valid feedback is rare. To be sure, there are odd occasions in which independent data does resist forensic science. Forensic scientists may occasionally come to conclusions that are refuted by mundane, common sense evidence. For example, a forensic conclusion that a suspect is the only possible source of a particular trace may be refuted by evidence that that individual was in prison at the time the trace was known to be deposited. Such occurrences are said to be infrequent— though no records of them are kept. In other cases, another analyst may disagree with the results of a forensic conclusion. Again, such occurrences are said to be uncommon—though no records of them are kept. Forensic results may also occasionally be refuted or challenged by the results of another type of forensic analysis. Again, there are no records to indicate how often such events occur.").

> posed by mainstream scientific institutions like the NAS—that forensic science "adopt[] scientific culture"—while perhaps a noble idea, is unrealistic. . . . [T]he social structure of forensic science is fundamentally different from that of research science. Changing "forensic culture". . . will require something closer to an exercise in social epistemology: deliberate thinking about what sort of "culture" will be conducive to producing whatever it is we want from forensic science. It is not at all clear that the result of such an exercise would be to set a goal of making forensic science as much like research science as possible.[46]

This conclusion makes even a stronger case for supplementing Cole's evaluation of forensic science with third wave studies of expertise—we want a culture of expertise instead of a scientific culture.

III. FOCUSING ON EXPERTISE: THE THIRD WAVE OF SCIENCE STUDIES

> The standard way to try to measure expertise externally is by reference to credential such as certificates attesting to . . . proficiency. [But note] that there are not credentials that indicate possession of many . . . expertises. . . . Therefore we conclude that credentials are not a good criterion for setting a boundary around expertise. . . . A criterion that does seem to set the boundary in a better place is experience in a [technical] domain. [Without] experience at judging the products of a technical domain, there is no specialist expertise.[47]

In 2007, Harry Collins and Robert Evans published *Rethinking Expertise*, an attempt to invent a sociology not of science, but of expertise.[48] The authors even constructed a taxonomy of expertise, beginning with *ubiquitous expertises* that everybody has in order to live in society—"a huge body of tacit knowledge"—and then moving to *specialist expertises*, the three lower levels of which "are better described as levels of [ubiquitous tacit] knowledge"—(1) "beer-mat knowledge," (2) popular understanding of science, and (3) primary source knowledge (literature, the internet).[49] The

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[46] *Id.* at 44. It is also "unrealistic . . . for the oft-stated reason that forensic scientists and those who employ them have evinced resistance toward" the goal of adopting scientific culture. *Id.*

[47] HARRY COLLINS & ROBERT EVANS, RETHINKING EXPERTISE 67–68 (2007).

[48] *See id.* at 2.

[49] *See id.* at 13–14. The reference to beer-mat (i.e., the cardboard coasters placed under beer glasses on a bar) knowledge comes from the phenomenon of printing statements or questions of general knowledge (e.g., distance to the moon, the country with the largest

higher levels of specialist expertise (or "specialist tacit knowledge"), requiring more than ubiquitous expertise, are most relevant to forensic science: (5) *contributory* expertise, "which is what you need to do an activity with competence,"[50] and (6) *interactional* expertise, "which is the ability to master the language of the specialist domain in the absence of professional competence."[51] The latter category, "a new concept" and the focus of much of *Rethinking Expertise*,[52] is important because it captures the genuine expertise of a non-scientist (i.e., without formal training or credentials) who, through experience in a scientific community, knows what he or she is talking about when there is a scientific controversy. Finishing out the taxonomy, there are five *meta*-expertises, including (i) ubiquitous discrimination and (ii) local discrimination, both of which involve judges who are not experts but make judgments about them; (iii) technical connoisseurship, the expertise of an art critic who is not an artist; (iv) downward discrimination, when a specialist judges a lesser expert; and (v) referred expertise, when an expert moves to a new domain and applies his or her expertise from an earlier domain.[53] As an aside, the gatekeeping judge following *Daubert* is a meta-expert exercising ubiquitous discrimination to evaluate "the experts' demeanor, the internal consistency of their remarks, . . . and so forth."[54] My focus will be on the two highest levels of specialist expertise: contributory expertise and interactional expertise.

