

Who Will Regulate American Forensic Science?

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I. INTRODUCTION

It is an honor to be invited to contribute to this symposium for a scholar and friend who truly merits a term I do not use lightly: mentor.¹ Professor Michael Risinger spent hours discussing some of my early articles with me, leading to vast improvements in their expression, argumentation, and wit. As I became a target for aggrieved forensic practitioners whose discipline I had questioned, Michael's and colleagues' writings became a model for my efforts at rebuttal through careful and methodological argumentation and, again, the occasional touch of wit.² Later, Michael was candid enough to tell me when I learned that lesson perhaps too well.³

I am especially flattered that Michael invited me to contribute to this symposium by "using my science studies chops." Michael's outsider's explication of the sociology of science using my work as an example, which he delivered at Professor Caudill's symposium in 2006, remains a classic piece of writing about science studies—at least to me.⁴ In this passage,

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¹ Two such people are retiring this year. The other is William Thompson.

² E.g., D. Michael Risinger et al., *Brave New 'Post-Daubert World'—A Reply to Professor Moenssens*, 29 SETON HALL L. REV. 405 (1998).

³ E.g., Simon A. Cole, *Don't Shoot the Messenger By One of the Messengers: A Response to Merlino et al.*, 45 TULSA L. REV. 111 (2009).

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, 686 (2007). Curiously, passages of this article reappeared verbatim in a redacted

Michael used the example of fingerprint identification as an example of the extreme relativist position often associated with science studies, and he used my discovery of those limits and the necessity for me of distinguishing between the limits of scientific knowledge in a strictly epistemological sense from limits produced by the fact that this particular group of knowledge-makers had in fact failed to correctly identify the kind of studies and data that would have supported the knowledge claims that they were making and thus, not surprisingly failed to amass those studies and data.

What is interesting and challenging about forensic science, as Michael's paper illustrates, is that debates about philosophy of science are not all that helpful in tackling what we might call "the problem" of forensic science. When a discipline has not even framed an empirical question, there is little need to debate the merits of relativism versus realism. Nor, I would argue, is it helpful—as so many seem to think it is—to exhort the forensic disciplines to fit themselves to a template called "the scientific method" constructed around hypothesis testing. As Michael, drawing on Susan Haack, has correctly argued, "scientific method" is more an honorific than a universal description of every activity society conventionally calls "science." And yet, forensic disciplinary communities certainly do make scientific knowledge of a sort; their claims enjoy broad social acceptance both internally and externally. It is probably fair to say that most of these disciplinary communities are still struggling to move forward to more defensible knowledge claims, despite more than a decade of work by outsiders and insiders trying to push them in this direction. How and why these things happen, it seems to me, are sociological questions.⁵

(and thus, to me, anonymous) version of a letter supporting my case for promotion to Associate Professor with tenure. When I was being considered for promotion to full Professor, Michael dispensed with anonymity entirely. I was minding my own business one weekend afternoon, when my mobile phone rang, and Michael barked, "It's Michael. Your promotion file. I agree with almost everything you say. But publication #3, footnote 26—explain yourself." After a lengthy discussion, we bid goodbye, and I started to tell my spouse, "the funniest thing just happened." At that point, my mobile phone rang again . . .

⁵ David Caudill offers such a sociological contribution in this volume. David Caudill, *Toward A Sociology of Forensic Knowledge? A (Supplementary) Response to Cole*, 48 SETON HALL L. REV. 583 (2018). In this paper, Caudill adopts the perspective of one of the pioneers of science studies, Harry Collins. Collins, somewhat notoriously, has broken with much of the field by accusing science studies of leveling expertise in such a way as to afford the views of "experts"—however defined—as deserving of no greater weight than laypeople. In place of this leveled view, Collins and his collaborators offer a typology of kinds of expertise. Working in this tradition, Caudill shows that for forensic fire analysts—but the point could be made equally well for many forensic disciplines—the question of how they make knowledge is more fruitfully framed around *expertise* than around *science*. And, he argues that it is possible, and perhaps even practically necessary, to imagine a community of fire experts who deploy scientific knowledge about the behavior of fire that was made by others (by scientists) even though they did not, and perhaps are not even qualified to, generate that knowledge themselves. I agree entirely with this argument. And, I share Collins's and

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For the past eight years, I have begun my writings about forensic reform by suggesting that the post-National Research Council (NRC) report (also known as the “NAS Report”)⁶ era, beginning in 2009, is a historic moment for American forensic science. Whether it will go down in history as a turning point of reform or a lost opportunity, however, is still not clear.⁷ In 2017, it is possible to believe that this year will, when history is written, become yet another landmark year because of the closing of the National Commission on Forensic Science (NCFS). Note also that the forensic reform efforts of the White House Office of Science and Technology Policy (OSTP) ended with the Obama Administration. Likewise, while one can hope that the report on forensic science by the President’s Council of Advisors on Science and Technology (PCAST) will be influential, PCAST itself is completely in abeyance, and will be unable to act on anything, let alone on forensic science, during the current administration, and perhaps forever.⁸ I am less certain of 2017’s landmark status than of 2009’s, but it could be.

