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## ACCULTURATING FORENSIC SCIENCE: WHAT IS 'SCIENTIFIC CULTURE', AND HOW CAN FORENSIC SCIENCE ADOPT IT?

Simon A. Cole

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## ACCULTURATING FORENSIC SCIENCE: WHAT IS ‘SCIENTIFIC CULTURE’, AND HOW CAN FORENSIC SCIENCE ADOPT IT?

*Simon A. Cole\**

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### INTRODUCTION

The topic for this special issue is: “How should judges, legislators, and the legal community in general respond to the National Research Council of the National Academy of Sciences’ 2009 report, *Strengthening Forensic Science in the United States?*” I think there are some fairly easy answers to this question that should not brook a great deal of controversy or disagree-

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\* Associate Professor of Criminology, Law & Society, University of California, Irvine; Ph.D. (Science & Technology Studies), Cornell University; A.B., Princeton University. For discussion, references, and advice, I am grateful to Douglas Haynes, Sheldon Greenfield, Shobita Parthasarathy, Adina Schwartz, Gary Edmond, Beth Bechky, Roger Koppl, Corrina Kruse, Birgitta Rasmusson, Carroll Seron, Laura Kelly, and Norah Rudin. Thanks also to the student editors at Fordham Law School for their hard work on this Article. Some of my ideas for this Article were developed during a joint research visit to the Swedish National Forensic Laboratory and Linköping University. I am grateful to both institutions for their generosity and hospitality. This material is partially based upon work supported by the National Science Foundation under grant No. SES-0115305. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author and do not necessarily reflect the views of the National Science Foundation.

ment. First, validation studies<sup>1</sup> should be performed for forensic assays<sup>2</sup> for which they have not yet been performed.<sup>3</sup> Although the NAS Report is fairly clear about the absence of validation studies, some controversy remains over whether validation studies have been performed for some assays. Therefore, Dr. Bohan suggests that “validation investigations” should be performed to assess the state of validation of each assay.<sup>4</sup> These might be followed by validation studies.

Second, the proposed National Institute of Forensic Science (NIFS) should be created.<sup>5</sup> Certainly, there are potential downsides and criticisms, but creating NIFS is probably better than the status quo. If created, the Institute should be in the form proposed by the NAS Report.<sup>6</sup> Crucial aspects of this form include that it should be an independent agency, especially independent of law enforcement, and a proper scientific organization staffed by scientists. If it is “captured”<sup>7</sup> by law enforcement, it becomes less obvious that it would be a force for improvement rather than stagnation.

Third, the reporting of forensic analyses results should be reformed and standardized such that they are scientifically supportable.<sup>8</sup> Judges should restrict the admissibility of forensic assays that lack the aforementioned validation.<sup>9</sup>

Finally, judges, legislators, and the legal community should contemplate the broader meaning of the NAS Report’s conclusion that the courts’ handling of forensic evidence over the past couple of decades has been “utterly ineffective.”<sup>10</sup> Setting forensic evidence aside, what weaknesses in our current system of justice does this “utter ineffectiveness” identify? Re-

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1. “[A] validation study is designed to measure the accuracy of a scientific technique. The Study attempts to identify and quantify the inherent margin of error in the technique.” Edward J. Imwinkelried, *Coming to Grips With Scientific Research in Daubert’s “Brave New World”: The Courts’ Need to Appreciate the Evidentiary Differences Between Validity and Proficiency Studies*, 61 BROOK. L. REV. 1247, 1254 (1995) (internal citations omitted).

2. An “assay” might also be called a “forensic test” or a “forensic technique,” and would include, inter alia, tests of techniques involving the comparison of latent prints, firearms and tool marks, handwriting identifications, and bite marks.

3. NAT’L RES. COUNCIL, NAT’L ACADEMY SCI., *STRENGTHENING FORENSIC SCIENCE IN THE UNITED STATES: A PATH FORWARD* 22 (2009) [hereinafter NAS REPORT].

4. Thomas L. Bohan, *Strengthening Forensic Science: A Way Station on the Journey to Justice*, 55 J. FORENSIC SCI. 5, 6 (2010).

5. NAS REPORT, *supra* note 3, at 19.

6. *Id.*

7. Roger Koppl, *Who Will Capture Forensic Science?*, THINKMARKETS (Apr. 6, 2009), <http://thinkmarkets.wordpress.com/2009/04/06/who-will-capture-forensic-science/#more-1289>.

8. NAS REPORT, *supra* note 3, at 21.

9. *Id.* at 85-86.

10. *Id.* at 109.

forms to the justice system should be enacted so that in the future, courts' handling of scientific issues is less likely to become a glaring embarrassment to the legitimacy of the courts.