In distinguishing these two higher levels of specialized expertise, Collins and Evans note that the "first three categories of expertise, beer-mat knowledge, public understanding, and primary source knowledge, might be said hardly to enter the category of specialist expertise at all," since they don't require mastery of a domain, and basically involve "reading rather than immersion in the specialist culture. "'Enculturation' is the only way to master an expertise which is deeply laden with tacit knowledge because it is only through common practice with others that the rules that cannot be

population, etc.) on beer-mats, sometimes constituting a quiz, perhaps with the answer on the opposite side. An example, is a beer-mat with a definition of a hologram, thereby delivering knowledge to the reader:

> The words on the beer mat are not simply nonsense nor could they be taken to be, say, a riddle or a joke. Presumably there are people . . . who have studied the beer mat and, if asked: "Do you know how a hologram works" would reply: "Yes" . . .

Id. at 18. Notably, acquisition of the three low levels of expertise "rests on the prior acquisition of a vast, but generally unnoticed, foundation of ubiquitous expertise." *See. id.* at 14.

50 *See id.* at 14.

51 *See* COLLINS & EVANS, *supra* note 47, at 14.

52 *See* COLLINS & EVANS, *supra* note 47, at 14.

53 *See* COLLINS & EVANS, *supra* note 47, at 15.

54 *See* COLLINS & EVANS, *supra* note 47, at 15.

written down can come to be understood."[55]

After defining expertise as immersion in a specialist "culture," Collins and Evans split that with expertise between contributory and interactional experts. As to contributory experts, which is the conventionally recognized type of expert, they begin as novices and advance through the stages of advanced beginner, competence, proficiency, and finally, expertise.[56] Interactional experts, an overlooked category, have no expertise in the practice of a domain, but are experts in its language.[57] That might seem *not* to be expertise; however, "mastery of any language, naturally occurring or specialist, requires enculturation within a linguistic community."[58] Interactional expertise therefore challenges both (i) the view that *full* immersion in a domain is necessary to master a language, and (ii) the view that mastering a domain's language requires only "the acquisition of propositional knowledge—a set of formal rules and facts gained through reading and instruction."[59]

> The idea of interactional expertise implies that complete fluency in the language of a specialist domain can be acquired in the *absence* of full-blown physical immersion . . . [and] the level of fluency . . . that can be attained by . . . an interactional expert is indistinguishable from that [of] a full-blown contributory expert.[60]

The significance of this analysis for Cole's evaluation of forensic science, and its culture, is that an expert in a scientific field, for example, need not be a scientist who "contributes" to that field—examples offered by Collins and Evans include "activists," seemingly mere members of the public who actually *know* enough to interact successfully with scientists.[61] More importantly, the interactional expert is often one who communicates to the general public, such as a sociologist of science who publishes a study of a scientific domain, or a science journalist who reports on a scientific controversy.[62]

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55 COLLINS & EVANS, *supra* note 47, at 24.

56 *See* COLLINS & EVANS, *supra* note 47, at 24–27.

57 *See* COLLINS & EVANS, *supra* note 47, at 28.

58 COLLINS & EVANS, *supra* note 47, at 30.

59 *See* COLLINS & EVANS, *supra* note 47, at 28–29.

60 COLLINS & EVANS, *supra* note 47, at 30–31.

61 *See* COLLINS & EVANS, *supra* note 47, at 32. In order for an interactional expert to successfully interact with a contributory expert, the latter must possess interactive ability. *See id*. at 38.

62 *See* COLLINS & EVANS, *supra* note 47, at 31–32. Other examples include salespersons, or a manager of a large scientific project who is not "a contributor to everyone else's narrow specialism." COLLINS & EVANS, *supra* note 47, at 32.