Caudill’s belief that expertise exists, that expertise is not undifferentiated, that it is important to articulate the grounds for various expertise, and that it is useful and interesting to move beyond the deconstruction of all knowledge claims on to the problem of how we should make consequential decisions, *despite* the fundamental uncertainty of all knowledge claims. Legal disputes are one setting where that issue is forced. And, I am flattered by both Collins’s and Caudill’s use of my own supposed expertise about the scientific validity of fingerprint identification—which is quite different from the expertise possessed by a forensic practitioner (like Andrew Sulner, *see* this volume)—as an example of their theory of expertise. But, I will say that I still think today upon reading Caudill’s paper—that a typology of *tasks* is a more useful way of parsing out the intersecting knowledge claims surrounding forensic science than a typology of expertise. Put simply, designing and performing a scientific study to test a forensic knowledge claim about the behavior of fire is quite a different task from forming an opinion about the origin of a fire in a particular case. And, consuming, understanding, evaluating, and interpreting such a study is a different task still. So, for these reasons, I have always found it more helpful in forensic science to think about tasks than about expertises. Space precludes me from saying much more about this, except to note that *task* is, of course, a “Risingerian” word—a concept central to major contribution to the body of scholarship on *Daubert* and *Kumho Tire*.

⁶ NAT’L RESEARCH COUNCIL OF THE NAT’L ACAD. OF SCIENCES, STRENGTHENING FORENSIC SCIENCE IN THE UNITED STATES: A PATH FORWARD (2009), <https://www.ncjrs.gov/pdffiles1/nij/grants/228091.pdf> [<https://perma.cc/DVV2-6CTZ>] [hereinafter NRC REPORT]. For the definitive account of how to describe that report, see D. Michael Risinger, *The NAS/NRC Report on Forensic Science: A Path Forward Fraught with Pitfalls*, 2010 UTAH L. REV. 225, 225 n.2 (2010).

⁷ I do not, of course, mean that forensic reform actually began in 2009. It began, in many ways, years, even decades before that. I mean, rather, that it gained significant prominence and momentum with the publication of the NRC Report. I am suggesting that a historian of forensic science writing 50 or 100 years hence might well see 2009 as a particular important date.

⁸ PRESIDENT’S COUNCIL OF ADVISORS ON SCI. & TECH., FORENSIC SCIENCE IN CRIMINAL COURTS: ENSURING SCIENTIFIC VALIDITY OF FEATURE-COMPARISON METHODS (2016), https://obamawhitehouse.archives.gov/sites/default/files/microsites/ostp/PCAST/pcast_forensic_science_report_final.pdf [<https://perma.cc/EKZ3-FRA6>].

One possible reading for future historians of American forensic science will, of course, be to read 2017 as the beginning of the end of the historic period of forensic reform, which began in 2009. Most disturbing in this regard is the undisguised glee with which interest groups resistant to forensic reform, such as the National District Attorneys Association, greeted the news, which suggests that, contrary to prosecutors' earlier expressions of eagerness to work with other stakeholders on forensic reform, they were in fact simply waiting the NCFS out.⁹ On the other hand, it is also possible to read the closing of the NCFS as a mere bump in the road toward forensic reform. Perhaps enough momentum has been built up to move forward. I do not pretend to be able to know at this point which reading is more correct.

If momentum has been built up, then where will forensic reform come from? Presumably it may come from the disciplines themselves, from institutions like the American Academy of Forensic Science (AAFS), the Federal Bureau of Investigation, state forensic science commissions, the innocence movement, Europe, etc. None of these entities, however, has the type of broad official remit over all of American forensic science that the NCFS had. There is only one entity remaining with that broad remit: the Organization of Scientific Area Committees (OSAC) administered by the National Institute of Standards and Technology (NIST).

In this sense, in 2017, OSAC suddenly became the last organization standing of the forensic reform effort that gained significant momentum from the NRC in 2009. One thing that is curious about this is that OSAC does not, at first glance, seem to have a mandate to reform all of forensic science. OSAC has a rather limited mandate centered around *standards*. To be sure, the absence of standards has long been considered one of the weaknesses of American forensic science, which many scholars and the NRC report have mentioned prominently. But the absence of standards is hardly the only problem with contemporary American forensic science, and, arguably, it is not the most important or most interesting problem. A brief survey of general problems with American forensic science might, in addition to standards, include:

- Forensic science is inadequately resourced by governments to do what is asked of it.
- Forensic science is insufficiently connected to “mainstream” science or “national science assets.”

⁹ Spencer S. Hsu, *Sessions Orders Justice Department to End Forensic Science Commission, Suspend Review Policy*, WASH. POST (Apr. 10, 2017), https://www.washingtonpost.com/local/public-safety/sessions-orders-justice-dept-to-end-forensic-science-commission-suspend-review-policy/2017/04/10/2dada0ca-1c96-11e7-9887-1a5314b56a08_story.html?utm_term=.0f9299da7163 [https://perma.cc/WP75-53Y9].

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- Forensic science testimony and reporting often invokes logical fallacies (e.g., “the prosecutor’s fallacy”).
- Forensic science testimony and reporting often over-claims—that is, overstates the probative value of the evidence.
- Many forensic techniques have not been validated.
- Forensic protocols fail to adopt procedures for minimizing confirmation bias that are well established in other areas of science.
- Most forensic laboratories are controlled by law enforcement agencies. This arguably creates potential pro-government bias and interferes with forensic scientists’ allegiance to “science.”
- Insufficient basic research is carried out in forensic science.
- The system of self-regulation that governs most of forensic science is insufficiently rigorous. Certification of analysts and accreditation of laboratories are insufficiently rigorous, not mandatory, and controlled by the profession itself to an inappropriate degree.
- Education and training in forensic science are insufficient.
- Many laboratories’ protocols and quality assurance/ control mechanisms are insufficiently rigorous.
- Forensic science lacks a sense of intellectual curiosity that would prompt research into answering basic empirical questions about the performance of various assays.
- Forensic science does not embody a sufficient commitment to the spirit of open inquiry to justify its self-conceptualization as “science.”
- The defensiveness and hostility of forensic science to exogenous criticism and efforts at partnership from academia is inconsistent with its self-conceptualization as “science.”¹⁰

If I had to choose the most important of these, I would probably pick “validation” rather than “standards.” “Standards” is only one of the four or five or nine¹¹ prongs of the *Daubert* legal standard for scientific evidence. And, again, it is arguably not the most important one; the most important one

¹⁰ Simon A. Cole, *The Innocence Crisis and Forensic Science Reform*, in *WRONGFUL CONVICTION AND CRIMINAL JUSTICE REFORM* 167–68 (Marvin Zalman & Julia Carrano eds., 2014).