These are all things that I think many scholars will agree *should* be done. Whether they are likely to be done and what is the best strategy to assure that they are done will command somewhat less consensus and may well be the subject of other contributions to this special issue. Indeed, in some cases, compelling arguments may be made that half-hearted, ill-conceived attempts to implement some of these reforms may end up making the situation worse rather than better.

All that being said, however, simply asserting that the recommendations of the NAS Report should be followed seems to avoid a much larger issue. An NAS Report is, after all, a highly inefficient, expensive, and slow way of accomplishing tasks. It is also "one-off," in the sense that we cannot realistically expect there to be periodic NAS Reports indicating what needs to be done in forensic science, especially given the well known difficulties there were in bringing this particular NAS Report into being.<sup>11</sup> The larger issue is why the aforementioned actions never occurred in the first place. In other words, why was it necessary for the National Academy of Sciences (NAS) to intervene in 2009 to demand that validation studies be conducted a century after the introduction of latent print evidence into court,<sup>12</sup> nearly eighty years after the introduction of firearms and tool mark evidence,<sup>13</sup> nearly fifty years after the introduction of bite mark evidence,<sup>14</sup> and more than a century after the introduction of handwriting evidence?<sup>15</sup> Why did the NAS Report have to state that forensic science should be treated as science and not as an arm of law enforcement,<sup>16</sup> or call for the reporting of forensic results to be both standardized and scientifically supportable?<sup>17</sup> Why was it necessary for the NAS to suggest that forensic science be better regulated, or that accreditation of laboratories and certification of analysts be mandatory?<sup>18</sup>

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11. Donald Kennedy, Editorial, *Forensic Science: Oxymoron?*, 302 SCIENCE 1625 (2003).

12. Simon A. Cole, *Grandfathering Evidence: Fingerprint Admissibility Ruling From Jennings to Llera Plaza and Back Again*, 41 AM. CRIM. L. REV. 1189, 1216-17 (2004).

13. MODERN SCIENTIFIC EVIDENCE: THE LAW AND SCIENCE OF EXPERT TESTIMONY (David L. Faigman et al. eds., 3d ed. 2007).

14. *Id.*

15. *Id.*

16. NAS REPORT, *supra* note 3, at 19.

17. *Id.* at 21.

18. *Id.* at 23.

If we pose the questions this way, then the answer to the question, “what is to be done?” takes on a different tone. In addition to the short term actions listed above, a long-term structural solution is required to ensure that past mistakes are not repeated regardless of NAS intervention. Again, NAS intervention is an extremely costly, inefficient, time-consuming, and unrealistic mechanism for governing an area of scientific activity. What is wanted is for forensic science to become self-governing in such a way that future NAS Reports become unnecessary and that future outside observers will not find lacunae quite as great as the ones discovered by this NAS Committee (validation, accreditation, certification, standardization of testimony, and so on). The hope then would be that fifty years from now, rather than convening another NAS committee which finds new deficiencies in forensic science (or, worse, finds that the existing “serious deficiencies in the nation’s forensic science system”<sup>19</sup> have still not been addressed), those actions that a future NAS committee would expect to have occurred will have occurred naturally.

The NAS Report does not focus very much on the “why” questions, but, to the extent that it does, its discussion is centered around what it calls the “culture of science.”<sup>20</sup> The Report describes the “culture of science” as an important missing ingredient in at least parts of forensic science.<sup>21</sup> It states that “some . . . activities” that fall under the broad rubric of “‘forensic science’ . . . are not informed by scientific knowledge, or are not developed within the culture of science.”<sup>22</sup> Further, it touts “scientific culture” as a potential antidote to what one of the Committee co-chairs called elsewhere “The Problems That Plague the Forensic Science Community.”<sup>23</sup> “The forensic science disciplines will profit enormously by full adoption of this scientific culture.”<sup>24</sup> Moreover, the Report asserts that “a culture that is strongly rooted in science” is a “minimum” criterion for the new federal agency it proposes, the National Institute of Forensic Science (NIFS).<sup>25</sup> It

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19. News Release, Nat’l Academies, ‘Badly Fragmented’ Forensic Science System Needs Overhaul: Evidence to Support Reliability of Many Techniques is Lacking (Feb. 18, 2009), available at <http://www8.nationalacademies.org/onpinews/newsitem.aspx?RECORDID=12589>.

20. NAS REPORT, *supra* note 3, at 39.

21. *Id.*

22. *Id.*

23. *See id.* at 114, 125; Hon. Harry T. Edwards, Senior Circuit Judge and Chief Judge Emeritus, D.C. Cir., Keynote Address at 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).