Collins and Evans even raise the question of whether an interactional expert could be admitted as an expert witness, since in their view "interactional expertise is just as good in forums that work through the medium of language as contributory expertise."[63] The example offered by Collins and Evans is Simon Cole himself, in his role as an expert in fingerprint cases—although he has studied the profession, he has been attacked on cross-examination (as a junk scientist) because he is not (and has no experience as) a fingerprint examiner:

> What we would like to bring about is the establishment of a discourse that would enable Cole . . . under cross-examination [to respond] with a confident: "I do not have contributory expertise in the matter of fingerprint identification but I do have interactional expertise in the domain. . . ."[64]

Of course, the category of contributory experts in science is not limited to the core set of trained scientists, because Collins and Evans talk of the possibility of specialist contributory "experts without formal qualifications," who have "no paper qualifications."[65] Cole, however, had neither formal training nor experience as a fingerprint examiner, so his expertise was interactional. Given that Collins and Evans believe that an interactional expert (unlike a mere member of the public) has the legitimacy to participate in scientific decision-making, it is not surprising that they believe an interactional expert should be able to testify as an expert witness.

The final piece of this focus on expertise is the problem of pseudo-science—allowing extrinsic influences to distort the results of tests, studies, or experiments. Here, Collins and Evans can only rely on consensus where it exists, such that when real scientists propose a new theory based on new findings,

> the scientists pushing forward in the new direction have the intention to change as little as possible consistent with their new theories and findings. They do not want to overthrow the

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63 *See* COLLINS & EVANS, *supra* note 47, at 42.

64 COLLINS & EVANS, *supra* note 47, at 72.

65 *See* COLLINS & EVANS, *supra* note 47, at 49. Collins and Evans offer the example of "expert" farmers in Brian Wynne's study of the effect of the Chernobyl disaster on Cumbrian sheep farmers "after radioactive fallout contaminated their pastures." *Id*. at 48 (citing Wynne, *supra* note 15); *see also* COLLINS & EVANS, *supra* note 47, at 49 ("The sheep farmers have specialist contributory expertise"). Collins and Evans have therefore constructed "a wider envelope of experts . . .in that anyone with the right kind of experience, whether they have scientific training or not, has a potential place inside it." *Id.* at 114.

> scientific method, nor the greater body of scientific findings, nor the major social institutions of science, nor the existing data of science.[66]

Science as we know it involves "the elimination of personal bias" and the preservation of "continuity between a new approach and the main body of science."[67]

I agree with Cole's arguments (i) that forensic "culture" has numerous characteristics—unique tasks, failures, and audiences—that distinguish it from, for example, the scientific culture of a university or a pharmaceutical research laboratory; (ii) that the medical model may be more appropriate in terms of the organization of forensic culture; and (iii) that the goal of making sure every forensic technician is a trained scientist is not necessarily the preferred solution to the shortcomings of forensic science. I would add that a focus on expertise, rather than science, helps explain these three arguments, because (i) expertise can be attained by academic training or by experience, (ii) an expert is not necessarily a practitioner in the field, and (iii) an expert who can explain a scientific domain (through interaction with core scientists) need not be a contributor to the scientific progress of the domain. To demonstrate this re-orientation away from science and toward expertise, I briefly describe the perceived weaknesses of, and proposed solutions for improvement of, arson investigators who serve as expert witnesses for prosecutors.

IV. THE EXAMPLE OF ARSON INVESTIGATION

Sometimes, with the benefit of insight gained over time, we learn that what was once regarded as truth is myth, and what was once accepted as science is superstition.[68]

A. *The Gap between Fire Science and "Forensic Fire Science"*

Cole mentions that in the late 1980s, "an increase in the criticism of various forensic disciplines based on lack of standard practices and validation" resulted in new attention to the field of arson investigation: "more than a third of the first twenty standards promulgated [by American Society for Testing and Materials' committees] by 1995 dealt with fire investigation. The source of those standards . . . were standards put forth in 1998 by the International Association of Arson Investigators."[69]

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66 *Id.* at 130.

67 *See id.* at 130, 132.

68 *Lee v. Tennis*, No. 08-cv-1972, 2014 WL 3894306 (M.D. Pa. June 13, 2014), http://truthinjustice.org/FinalRulingHanTakLee.pdf.