¹¹ For a description of the many ways one can characterize the number of “Daubert factors,” see Mark P. Denbeaux & D. Michael Risinger, *Kumho Tire and Expert Reliability: How the Question You Ask Gives the Answer You Get*, 34 *SETON HALL L. REV.* 15, 32 n.64 (2003).

may well be “validation,” or, as it is called in *Daubert*, “testing.”¹²

The focus on standards is, of course, a consequence of the choice of NIST as the scientific agency to co-coordinate the forensic reform effort with the Department of Justice (DOJ). This choice itself, as is well known, came after the NRC considered and rejected a number of federal scientific agencies, such as the National Science Foundation, to spearhead the forensic reform effort. The NRC ended up advocating that an entirely new agency be created.¹³ The NRC also explicitly recommended against locating the forensic reform effort in the DOJ, a recommendation that was, crucially, rejected in the formation of the NCFS.

By the time it became clear that the NRC’s envisioned new forensic scientific agency was not going to be created, and that the DOJ was going to be heavily involved in the forensic reform effort, the splitting of responsibility between the DOJ and NIST had begun to seem to forensic reformers as perhaps the best deal that could be had. The NCFS was, at least, not completely controlled by the DOJ. And NIST was a respected agency that was viewed as having a true scientific orientation—that is, an orientation around scientific truth and knowledge. NIST, of course, also had some historic and contemporary involvement both in forensic science and in cognate areas like biometrics.¹⁴

Despite its scientific credentials, there were some oddities about NIST as the lead scientific agency for forensic science. The chief one, which became clear as work began, was NIST’s understandable insistence on sticking to its mission as an agency oriented toward the production of *standards*.¹⁵ NIST did not have a broad mandate to engage in the production of knowledge in the service of the justice system, if it was not connected to standards. The area in which this issue most clearly manifested itself was in the area of validation. Some scholars and members of the NCFS objected to the logic of creating standards for disciplines, assays, or procedures, which had not yet been validated. What was the point of creating standards for something that was not validated? Surely, validation should come first. But come from where? Who would do this validation, and who could create the incentive structure for validation to get done? This, of course, brought us back to the question of why validation had not been done in the first place.

¹² Edward J. Imwinkelried, *The Meaning of “Appropriate Validation” in Daubert v. Merrell Dow Pharmaceuticals, Inc., Interpreted in Light of the Broader Rationalist Tradition, Not the Narrow Scientific Tradition*, 30 FLA. ST. U. L. REV. 735, 735–38 (2003).

¹³ NRC REPORT, *supra* note 6, at 19.

¹⁴ Rich Press, *Who Was Detective X?*, NIST (Feb. 9, 2017), <https://www.nist.gov/featured-stories/who-was-detective-x> [<https://perma.cc/P2VT-EFD5>].

¹⁵ Nat’l Inst. of Standards & Tech., *NIST Mission, Vision, Core Competencies, and Core Values* (July 10, 2009), <https://www.nist.gov/about-nist/our-organization/mission-vision-values> [<https://perma.cc/8U2K-B8XC>] (updated Jan. 26, 2017).

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Historically, there has been little incentive structure for validation in American forensic science. Instead, there has been a disincentive for conducting validation studies owing to the permissiveness of the legal regime toward extreme scientific claims that could be made without validation. NIST was reluctant to take on the mission of validating all forensic disciplines, assays, and procedures. Instead, NIST was willing to perform some validation research and to serve as a central evaluator for the justice system of such research, which might be performed by others.¹⁶

So have we ended up at a point where, of all the problems with forensic science that have been identified, for historically contingent reasons, only the problem of “standards” will be dealt with? That, of course, is probably not the case. It is probably not the case because, as the emerging “sociology of standards” has pointed out, standard setting can be viewed as a form of regulation by other means. In this sense, by creating standards, OSAC can, in theory, regulate all or nearly all of American forensic science. Nearly everything can be covered by standards.

In this sense, we might view the impending period of standard-setting as a stage in the historical effort to regulate American forensic science. Therefore, it is perhaps helpful to review that history. It is, however, also perhaps helpful to consider the unique obstacles to regulation of forensic science, as opposed to other forms of science. I have argued elsewhere that many of the self-regulatory features thought to apply to science seem less applicable to forensic science.¹⁷ Science, especially academic science, is generally thought to operate under a prestige economy in which reputation matters more than money.¹⁸ To be sure, this claim is both oversimplified and dated. Even so, there remains something to it. Implicit threats to reputation are assumed to deter wrongdoing, such as scientific fraud and even just bad science.

Few forensic scientists live in this prestige economy in which reputational rewards are allocated in the form of citations for prestigious scientific publications. Rather, they operate in a much more bureaucratic structure within police organizational hierarchies with productivity

¹⁶ Nat'l Comm. on Forensic Sci., *Recommendation to the Attorney General, Technical Merit Evaluation of Forensic Science Methods and Practices* (Sept. 12, 2016), <https://www.justice.gov/archives/nfcs/page/file/905541/download> [https://perma.cc/2RW5-FXHN] (recommending “NIST’s evaluation may include but is not limited to: a) research performed by other agencies and laboratories, b) its own intramural research program, or c) research studies documented in already published scientific literature.”).