24. NAS REPORT, *supra* note 3, at 125.

25. *Id.* at 18.

would seem, then, that in addition to following the recommendations of the NAS Committee, forensic science should adopt scientific culture, and this Article will focus on that question.

In focusing on this issue, I believe that I have taken on a more difficult task than, arguing, as I have elsewhere,<sup>26</sup> that validation studies of forensic techniques should be conducted or even that evidence should not be admissible without such validation studies.<sup>27</sup> There are vigorous debates about what “validation” means,<sup>28</sup> and since the NIFS has not yet been created, there are disagreements about what “it” is.<sup>29</sup> Even so, validation, the NIFS, and standardized testimonial reporting are concrete things 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. Turning to the scholarly literature is of little help. “Scientific culture” is a rather vague and contested term that is used to mean a variety of different things.<sup>30</sup> If no one agrees upon what we mean by “scientific culture,” then the NAS Report’s call to “adopt” it becomes an empty rhetorical gesture, easily answered by any interest group that simply chooses its preferred definition of “scientific culture” and declares that forensic science has or has not adopted it. I will argue, however, that all is not lost merely because of the vagueness of “scientific culture” and the NAS Report’s discussion of it. To the contrary, 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 articulated above are to occur.

I should note that, while I will be critical of the NAS Report’s treatment of the notion of “scientific culture,” this should not be construed as criticism of the Report as a whole or of the NAS Committee. In my view, the

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26. See Simon A. Cole, *Comment on ‘Scientific Validation of Fingerprint Evidence Under Daubert,’* 7 LAW PROBABILITY & RISK 119 (2007); Simon A. Cole, *Is Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents’ Discourse,* 28 LAW & POL’Y 109 (2006).

27. See Cole, *supra* note 12.

28. See, e.g., Lyn Haber & Ralph Haber, *Scientific Validation of Fingerprint Evidence Under Daubert,* 7 LAW PROBABILITY & RISK 87, 88 (2008); Imwinkelried, *supra* note 1, at 1256-60.

29. See Imwinkelried, *supra* note 1, at 1256-60; Haber & Haber, *supra* note 28.

30. Compare Sarah Franklin, *Science as Culture, Cultures of Science,* 24 ANN. REV. ANTHROPOLOGY 163 (1995), with Benoit Godin & Yves Gingras, *What Is Scientific and Technological Culture and How Is it Measured? A Multidimensional Model,* 9 PUB. UNDERSTANDING SCI. 43 (2000).

Report's lucid discussion of such issues as validation and "individualization" far outweigh the ambiguities I will identify around the notion of "scientific culture." Indeed, it is partly because I agree so wholeheartedly with so much of what is elsewhere in the Report that I have chosen to engage in this extended attempt at developing further the Report's discussion of "scientific culture."

In Part I, I describe how the NAS Report characterizes "scientific culture." I suggest that the described attributes of scientific culture are vague and unspecific, and that more thought is necessary to elucidate how they might map onto forensic science. In Part II, I suggest that the NAS Report's characterization of "scientific culture" is based on popular accounts of science and "the scientific method." I suggest that these accounts are incomplete, generally considered obsolete, and not particularly helpful in pointing a way toward reform of forensic science. In Part III, I posit a conception of science as *work* rather than *method*. In Part IV, I offer a tentative mapping of how forensic science might be understood as work by dividing forensic labor in a set of general tasks. In Part V, I offer a tentative mapping of the goals and desired attributes of scientific workers who would perform each type of forensic task. In Part VI, I briefly describe how the status quo seems to fall short of the desired situation described in Part V. In Part VII, I suggest that medicine offers a reasonable analogy for the sort of structuring of labor into tasks that might be desirable for forensic science. I conclude with some observations and clarifications about the medical model I proposed.

It should also be noted that my comments about the state of forensic science are, like the NAS Report itself, exclusively concerned with forensic science as it exists in the United States. While many of the issues identified by the NAS Report (i.e., validation) transcend national borders, many others (i.e., standardization) do not. While the NAS Committee was obviously limited in terms of what it could accomplish given its constraints on time and resources, the NAS Report can be reasonably criticized for neglecting to look outside the United States for potential solutions to the problems it identified. Indeed, some of my prescriptive remarks are drawn from my limited knowledge of practices outside the United States.