69 Cole, *supra* note 18, at 574.

John Lentini, a leading fire investigator who was influential in these efforts, has since become a leading critic of the arson investigation techniques used in recent decades by purported fire experts. He has accused prosecutors of employing "every fire science myth in the book," and declared the history of arson expertise to be "the ultimate triumph of junk science."[70] In another account:

> One notorious area of junk forensic science to come under scrutiny in recent years is arson investigation. . . . As early as 1977, a study had warned that there was no scientific research to validate the field's dominant theories about the allegedly foolproof signs of arson. Yet the courts continued to allow those theories to be heard by juries, producing countless convictions.[71]

Some of the mythologies about the cause and origin of a fire arise from intuition: "The notion that gasoline burns hotter than wood is appealing," but false, even though this mistake made it into the first edition of *Kirk's Fire Investigation* and even though some investigators "infer the presence of accelerants when they observe a melted aluminum threshold."[72] As to the myth of crazed glass, the "notion that crazed glass indicates that the glass was rapidly heated was appealing enough that Brannigan, Bright and Jason, three respected fire researchers at the National Bureau of Standards (now [the National Institute of Standards and Technology, or] NIST), allowed it into the *Fire Investigation Handbook*."[73]

These publications tend to extend the life of mythologies, especially when picked up in training courses where "hundreds of investigators can be exposed to this false 'gospel.'"[74] Moreover, much of this "misinformation

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[70] *See* Mark Hansen, *Long-Held Beliefs About Arson Science Have Been Debunked After Decades of Misuse and Scores of Wrongful Convictions*, ABA J., (Dec. 2015), http://www.abajournal.com/magazine/article/long_held_beliefs_about_arson_science_have_been_debunked_after_decades_of_m ("Lentini says prosecutors employed nearly every fire science myth in the book, 'It's the ultimate triumph of junk science,' he says.").

[71] Radley Balko, *A Brief History of Forensics*, WASH. POST (Apr. 21, 2015) https://www.washingtonpost.com/news/the-watch/wp/2015/04/21/a-brief-history-of-forensics/?utm_term=.bf39e5c95377. This quotation highlights an often overlooked dimension in the discourse criticizing arson expertise, namely the role of judges as co-producers of this junk science. *See* Gary Edmond & Emma Cunliffe, *Cinderella Story? The Social Production of a Forensic "Science*,*"* 106 J. CRIM. L. & CRIMINOLOGY 219 (2017).

[72] *See* John J. Lentini, *The Mythology of Arson Investigation*, http://firescientist.com/Documents/The%20Mythology%20of%20Arson%20Investigation.pdf (last visited Feb. 8, 2018). This paper is essentially a distillation of Chapter 8 from the author's textbook, JOHN J. LENTINI, SCIENTIFIC PROTOCOLS FOR FIRE INVESTIGATION (1st ed. 2006).

[73] *Id.*; U.S. DEP'T OF COMMERCE, NAT'L BUREAU OF STANDARDS, FIRE INVESTIGATION HANDBOOK (1980).

[74] *See id. See also id.* at 4 ("Much of the mythology about fire investigation was collected

has not been officially repudiated or 'recalled' . . . [and] the libraries of most fire investigators contain numerous texts that are filled with this misinformation."[75]

B. *Training and Education: Experience Versus Science?*

A common criticism of forensic arson experts is that "most . . . are not scientists"—a "recent survey of 217 fire investigators" found that 14% had an advanced degree, 34% had a college degree, and 13% had only a high school education.[76]

> In most states, in order to be certified, investigators had to take a forty-hour course on fire investigation, and pass a written exam. Often, the bulk of an investigator's training came on the job, learning from "old-timers" in the field, who passed down a body of wisdom about the telltale signs of arson, even though . . . there was nothing in "the scientific literature to substantiate their validity."[77]

The National Fire Protection Association published scientifically based guidelines in 1992, but many arson investigators still "believed that what they did was more an art than a science—a blend of experience and intuition."[78]

> In 1997, the International Association of Arson Investigators filed a legal brief arguing that arson sleuths should not be bound by [*Daubert*]. What arson sleuths did, the brief claimed, was "less scientific." By 2000, after the courts had rejected such claims, . . . there remained great variance in the field, with many practitioners still relying on the unverified techniques that had been used for

by the Aerospace Corporation, under a contract to the Law Enforcement Assistance Administration (LEAA) in a 1977 booklet entitled *Arson and Arson Investigation: Survey and Assessment.* To their credit, the authors of this survey pointed out, '[a]lthough burn indicators are widely used to establish the causes of fires, they have received little or no scientific testing.' They recommended, 'a program of carefully planned scientific experiments be conducted to establish the reliability of currently used burn indicators. . . .'").