¹⁷ Simon A. Cole, *Forensic Culture as Epistemic Culture: The Sociology of Forensic Science*, 44 *STUD. IN HIST. & PHIL. OF BIOLOGICAL & BIOMEDICAL SCI.* 36 (2013).

¹⁸ See generally BRUNO LATOUR & STEVE WOOLGAR, *LABORATORY LIFE: THE SOCIAL CONSTRUCTION OF SCIENTIFIC FACTS* 187–230 (1979); M.J. Mulkay, Geoffrey N. Gilbert & Steve Woolgar, *Problem Areas and Research Networks in Science*, 9 *SOC.* 187, 195 (1975).

requirements.¹⁹ Their products are not scientific paper, but reports, which are not cited, but merely consumed and then discarded by the criminal justice system. These reports are not shared with peers in the conventional sense, and they rarely, if ever, generate reputational rewards for their authors. This situation means that the regulation of forensic science cannot come about merely through what is often called “scientific culture.” It needs to come from elsewhere: perhaps from the bureaucratic structure itself, perhaps from governments, perhaps from the consumers of the evidence.

A. *Self-Regulation*

Forensic disciplines have called for governments to regulate them since the early twentieth century.²⁰ These calls have been unsuccessful.²¹ American governments have showed little interest in regulating forensic science, leading to the oft-remarked situation that hairdressing and pet food production are more rigorously regulated by government than forensic science.

Part of the reason for this disinterest was that governments tend not to regulate professionals. They tend to self-regulate. This eventually became clear to the forensic community, generating calls for self-regulation, again as early as the 1920s and 1930s. The general consensus, however, is that forensic organizations, both the discipline-specific ones and the broader ones, such as the American Academy of Forensic Science and the American Society of Crime Laboratory Directors, did a poor job of self-regulation.²²

B. *Legal Regulation*

Courts can play a role in regulating forensic science. Courts are, in some sense, the leading, or perhaps sole, consumers of most forensic science. This gives them great market power over forensic science, should they choose to use it. This power could be exerted through legal opinions establishing various requirements for forensic science to be used in trials and other legal proceedings. Such requirements would presumably be highly influential over forensic science, given the understanding that the criminal proceeding is in some sense the hypothetical *telos* of all forensic evidence,

¹⁹ See generally Marie-Eve Sylvestre, *Policing the Homeless in Montreal: Is this Really What the Population Wants?*, 20 POLICING & SOC. 432, 451 (2010).

²⁰ For fingerprints, see SIMON A. COLE, SUSPECT IDENTITIES: A HISTORY OF FINGERPRINTING AND CRIMINAL IDENTIFICATION 210–11 (2001) (discussing efforts to regulate fingerprint identification in the 1930s).

²¹ See generally Paul Giannelli, *Wrongful Convictions and Forensic Science: The Need to Regulate Crime Labs*, N.C. L. REV. 86, 163–235 (2007).

²² *Id.* at 235 (“Paradoxically, the most scientifically sound procedure—DNA analysis—is the most extensively regulated, while many forensic techniques with questionable scientific pedigrees go completely unregulated.”).

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whether the forensic evidence is actually used in that criminal proceeding or not.²³

Courts have done very little to use this market power to regulate forensic science. Indeed, quite the opposite; courts have largely allowed forensic science to remain unregulated and even provided disincentives to regulate. This is arguably the principal message of the NRC report, in its perhaps most famous line, that many forensic disciplines remain invalidated and courts have been “utterly ineffective” in regulating these disciplines.²⁴

Interestingly, a recent discussion paper describing “Potential Concepts for OSAC 2.0” included, among four potential concepts, a “Federal/State/Local Partnership” concept.²⁵ This concept consists essentially of legal regulation. OSAC would abandon the writing of standards and shift its focus to the writing of model legislation. The contemplated reforms of forensic science would be promulgated because they are required by law, not because they are “standard.” Adoption of this concept would signify an embrace of legal regulation, although the mechanism would be legislative rather than judicial.

C. *Government Regulation*

In the absence of effective self- or legal regulation, thoughts turn once again toward government regulation. This was the solution to the ills of forensic science the NRC proposed in 2009: a standalone federal agency dedicated to regulating forensic science in all aspects.²⁶ It is also the solution adopted in the United Kingdom, which has a position called, literally, the Forensic Science Regulator.

The NCFS, as noted above, was the government’s effort towards government regulation of forensic science. It was, of course, not the agency envisioned by the NRC: it was not permanent, it had no enforcement authority, and it was controlled by law enforcement rather than independent entities. Despite all this, it was the most ambitious effort at government regulation of forensic science in American history.

Now, it has been closed by a change in presidential administration.²⁷ In a bizarre way, the otherwise reactionary comments on the PCAST report by The American Congress of Forensic Science Laboratories (ACFSL)—an

²³ William C. Thompson, *The National Research Council’s Plan to Strengthen Forensic Science: Does the Path Ahead Run Through the Courts?*, 50 *JURIMETRICS J.* 35, 51 (2009).

²⁴ NRC REPORT, *supra* note 6, at 109.

²⁵ Nat’l Inst. of Standards & Tech., *Potential Concepts for OSAC 2.0* (2017), <https://www.nist.gov/topics/forensic-science/potential-concepts-osac-20> [<https://perma.cc/X549-NL6Z>].

²⁶ See NRC REPORT, *supra* note 6, at 19.