#### I. THE NAS REPORT'S TREATMENT OF SCIENTIFIC CULTURE

The NAS Report's discussion of scientific culture is contained almost entirely in Chapter 4, "Principles of Science and Interpreting Scientific Data."<sup>31</sup> In this chapter, the report offers the sort of account of "the scientific

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31. NAS REPORT, *supra* note 3, at 111.

method” often written by non-philosophers, such as journalists, lawyers, policy-makers, or scientists themselves.<sup>32</sup> The account draws heavily on Karl Popper’s theory of falsifiability.<sup>33</sup> According to the Report, there is a single scientific method called “the scientific method.”<sup>34</sup> The method consists of posing hypotheses and measuring them against data, resulting in either refutation or support. “Absolute truth” is never achieved, but this process of continual testing “approaches truth . . . incrementally.”<sup>35</sup>

Acceptance of the work comes as results and theories continue to hold, even under the scrutiny of peers, in an environment that encourages healthy skepticism. That scrutiny might extend to independent reproduction of the results or experiments designed to test the theory under different conditions. As credibility accrues to data and theories, they become accepted as established fact and become the “scaffolding” upon which other investigations are constructed.<sup>36</sup>

The NAS Report also offers an account of the “principles of science” that consists of another common attribute of such accounts of science: a recitation of supposed virtues common to scientists and the scientific way of thinking.<sup>37</sup> A large body of thought has historically attributed the material and epistemological success of science to a particular virtuous way of thinking. Such arguments have a long history dating back at least as far as Sir Francis Bacon.<sup>38</sup> In their most modern form, however, they tend to be associated with the sociologist Robert Merton, who famously articulated four “norms,” associated with the profession of science, although Merton treated these attributes as social norms, not epistemological virtues.<sup>39</sup> Merton’s four norms were: communism (collective ownership of data and knowledge), universalism (scientific knowledge is “impersonal,” in the sense that its truth value has nothing to do with the personal attributes of the individuals who develop it), disinterestedness (lack of interest in any particular outcome of the search for scientific truth), and organized skepticism (the collective subjection of all ideas to the highest level of scrutiny).<sup>40</sup>

The NAS Report implicitly invokes all four of Merton’s norms in its description of how “scientific culture” ought to function. Theories and data

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32. NAS REPORT, *supra* note 3, at 112.

33. KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* 57-74 (1959).

34. NAS REPORT, *supra* note 3, at 112.

35. *Id.*

36. *Id.*

37. *Id.*

38. *See generally* FRANCIS BACON, *THE NEW ORGANON* (2000).

39. *See* ROBERT K. MERTON, *SOCIAL THEORY AND SOCIAL STRUCTURE: TOWARD THE CODIFICATION OF THEORY AND RESEARCH* (1949).

40. *Id.*

should be shared through “collegial interactions” (communism).<sup>41</sup> “The need for credibility among peers drives investigators to avoid conflicts of interest” (disinterestedness).<sup>42</sup> Science should be characterized by “openness to new ideas.”<sup>43</sup> “The scientific culture encourages continued questioning and improvement.”<sup>44</sup> “A scientist encounters . . . unconscious bias if he/she becomes too wedded to a preliminary conclusion, so that it becomes difficult to accept new information fairly and unduly difficult to conclude that the initial hypotheses were wrong.”<sup>45</sup> Science should be “a self-correcting enterprise.”<sup>46</sup>

Science is characterized also by a culture that encourages and rewards critical questioning of past results and of colleagues [organized skepticism]. . . . The scientific culture encourages cautious, precise statements and discourages statements that go beyond established facts; it is acceptable for colleagues to challenge one another, even if the challenger is more junior [universalism].<sup>47</sup>

Anecdotal arguments may be made that forensic science, as currently constituted in the United States, demonstrates broad failures of all four norms. Communism is violated by refusals to share data, both broad validation data and specific data from specific cases requested through discovery.<sup>48</sup> Universalism is violated by the tendency toward *ad hominem* attacks on those who challenge the conventional wisdom and the extreme hostility expressed toward outsiders.<sup>49</sup> Disinterestedness is violated by the location

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41. NAS REPORT, *supra* note 3, at 112.

42. *Id.*

43. *Id.* at 113.

44. *Id.* at 114.

45. *Id.* at 123-24.

46. *Id.* at 125.

47. *Id.*

48. See, e.g., D. Michael Risinger & Michael J. Saks, *A House With No Foundation*, 20 ISSUES IN SCI. & TECH. 35 (2003); William C. Thompson, *Evaluating the Admissibility of New Genetic Identification Tests: Lessons From the ‘DNA War,’* 84 J. CRIM. L. & CRIMINOLOGY 22, 99 (1993).