75 *See id.* at 20.

76 *See* Hansen, *supra* note 70.

77 David Grann, *Trial by Fire: Did Texas Execute an Innocent Man?*, NEW YORKER (Sept. 7, 2009) http://www.newyorker.com/magazine/2009/09/07/trial-by-fire. Some states have higher standards: New Hampshire requires "a two-year associate's degree in fire science or a related field, . . . an intensive training course, and . . . supervised on-the-job training sessions for at least a year." *See* Douglas Starr, *Spark of Truth: Can Science Bring Justice to Arson Trials?*, DISCOVER MAG. (Oct. 24, 2011), http://discovermagazine.com/2011/nov/12-spark-truth-science-bring-justice-arson-trials.

78 *See* Grann, *supra* note 77.

generations.[79]

These observations led to a false dichotomy between scientific expertise and experience in the discourse criticizing inept arson investigators. For example, Rachel Dioso-Villa refers to "the contentious nature of the field of fire and arson investigation where opinions based on experience, rather than science, are still highly valued and deemed part of acceptable practice within the community."[80] I disagree with that dichotomy, and Dioso-Villa actually provides the basis for my disagreement! The unreliability of forensic arson expertise is not that the experts are not scientists, but that their "culture" is disconnected from consensus fire science:

> The fields of fire investigation and fire protection engineering developed on parallel tracks that published separate texts, manuals, and guides for practitioners. On the one hand, fire protection engineers attempt to understand the physical processes of fire and how to control its growth and spread in different environments using different substances. . . . On the other hand, fire fighters and police officers, whose primary objective was to determine whether the cause of a fire was accidental or incendiary, developed arson investigation.[81]

That is, the discipline of fire protection engineering was "continually evolving, due to the need to consider the effects of new materials, structures, and fuels and its reliance on mathematical equations, computer modeling, and the analysis of empirical data allow for the accommodation of new findings"[82]—and fire investigators were not keeping up. Dioso-Villa sees the problem as a lack of "scientific training," adding that it was not "necessary for them to hold a higher educational degree beyond a high school diploma," and arguing that the result of these educational deficiencies was that "theories and heuristics about the ways in which fires behave developed out of the collective experience of fire investigators within the community through their field investigations. Mentors passed down this experiential knowledge to their apprentices without experimental or scientific testing to validate their claims."[83]

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79 *See id.*

80 Rachel Dioso-Villa, *Scientific and Legal Developments in Fire and Arson Investigation Expertise in* Texas v. Willingham, 14 MINN. J.L. SCI. & TECH. 817, 841 (2013).

81 *Id.* at 821.

82 *See id.*

83 *Id.* at 821–22.

My point is that if the new arson investigator's training, including on the job training by mentors, as well as their texts, manuals, and guides, were all in sync with consensus fire science (i.e., their "knowledge" would be validated), we should not be so concerned with educational levels or whether investigators are trained "scientists." Experiential knowledge, learned in a community through mentors, need not be the opposite of scientific knowledge. For example, we would not doubt the investigative abilities of a highly-experienced homicide detective simply because he or she (i) lacked a college education and (ii) learned his or her skills on the job.

C. *The Problems of Bias and Over-Claiming*

"[F]ire investigators—and the people who use them as experts—need to be prepared to accept the reality that sometimes the best answer that can be obtained is 'undetermined,' if either an accident or an incendiary call is not supported by conclusive evidence."[84]

There is also the question of bias in forensic fire scene investigation. First, being in the role of a criminal investigator—recall that the NAS Report recommended independence of forensic science from law enforcement—"threatens objectivity [and] has a direct impact on what information an investigator seeks, as well as how the information is perceived and processed."[85] Second, expectation bias

> is the tendency for observers to believe and express data that agree with their expectations for the outcome of an experiment, and to disbelieve, discard, or downgrade the corresponding weightings for data that appear to conflict with those expectations. . . . The less instrumented and more subjective a forensic technique or measurement, the more it is subject to expectation induced errors. The vague and subjective nature of fire pattern analysis makes it especially susceptible to expectation bias.[86]

Finally, the potential for over-claiming raises the question of uncertainty; in one case where a convicted defendant had likely been

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[84] John J. Lentini, *The Evolution of Fire Investigation and Its Impact on Arson Cases*, 27 CRIM. JUST. 1 (2012) https://www.americanbar.org/content/dam/aba/publications/criminal_justice_magazine/sp12_fire_investigation.authcheckdam.pdf.