²⁷ See Hsu, *supra* note 9.

organization subtitled “The United States Assembly of Forensic Science Laboratory Professionals” which, despite the grandiose name, is not a particularly well-established or representative forensic organization but rather a more self-appointed group—have proven to be remarkably prescient:

Interestingly, the PCAST report comes during a presidential administration that has demonstrated a deep sensitivity to the needs and demands of trial attorneys, criminal defendants, and advocates of sweeping criminal justice reform. Future administrations may take a different approach, tending to champion positions traditionally held by police and prosecutors. We have no opinion in these matters. But these swings in ideological perspective cause commensurate changes in how forensic science and its role in our criminal justice system are perceived. In the current political climate, forensic science is looked upon with far more suspicion and, in some cases, distain than would be the case in other political circumstances. And because forensic science is both expected and apt to remain independent of these political currents, it is vulnerable to being misrepresented and even bullied in a way that compromises its occupational stability. To truly strengthen forensic science, therefore, it will be necessary to somehow insulate it from the turbulence caused by changes in political winds. PCAST did no favors in this regard.²⁸

Thus, the ACFSL blunted the key asset of PCAST’s intervention into forensic science. Unlike commissions composed by stakeholders, like NCFS, the NRC committee, and, to a lesser extent, OSAC, PCAST is a purely scientific body with extremely strong credentials in what conventionally counts as “scientific prestige.” It intervened in a controversy, in which it was widely alleged that science had become politicized, primarily through the forensic scientists’ excessive orientation toward law enforcement. ACFSL adopted the now-familiar perversion of relativist sociology of science—that all science is “mere” politics, and, therefore, equally undeserving of trust—to cast PCAST as a political body and law-enforcement-employed forensic scientists as politically neutral. This argument turns the ideal that we go to governmental or quasi-governmental bodies for politically neutral scientific advice on its head.²⁹ This then, leads

²⁸ Am. Congress of Forensic Sci. Labs., *The 2016 PCAST Report*, Position Statement, (Sept. 21, 2016).

²⁹ See, e.g., STEPHEN HILGARTNER, *SCIENCE ON STAGE: EXPERT ADVICE AS PUBLIC DRAMA* (2000); SHEILA JASANOFF, *THE FIFTH BRANCH: SCIENCE ADVISERS AS POLICYMAKERS*

to the absurd argument that no government-sponsored scientific advice should ever be heeded because such advice is purportedly endlessly subject to revision through elections. Forensic science should be left autonomous, immune to any intervention from any government-associated scientific advice. This was a laughable argument until Election Day 2016 at which point it suddenly, unexpectedly became devastatingly prescient.

II. HISTORY OF AMERICAN FORENSIC SCIENCE STANDARDS

Given the obstacles along all the other routes toward regulation of forensic science, it is tempting to think of standard-setting as a plausible means of regulating forensic science. This is not unreasonable. In many other settings, standards have served as alternatives to regulation by states, organizations, or social conventions.³⁰ Can standards regulate forensic science? In order to answer this question, we will first turn to a brief history of standards in American forensic science and then turn to what the emergent sociology of standards and standardization might have to say about that question.³¹

As of the late 1980s, with perhaps one minor exception,³² there were no specifically “forensic science” standards promulgated by any recognized standards development organization in the United States. Indeed, each laboratory or practitioner was free to adhere to whatever practices might appeal to them, based on whatever were regarded as standard textbooks or authorities in any individual field. The state of the profession in regard to standards relating to validity and standard practice to ensure accuracy is well described in Peterson et al. (1989).³³ However, this was about to change.

In the late 1980s, there was an increase in the criticism of various forensic disciplines based on lack of standard practices and validation from both inside and outside forensic science.³⁴ Whether this was the spur is hard

(1990).

³⁰ 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, 71 (2010).

³¹ *Id.*

³² There was an early run at creating a standard-setting effort in the early 1970s, and in fact a committee of the American Society for Testing and Materials (ASTM), Committee E-30 on forensic science, was established on the request of members of the forensic science community in 1970, but it apparently remained virtually inactive. Committee E-30 had nominal subcommittees, including E-30-02 on document examination. The first and only standard promulgated by E-30 before the 1990s was E444: Standard Descriptions of Scope of Work Relating to Forensic Document Examiners (1972).

³³ Joseph L. Peterson & John E. Murdock, *Forensic Science Ethics: Developing an Integrated System of Support and Enforcement*, 34 J. FORENSIC SCI. 749 (1989).

³⁴ See, e.g., Randolph N. Jonakait, *Forensic Science: The Need for Regulation*, 4 HARV. J.L. & TECH. 109 (1991), <http://jolt.law.harvard.edu/assets/articlePDFs/v04/04HarvJLTech1>

to establish specifically, but for whatever reason, members of the forensic science community reached out to the American Society for Testing and Materials (ASTM, now ASTM International) in order to initiate an effective standards effort in regard to at least some parts of forensic science³⁵ ASTM was the oldest and largest “standards developing organization” (SDO) for consensus standards in the United States, and one of the oldest in the world. Its standards for industrial materials and processes were widely used and respected. Apparently (I say apparently because the specifics of this are hard to run down), anyone who appeared to be involved in a respectable enterprise in need of standards could join the ASTM and precipitate the formation of a committee to generate such standards. I say “respectable enterprise” because it is not clear that a group of astrologers seeking standards generated through the ASTM consensus process would have been allowed to form a committee, but it is not clear that they would not have been allowed to, or what criteria were in place to distinguish between enterprises like astrology and those who would be allowed to join and form a committee.