49. See, e.g., David L. Grieve, *Simon Says*, 51 J. FORENSIC IDENTIFICATION 85 (2001); André Moenssens, *Fingerprint Identification: A Valid Reliable “Forensic Science”?*, 18 CRIM. JUST. 31 (2003); André Moenssens, *Handwriting Identification Evidence in the Post-Daubert World*, 66 UMKC L. REV. 251 (1997); Leslie Roberts, *Prosecutor v. Scientist: A Cat and Mouse Relationship*, 257 SCIENCE 733 (1992); Thompson, *supra* note 48, at 100-02; Gina Kolata, *Critic of ‘Genetic Fingerprinting’ Tests Tells of Pressure to Withdraw Paper*, N.Y. TIMES, Dec. 20, 1991, at A20; Ronald Nichols, *The Scientific Foundation of Firearms and Tool Mark Identification—A Response to Recent Challenges*, CAL. ASS’N CRIMINALISTS NEWS (2006), at 8-27; Glenn Langenburg, *Defending Against the Critic’s Curse*, DETAIL (Sept. 30, 2002), <http://www.clpex.com/Articles/CriticsCurse.htm>; André Moenssens, *The Reliability of Fingerprint Identification: A Case Report* (2002), <http://www.forensic>

of most forensic science in law enforcement agencies associated with a particular “side” of the criminal trial.<sup>50</sup> Organized skepticism is violated by the strong resistance to challenging cherished assumptions.<sup>51</sup>

I would like to note that we could also find similar anecdotes about mainstream science. More importantly, even in “normal” scientific practice, Merton’s norms appear to function more as ideals to which to aspire than as determinants of actual behavior. Indeed, one particularly well known empirical study of a set of actual (normal, respectable, non-deviant) scientists involved in a scientific controversy found that, rather than behaving according to Merton’s norms, they behaved in precisely the opposite fashion, which were referred to as “counter-norms.”<sup>52</sup> These counter-norms were not viewed as deviant behavior on the part of the scientists. Interview data showed that most scientists expected one another to behave in this manner.<sup>53</sup> For example, the notion that real scientists would adhere to dispassion, rather than function as advocates for their chosen theories was regarded by most interview subjects as hopelessly naïve.<sup>54</sup> The purpose of this discussion is not to claim that forensic science is “pathological science.”<sup>55</sup> Rather, the point is to draw attention to the extent to which violations of at least some of Merton’s norms appear to be openly and deliberately espoused, rather than covertly and inadvertently practiced, and even embedded in the “culture” of forensic science.

The NAS Report’s recitation of Popper and Merton is a familiar enough attribute of lay accounts of what science is and how scientists should be-

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evidence.com/site/ID/pollak2002.html (last visited Nov. 22, 2010); or almost anything published in CRIME LAB REPORT, <http://www.crimelabreport.com/>.

50. Paul Giannelli, *The Abuse of Scientific Evidence in Criminal Cases: The Need for Independent Crime Laboratories*, 4 VA. J. SOC. POL’Y & L. 439 (1997).

51. See, e.g., DAVID R. ASHBAUGH, QUANTITATIVE-QUALITATIVE FRICTION RIDGE ANALYSIS: AN INTRODUCTION TO BASIC AND ADVANCED RIDGEOLOGY 4 (1999) (“In the past, the friction ridge identification science has been akin to a divine following. Challenges were considered heresy and challengers frequently were accused of chipping at the foundation of the science unnecessarily. This cultish demeanor was fostered by a general deficiency of scientific knowledge, understanding, and self-confidence within the ranks of identification specialists.”); see also David L. Grieve, *Possession of Truth*, 46 J. FORENSIC IDENTIFICATION 521, 528 (1996) (“[T]he assumption of absolute certainty as the only possible conclusion has been maintained by a system of societal indoctrination, not reason, and has achieved such ritualistic sanctity that even mild suggestions that its premise should be re-examined are instantly regarded as acts of blasphemy.”).

52. Ian I. Mitroff, *Norms and Counter-Norms in a Select Group of the Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists*, 39 AM. SOC. REV. 579, 587-89 (1974).

53. *Id.* at 589.

54. *Id.*

55. Irving Langmuir, *Pathological Science*, 42 PHYSICS TODAY 36 (1989).

have. But the mere recitation of these epistemological virtues is not enough. How are hypothesis testing and organized skepticism, for example, to be operationalized in everyday forensic practice? That is not clear. In the next section, I will argue that the reason it is not clear is because the NAS Report, like many accounts of science, equates “science” with what I will call “discovery science,” while paying little attention to large swaths of scientific practice that cannot be characterized as discovery.