[85] Paul Bieber, *Forensic Fire Scene Examination—What It Tells Us, and What It Doesn't*, 40 CAL. ATT'YS CRIM. JUST. FORUM 33, 33, 36 (2013).

[86] *Id*. *See also id.* ("In a two-part blind research study on the [effects] of expectation bias on fire pattern analysis conducted by the Arson Research Project in 2012, fire investigators who were given biasing information prior to analyzing a set of fire patterns were 18% more likely to choose an unreliable methodology in conducting their analysis than a control group who examined the same fire patterns without biasing information.").

innocent of the charge of arson, "an internationally known fire and explosives expert . . . said the blaze was almost certainly accidental, [thus] the cause of the fire should have been labeled 'undetermined'. . . ."[87] Even though the science was not there to support the verdict, the expert confirmed a concern of Collins and Evans:

> [S]cientific knowledge takes a long time to make and therefore scientists are often pressed to make authoritative decisions on technical matters before there is any consensual scientific knowledge on which to base them. . . . [T]oo much greed for scientific authority is bad for science, forcing scientists to act in scientifically inauthentic ways.[88]

Viewing this situation from Cole's perspective, the question is: what culture should arson investigation develop to overcome its shortcomings?

D. *Non-Scientist Experts*

Some forensic identification techniques do not seem to be linked to any scientific field or discipline—I am thinking about bite mark, hair, footprint, and gait comparisons. Adopting a scientific culture in those practices would seem to require validation by a new regime of testing and a new scientific discipline. Arson investigation, however, *is* linked to fire chemistry and to disciplines like fire safety engineering (which studies how fires start and end, how materials burn, and so forth). Those fields are scientific cultures, and arson investigators could potentially obtain higher degrees and join their ranks. The more important goal, however, is to ensure that investigations (and any associated expert testimony) proceed under the guidance of consensus science. Recall Cole's arguments that (i) a forensic "culture" is likely different from, for example, a university research facility; (ii) the medical model may be more appropriate for organizing that forensic culture; and (iii) requiring scientific degrees may not be the best or only means to ensuring the reliability of forensic expertise. Using forensic arson investigation as an example, and focusing on expertise rather than science, I would argue that (i) fire investigation expertise can be attained by academic training or by experience, (ii) an expert fire investigator need not be a practitioner in the fields of fire chemistry or fire safety engineering; and (iii) an expert who can testify in accordance with consensus science (due to interaction with fire scientists) need not be a contributor to the scientific research of the discipline.

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87 *See* Starr, *supra* note 77.

88 COLLINS & EVANS, *supra* note 47, at 8.

V. CONCLUSION

> Certainly it is possible for individuals with no chemistry or physics beyond high school to apply themselves and learn the basic science, and keep up with developments in the field.[89]

I have attempted in this Article to supplement Cole's useful arguments with some insights from the recent project to develop a sociology of expertise, that is, the study of how experts are enculturated into a domain of knowledge. Irrespective of the discourse critical of the unfortunate history of arson investigation, experience is not the opposite of expertise; properly anchored in consensus science, experience can be a marker of expertise.[90] The notion that a realistic goal may be to encourage *forensic* cultures in sync with science, rather that full-blown *scientific* cultures, finds support in the effort to identify reliable experts irrespective of their level of formal education.

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[89] Lentini, *supra* note 84.

[90] By claiming that experience *can* be a marker of expertise, I am not taking issue with those who argue that a judge should not view (mere) credentials or experience as markers of expertise. *See* Brandon L. Garrett & Gregory Mitchell, *The Proficiency of Experts*, 166 U. PA. L. REV. (forthcoming 2018), https://www.law.uchicago.edu/files/file/garrett_-_mitchell_-_the_proficiency_of_experts_v.27.pdf. ("We argue that credentials and experience are often poor proxies for proficiency."). Garrett and Mitchell argue that judges should demand actual proficiency, and rely on proficiency testing, before qualifying an expert.