At any rate, in 1989, the ASTM was approached by members of the forensic science community who were then allowed to reorganize and rejuvenate the non-functioning ASTM committee E-30³⁶ on forensic science as a functioning standards committee. It is clear that one of the main movers of this effort was John Lentini, who saw advantages in the process both for promulgation and standardization of more valid procedures, and also for raising the status of those performing the procedures. In explaining this, Lentini wrote in 1995:

The impetus for standardization comes from several directions. Laboratories seeking accreditation can refer to Standard Test Methods for their written procedures, rather than re-inventing the wheel. Bodies that administer examinations for certification of individuals can have a body of knowledge from which to draw their examination materials. And, competent individuals performing valid tests will have an authoritative source to lend credibility to their conclusions, and to question the credibility of improper or invalid methodology.³⁷

Lentini’s influence on the early product of E-30 is clear. Lentini is one of the leading fire investigators in the world and was a leader in bringing

09.pdf [<https://perma.cc/5C3A-PC9D>].

³⁵ John J. Lentini, *ASTM Standards for Forensic Sciences*, 40 J. FORENSIC SCI. 146, 146 (1995).

³⁶ Peterson & Murdock, *supra* note 33.

³⁷ Lentini, *supra* note 35, at 146.

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more scientific approaches to the field even then. More than a third of the first twenty standards promulgated by committee E-30 and its subcommittees³⁸ by 1995 dealt with fire investigation. The source of those standards, as Lentini indicates in his article,³⁹ were standards put forth in 1988 by the International Association of Arson Investigators (IAAI), of which Lentini was a member. The IAAI was not a standards development organization, and the advantages of obtaining ASTM status and blessing for their product were obvious.

It is of some interest that an early adopter of the rejuvenated ASTM process for setting standards was forensic document examination.⁴⁰ Perhaps it was because there was a forensic document examination subcommittee in the original non-functioning ASTM E-30. Perhaps it was because forensic document examination came under significant criticism for weak validation in the late 1980s,⁴¹ and the attraction of having “an authoritative source to lend credibility to their conclusions” in Lentini’s words, was not lost on the practitioners of that discipline.

At any rate, upon reflection, it seems that a consensus standards promulgation process as it is normally conceived did not fit the context of forensic science very well, and certainly not as the ASTM E-30 and its subcommittees functioned in the 1990s, and even beyond. In most standard-setting processes, the overarching assumption is that they are best when emerging from a process where representatives of all “stakeholders” are assembled in a group and must dicker over the contours of the standard in issue. This approach assumes that there will be a variety of interested parties involved in the process, some of whom would benefit from laxer standards, and some from stricter standards, but all of whom have an interest in *some* standard emerging to establishing a baseline for the practice of the mutually dependent enterprises of the stakeholders.⁴² The members of these forensic

³⁸ At the time of Lentini’s article, there were three area subcommittees of E-30; Criminalistics (E-30-01), Document Examination (E-30-02) and forensic engineering (E-30-03). *Id.* at 146–47.

³⁹ *Id.* at 146.

⁴⁰ Andrew Sulner, *Critical Issues Affecting the Reliability and Admissibility of Handwriting Identification Opinion Evidence—How They Have Been Addressed (or Not) Since the 2009 NAS Report, and How They Should Be Addressed Going Forward: A Document Examiner Tells All*, 48 SETON HALL L. REV. 631 (2018) (“The FDE community has been more active than any other forensic discipline in producing professional standards, having published an array of twenty-one standards through ASTM International, a private consensus standards development organization (SDO).”).

⁴¹ D. Michael Risinger et al., *Exorcism of Ignorance as a Proxy for Rational Knowledge: The Lessons of Handwriting Identification ‘Expertise,’* 137 U. PA. L. REV. 731 (1989).

⁴² This is best illustrated, perhaps, by a simple example involving standards for the tensile strength of iron bars used to reinforce concrete (“rebar”). Manufacturers of such bars have an interest in a lower standard, and, therefore, an easier-to-produce and cheaper product. Immediate “consumers” who produce reinforced concrete works of various sorts have an

subcommittees might have some conflicting interests (government employed practitioners vs. private practitioners, for instance), but they were all members of the same guild group with group commitment to the general validity of the enterprise as practiced by the best practitioners. Absent were representatives of the consumers of their product (the courts and the criminal defendants who were the ultimate parties forced to consume the product), or of the general public interest, or of neutral scientists who were committed to the notion of validity generally.

As the ASTM process proceeded through the mid-1990s, two major events impacted the state of play. One was, of course, *Daubert* and its progeny. The other was the Justice Department's/FBI's response to *Daubert*, and also to the weaknesses in the FBI laboratory revealed by the Inspector General's report on the explosives testimony in the first World Trade Center bombing.⁴³ That response was to establish "technical working groups" (TWGs) in virtually every forensic area to propose improvements to practice (or to bless current practice, depending on your perspective). This effort got started in 1995, and by the time the name of these bodies was changed from TWGs to "scientific working groups" (SWGs) in 1998 (in what appeared to some a cold-blooded public relations gambit), the main action in most areas had moved from the ASTM to the SWGs. The membership in ASTM subcommittees was essentially voluntary and open, but the membership in the SWGs was controlled by the FBI and dominated by practitioners from government labs, and was thus even less representative than the ASTM subcommittees. SWG products might be high quality, but an SWG was not an SDO, so its products did not result in anything but a more or less respected opinion by the SWG. At some point the SWG products were sometimes subject to attempts to run them through "standards developing organizations" (SDOs) like ASTM, but not always. In any event, all of the loci of professional and standards-developing reflection upon

interest in higher standards as long as the standards are not so demanding as to drive up the prices to a point of diminishing demand, and therefore profits. Consumers of the product of the reinforced concrete industry may have an interest in even stronger standards for ensuring the longevity, and sometimes the safety, of the products they acquire (concrete roadbeds, pre-cast concrete members in the construction of bridges or buildings, etc.). The public interest may be represented by various government entities who are themselves consumers of the product for public works. The assumption of the "consensus standard" standard setting process is that if you get a properly selected and balanced representation of all stakeholders, and therefore all competing interests, into a room (literally or virtually) over a long enough period of time, a standard will emerge that will appropriately take into account and balance all interests. It is further assumed that this standard will thereafter become formally binding in specific circumstances by being incorporated into contracts, and indeed, become more generally binding by becoming industry practice, and therefore being available to courts to resolve various disputes in litigation even when not specifically referenced in a contract.