## II. SCIENCE AND SCIENTIFIC METHOD

The lack of precision around the term “scientific culture” is closely related to the lack of precision around other honorific terms, such as “science” and “the scientific method.”<sup>56</sup> I will argue that there are definitional confusions around all three terms. Moreover, I will suggest that, for all three terms, these confusions are exacerbated by a tendency to equate “science” with what I will call “discovery science”—scientific activity designed to create new, generalizable knowledge about the natural world. In fact, “science” is a far more rich and more varied enterprise than just “discovery science.” There are a wide variety of activities that we conventionally consider “science” that would not be described as “discovery science.” These activities would include: routine laboratory work, such as work that may be part of larger projects that may themselves be “discovery science”; industrial science; engineering; much of medicine; descriptive science; and so on. There are armies of workers engaged in what sociologists call “technoscientific work” in the enterprise of modern science, who are performing work that could not, in itself, be characterized as discovery.<sup>57</sup> They are manipulating cell cultures, synthesizing molecules, tuning detectors, analyzing samples, and so on.

When lay people, including lawyers and judges, write about “science,” however, they tend to write exclusively about “discovery science.”<sup>58</sup> This is perhaps because those who first produced public “accounts” of science—philosophers, historians, science journalists, and scientists themselves—were primarily interested in the making of new knowledge about the natural world.<sup>59</sup> Philosophers and historians of science, for example, were interested in how new scientific knowledge was made, not, for example, in what epistemological basis a laboratory technician had for performing a

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56. Gary Edmond & David Mercer, *Trashing ‘Junk Science’*, 1998 STAN. TECH. L. REV. 3, 28 (1998).

57. See generally BETWEEN CRAFT AND SCIENCE: TECHNICAL WORK IN U.S. SETTINGS (Stephen R. Barley & Julian E. Orr eds., 1997).

58. E.g., KARL R. POPPER, *THE LOGIC OF SCIENTIFIC DISCOVERY* (1959).

59. *Id.*

routine assay. Likewise, the educated public that consumed science journalism and memoirs of scientists were interested in discovery, not mundane laboratory work. Among lay people, these remain the dominant accounts of science.<sup>60</sup> Therefore, when lay people, including judges and attorneys, write or talk about science, they tend to write or talk about discovery science, and they tend to use as authorities accounts that primarily focus on discovery science.<sup>61</sup>

The development of sociology and anthropology of science, however, brought attention to hitherto ignored areas of “mundane” scientific work. Sociologists and anthropologists are not only interested in discovery but also in what scientific practice was actually like *as work*.<sup>62</sup> They focused on what scientists actually did all day, rather than merely isolating those rare moments in which scientists discovered new knowledge.<sup>63</sup>

This is not to argue that sociology and anthropology are better than philosophy and history or vice versa. Rather, it is to argue that there are a variety of activities that encompass the enterprise we call “science” and there are a variety of accounts of these different activities. Lay people, like judges and lawyers, tend to be familiar only with accounts of discovery science and thus seek to apply those accounts to all scientific activities.<sup>64</sup> This results in a mismatch, which can produce a variety of misunderstandings. Mundane scientific activity can be characterized according to explanations that were constructed for discovery science and can be found wanting because it does not meet expectations that were devised for discovery science. Normatively, policy-makers may apply norms that may be appropriate for discovery science to mundane scientific activity. I will suggest that 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.

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60. Matthias Nuckles et al., *How Do Experts Adapt Their Explanations to a Layperson's Knowledge in Asynchronous Communication? An Experimental Study*, 16 USER MODELING & USER ADAPTED INTERACTION 2 (2006).

61. See Gary Edmond & David Mercer, *Conjectures and Exhumations: Citations of History, Philosophy and Sociology of Science in US Federal Courts*, 14 LAW & LITERATURE 309 (2002) [hereinafter Edmond & Mercer, *Conjectures and Exhumations*]; Edmond & Mercer, *supra* note 56.

62. E.g., ANDREW PICKERING, *SCIENCE AS PRACTICE AND CULTURE* (1992).

63. *Id.*

64. Edmond & Mercer, *supra* note 56.

### A. Science

The critique of forensic science found in the NAS Report is sometimes characterized in the popular media as a claim that forensic science is “unscientific.”<sup>65</sup> That is not, in fact, a claim made in the NAS Report. Nowhere does it say that forensic science is “not science.” Instead, the NAS opted for much more specific claims, such as that forensic science lacks adequate validation, certification, accreditation, oversight, and basic research, among other things.<sup>66</sup> The same is true of critiques published by forensic science reformers prior to the NAS Report.<sup>67</sup> These critiques, likewise, tended to focus on more specific issues than the broad-brush claim that forensic science was “not science.”<sup>68</sup>