⁴³ See generally JOHN F. KELLY & PHILLIP K. WEARNE, *TAINING EVIDENCE: INSIDE THE SCANDALS AT THE FBI CRIME LAB* (1998).

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various forensic science areas and techniques were concentrated among forensic science practitioners until the coming of the NAS committee in 2006 and the issuance of their report in 2009.

A. *Standards as Regulation*

Can we optimistically hope that standard-setting, in the form of OSAC, can make significant progress toward the reform of American forensic science? The sociology of standards and standardization gives reasons for both pessimism and optimism.

One thing that the sociology of standards makes clear is that standard-setting is far from a uniform activity. Standard-setting takes many forms, sets out with a variety of goals, and follows contingent pathways.⁴⁴

Not all standardization is aimed at reform. The goals of standardization can range from aspirational to reflecting the status quo: “standards can imply a lowest common denominator of available options, the power of the strongest party in standardization, a negotiated order among some or all stakeholders, or a confirmation of how things are done by most parties.”⁴⁵ A key question that has emerged already is whether the OSAC standards should be aspirational—should articulate where we want forensic science eventually to be—or should reflect the status quo. It has already become clear that, while forensic reformers may assume that OSAC standards should be aspirational, others believe they should reflect the current practice.

B. *Committee Composition*

Sociologies of standards note, not surprisingly, that the composition of committees can affect the outcome of standardization activities. “Standardization by committee leads to compromises, bitterly contested power plays, and negotiations . . . [s]imilarly, a strong personality can influence the creation of standards,” and, “[t]he composition of standard committees inevitably creates an institutional bias.”⁴⁶ The composition of committees has already emerged as an issue for OSAC. The process began with a clear quota system for assembling committees: 70% practitioners, 20% researchers, and 10% “R&D partners,” which presumably means industry.⁴⁷ Already, this might be thought to heavily favor practitioners who may be oriented toward the aforementioned status quo. For example,

⁴⁴ Timmermans & Epstein, *supra* note 30, at 70.

⁴⁵ Timmermans & Epstein, *supra* note 30, at 79.

⁴⁶ Timmermans & Epstein, *supra* note 30, at 77.

⁴⁷ Org. for Scientific Area Comms. (OSAC) for Forensic Sci., *Terms of Reference for the Scientific Area Committee (SAC) Subcommittees, Version 1.3* (Jan. 18, 2017), https://workspace.forensicosac.org/higherlogic/ws/public/download/7797/FSSB_OSAC%20ToR%20SAC%20SC_V1.3.pdf [<https://perma.cc/C38B-WRLS>].

PCAST commented: “OSAC’s membership includes relatively few independent scientists: it is dominated by forensic professionals, who make up more than two thirds of its members . . . PCAST concludes that OSAC lacks sufficient independent scientific expertise and oversight to overcome the serious flaws in forensic science.”⁴⁸ However, there is also some evidence that some of the subcommittees had not even followed the quota system in their composition. This may have skewed subcommittees even further toward practitioners.⁴⁹ Similar complaints have been made about the composition of the American of Academy of Forensic Science Standards Board (ASB) Consensus Bodies, which comprise the SDOs for OSAC.⁵⁰

The ASB uses a more differentiated set of occupational categories called “Interest Categories”:

- User/Government
- User/Industry
- Producers
- Laboratories and/or Testing Facilities
- Consumer Groups
- Academia
- Subject Matter Experts
- General Interest⁵¹

This list certainly seems to reflect a more generic understanding of a properly composed standardization committee than OSAC’s, which seems specifically directed at forensic science. The ASB procedures do not specify quotas for each interest group, but rather state that no interest group should have more than one third of the members of the body.⁵²

⁴⁸ President’s Council of Advisors on Sci. & Tech., *supra* note 8, at 126.

⁴⁹ The two DNA subcommittees show that the “DNA2” subcommittee does indeed have 20% researchers, but the “DNA1” subcommittee has only three (15%) academic researchers, as well as three current or former practitioners who are classified as researchers in part. John M. Butler, The National Commission on Forensic Science and the Organization of Scientific Area Committees, Address at Proceedings of the International Symposium on Human Identification (2014). In 2014, I complained to the Pattern Evidence Scientific Area Committee that they had not even followed the quota system in the composition of the subcommittee in which I had the greatest interest: the Friction Ridge Subcommittee. The result of this misallocation was that the friction ridge OSAC had even fewer academic credentials than the friction ridge SWG, SWGFAST. And, recall that, as noted above, the SWGs were even less representative than the ASTM committees.

⁵⁰ Letter from Andrew Sulner, Principal Owner, Forensic Document Examinations, LLC to Steve Orthey (July 14, 2009) (on file with the author).

⁵¹ See AAFS Standards Bd., *ASB Interest Categories* (2017), https://asb.aafs.org/wp-content/uploads/2017/07/Interest_Categories2017.pdf [<https://perma.cc/JL4H-Y7UE>].

⁵² Am. Acad. of Forensic Sci., *Procedures for the Development of American National*

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A quick glance at the available openings on all the Consensus Bodies shows that Consumer Groups is the most under-filled interest category, followed by Producers.⁵³ This raises the interesting question of who a “consumer” of forensic science is. The official definition is: “[g]roups, individuals, and organizations representing consumer interests including safety, health, and environment. ‘Consumer’ may also be interpreted to include any party in judicial proceedings that may include forensic evidence.”⁵⁴

Some Consumer Group slots are filled by prosecutors. It is reasonable, and arguably correct, to describe prosecutors (and judges) as “consumers” of forensic science. More broadly, the definition might also include defense attorneys, innocence advocates, crime victims, or victims of erroneous forensic evidence, and perhaps even journalists.

Interestingly, one of the other “Potential Concepts for OSAC 2.0” directly addresses the issue of committee composition. The title of this concept, “Community-based standards,” sounds at first as if it is intended to root standards even more deeply into the practitioner community.⁵⁵ But a closer look reveals that this concept proposes to replace the current structure, which combines stakeholders on committees and subcommittees (although in such a way that practitioners have a supermajority), with a structure in which stakeholders are segregated into different committees. Specifically, practitioners would populate the Scientific Area Committees (SACs), and “scientists” would populate the Forensic Science Standards Board (FSSB). This proposal perhaps reflects disenchantment with the idea that bringing stakeholders together will stimulate progress. It perhaps suggests that some believe that practitioners and “scientists” are far enough apart in orientation that they need to perform separate tasks.

It is also worth noting that, given the fanfare surrounding the launch of OSAC, a rather formidable hierarchy of committees and subcommittees populated by more than 500 individuals total, many (myself included) were surprised to learn that OSAC was not going to create standards. This, as it turns out, is a consequence of the fact that NIST, despite its name, is not itself a standard-setting organization. The OSACs, in turn, are not what are called in the trade SDOs. So, an appropriate SDO was sought and found in

Standards, at 2 (2016), <https://asb.aafs.org/wp-content/uploads/2016/01/ASB-Procedures.pdf> [<https://perma.cc/DZT3-PPEY>].

⁵³ AAFS Standards Bd., Am. Acad. of Forensic Sci., *Proceedings of the American Academy of Forensic Sciences, 68th Annual Scientific Meeting* (2016), <https://asb.aafs.org/consensus-bodies/> [<https://perma.cc/V3WF-RK5E>].

⁵⁴ AAFS Standards Bd., Am. Acad. of Forensic Sci., *ASB Interest Categories* (2016), <https://asb.aafs.org/wp-content/uploads/2016/02/ASB-Interest-Categories.pdf> [<https://perma.cc/2HLT-SCF7>].

⁵⁵ Nat’l Inst. of Standards & Tech., *supra* note 25.

the American Academy of Forensic Sciences Academy Standards Board (ASB). And so, the OSACs are committees that will write proposed standards documents and submit them to the ASB. But so can anyone else. Anyone can submit a proposed standard to the ASB. Hence, perhaps, the rethinking entailed by OSAC 2.0.

C. Implementation

Regardless of the composition of committees and the content of standards, their enforcement and adoption is far from assured. Given all the energy that was invested in the NCFSS, it is shocking to realize how weak its enforcement power was: non-binding recommendations to the Deputy Attorney General to require things of DOJ laboratories only. It had no power whatsoever over laboratories in other jurisdictions, where most forensic science still occurs.

Sociologists of standards note that “the power of standardization depends on whether standards are actually implemented.”⁵⁶ One might adapt for standards the old adage about academic articles: most are never cited; many are never read. Likewise, “[c]ountless standards do nothing.”⁵⁷ Sociologists point out that “the world is awash in competing standards,” and, therefore, “standards risk remaining paper tigers unless they are widely adopted.”⁵⁸ They add that “[t]he voluntary nature of many standards makes it difficult to develop momentum unless built-in incentives promote compliance.”⁵⁹ These incentives might range from government requirements to peer pressure—a “crowd effect.”⁶⁰ It is possible to imagine both of these incentives having an effect on forensic science—governments requiring crime laboratories to conform to standards, or crime laboratories conforming to standards because most of their peers do—but it is at least equally possible that these incentives are *not* effective.

Thus, even if the OSAC standardization process goes well, implementation is an open question. 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.”⁶¹ These may be

⁵⁶ Timmermans & Epstein, *supra* note 30, at 79.

⁵⁷ Timmermans & Epstein, *supra* note 30, at 81.

⁵⁸ Timmermans & Epstein, *supra* note 30, at 79.

⁵⁹ Timmermans & Epstein, *supra* note 30, at 79.

⁶⁰ Timmermans & Epstein, *supra* note 30, at 79.

⁶¹ Timmermans & Epstein, *supra* note 30, at 81.

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discouraging thoughts for those hopeful about the OSAC standards.

Of course, sociologists of standards point out that there are many ways for standards to “succeed.” Some succeed entirely through persuasive, rather than legal or market, force. Some succeed symbolically; no one really follows the standards, but they change the conversation.⁶² Some, it is claimed, succeed through their very flexibility, by being broad enough to accommodate various and changing practices.⁶³

Has NIST’s OSAC assembled just the right mix of scientific firepower, practitioner buy-in, consumer pressure, government power, cultural change, and persuasive force to be the entity that finally reforms forensic science? We cannot yet know, but the task is daunting.

⁶² Timmermans & Epstein, *supra* note 30, at 82.

⁶³ Timmermans & Epstein, *supra* note 30, at 81.