There are good reasons for forensic science reformers to avoid making the charge that forensic science is “not science.” Supporting such a claim would require defining what science is and showing that all of forensic science falls outside that definition. Defining “science” and distinguishing it from “pseudo-science” is a problem that philosophers of science call “demarcation.”<sup>69</sup> Current philosophy of science views the demarcation problem as unsolved—that is, there is no single definition of “science” that neatly divides everything upon which we want to bestow the title “science” from everything upon which we don’t want to bestow that title.<sup>70</sup> The best known purported “solution” to the demarcation problem, Karl Popper’s notion of “falsification,”<sup>71</sup> is not viewed by most contemporary philosophers of science as a complete solution: there are areas of study that we generally consider “science” (descriptive biology, geology, etc.) that do not meet the criteria of “falsifiability.”<sup>72</sup> If there is no agreed upon demarcation criterion that neatly divides knowledge claims into “scientific” and “non-scientific” categories in a satisfactory way, then there is little to be gained from arguing about how such a demarcation criterion would apply to forensic science.

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65. See, e.g., Yamil Berard, *Stakes are High as Doubt Is Cast on Forensic Lab Techniques*, Fort Worth Star-Telegram, Nov. 28, 2009, available at <http://www.mcclatchydc.com/2009/11/29/79698/stakes-are-high-as-doubt-is-cast.html>.

66. NAS REPORT, *supra* note 3, at 12.

67. E.g., Haber & Haber, *supra* note 28.

68. *Id.*

69. POPPER, *supra* note 58, at 11.

70. A.F. CHALMERS, *WHAT IS THIS THING CALLED SCIENCE?* (1999).

71. POPPER, *supra* note 58, at 18 (this is the notion that you can never prove a theory true, but you can prove it false by a contravening example).

72. *Id.*; see also CHALMERS, *supra* note 70.

A second reason that there is little use in debating whether or not forensic science is “science,” is that it is legally irrelevant. In *Kumho Tire v. Carmichael*, the United States Supreme Court, perhaps in part out of recognition of the problems with demarcation described in the preceding paragraph, relieved courts of the responsibility to decide whether various forms of expert evidence should constitute “science” or “non-science.”<sup>73</sup> The Court ruled that the same criteria enumerated in its *Daubert v. Merrell Dow Pharmaceuticals, Inc.* decision applied equally to all forms of expert evidence.<sup>74</sup>

The *Daubert* ruling invoked “falsificationism,” which has led some to think that the Court adopted it as a demarcation criterion.<sup>75</sup> The Court, however, considerably leavened its invocation of falsification with discussions of alternative, some would say incompatible, markers of “science.”<sup>76</sup> The fabled “incoherence” of the *Daubert* decision has occasioned a great deal of criticism from scholars.<sup>77</sup> We need not rehash all of this criticism here; however, one issue is particularly pertinent to our discussion. In turning to philosophy of science in an attempt to articulate a definition of reliable science, the *Daubert* Court necessarily turned to a literature that is concerned with what we might call “discovery science.”<sup>78</sup> Philosophers, like those cited prominently in the *Daubert* decision, were concerned with the sort of science practiced by scientists who are trying to develop new knowledge about the natural world (e.g., discover the laws of physics, understand the evolution of species, etc.).<sup>79</sup> While this sort of activity captures the attention of philosophers, there is a great deal of activity upon which we would bestow the title of “science” that is nothing like “discovery science.” For example, laboratory technicians performing routine assays, industrial scientists seeking to refine a product or process, and even physicians trying to diagnose patients or engineers trying to design a safer bridge might defensibly be called “science,” and yet are probably not best described as efforts to discover generalizable truths about the natural world.

The *Daubert* decision is generally believed to have been occasioned, in part, by a perception that that the courts were facing an increasing number

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73. *Kumho Tire v. Carmichael*, 526 U.S. 137, 147-49 (1999).

74. See *Daubert v. Merrell Dow Pharm. Inc.*, 509 U.S. 579, 592-94 (1993).

75. Susan Haack, *An Epistemologist in the Bramble-Bush: At the Supreme Court With Mr. Joiner*, 26 J. HEALTH POL. POL'Y & L. 217 (2001).

76. *Id.* at 226.

77. *Id.* at 232-33.

78. See Edmond & Mercer, *Conjectures and Exhumations*, *supra* note 61, at 310-12 (2002); cf. POPPER, *supra* note 58.

79. POPPER, *supra* note 58, at 37-38.

of highly technical scientific issues.<sup>80</sup> *Daubert*, in this formulation, was an effort by the Court to assist the lower courts by providing guidance in dealing with such issues.<sup>81</sup> But most legal issues involving science do not seem to involve discovery science. There are few legal cases that hinge on, say, particle physics or string theory. Instead, most legal issues would seem to pertain to precisely those “other,” more mundane kinds of science: laboratory tests, medical diagnoses, epidemiology, and industrial science, among others. The paradox is that, in seeking to assist lower courts in dealing with a perceived flood of scientific issues involving what we might call “mundane science,” the Court drew on a literature constructed exclusively around the philosophical problems raised by discovery science. This mismatch has not been sufficiently recognized, either by the courts themselves or in the scholarly literature, but it surely is behind much of the sense of dissatisfaction that continues to surround the *Daubert* regime. This mismatch will be crucial to my argument in this Article.

### B. Scientific Method

While “science” may be too broad a concept to define precisely, it is sometimes argued that the “scientific method” might be a somewhat narrower, and thus more precisely definable, concept.<sup>82</sup> In defining the “scientific method, however, 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.<sup>83</sup> As one philosopher of science sums it up, “there is no scientific method.”<sup>84</sup> There is, therefore, very little purpose to arguing about whether forensic science uses the “scientific method” if not all areas of conventional science use that method.

It should also be noted, however, that discussions of “scientific method” also suffer from the same mismatch between “discovery science” and

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80. See generally PETER W. HUBER, GALILEO'S REVENGE: JUNK SCIENCE IN THE COURTROOM (1991).

81. JUSTICE STEPHEN BREYER, FED. JUDICIAL CTR., INTRODUCTION, in REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 2 (2000) (“The legal disputes before us increasingly involve the principles and tools of science.”).

82. See *infra* note 87.

83. Susan Haack, *Not Cynicism, but Synechism: Lessons From Classical Pragmatism*, 41 TRANSACTIONS CHARLES S. PEIRCE SOC'Y 239, 249 (2005).

84. *Id.*

“mundane science.” The mythical “scientific method” that looms so large in popular conceptions is associated with the Scientific Revolution and classical experiments designed to discover new facts about the natural world.<sup>85</sup> There is no reason that the method that Galileo or Newton used to develop new knowledge about physics should necessarily be used by a technician working in a molecular biology laboratory, a physician making a diagnosis, an industrial chemist seeking to improve a product or process, an engineer seeking to design a bridge, or a forensic scientist seeking to shed light on a criminal case. The fact that such scientific workers do not use “the scientific method” does not render what they do “not science,” or, even more importantly, wrong.

Nonetheless, there appears to be a widespread misconception that any activity that deserves to call itself “science” should be able to account for itself in terms of the “scientific method” of hypothesis testing.<sup>86</sup> This misconception has led to a number of recent efforts to shoehorn forensic science into this canonical “scientific method.”<sup>87</sup> These works all characterize the “scientific method” as a multi-step process focused on hypothesis testing. Consider, e.g., several efforts to argue that “ACE-V,” the acronym latent print examiners use to describe their supposed “method,”<sup>88</sup> is “synonymous” with “the scientific method.” The motivation to associate ACE-V with the honorific “scientific method” is clear: the “scientific method” and hypothesis testing supposedly “ensure[] that the conclusions are the best conclusions possible, given the available data” and make it “certain that the results are sound and supported.”<sup>89</sup> Best of all, “[t]he overwhelming popularity of hypothesis testing is due to the reliability of the results when the process is used correctly.”<sup>90</sup> Such arguments seem to take the following form:

- A. Use of the “scientific method” produces knowledge that is good, valid, and true.
- B. Forensic assay X follows the scientific method.
- C. Forensic assay X produces results that are good, valid, and true.

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85. CHALMERS, *supra* note 70.

86. *See infra* note 87.

87. Mark A. Acree, *What Is Science? The Dilemma of Fingerprint Science Revisited*, 14 PRINT 4 (1998); Michelle Reznicek et al., *ACE-V and the Scientific Method*, 60 J. FORENSIC IDENTIFICATION 87, 88 (2010); Michele Triplett & Lauren Cooney, *The Etiology of ACE-V and its Proper Use: An Exploration of the Relationship Between ACE-V and the Scientific Method of Hypothesis Testing*, 56 J. FORENSIC IDENTIFICATION 345, 345 (2006); Pat A. Wertheim, *Scientific Comparison and Identification of Fingerprint Evidence*, 16 PRINT 4 (2000).

88. Sandy L. Zabell, *Fingerprint Evidence*, 13 J.L. & POL'Y 143, 154 (2005).

89. Triplett & Cooney, *supra* note 87, at 346.

90. *Id.*

Both the premises and conclusions of this syllogism are wrong. It is not claimed that hypothesis testing necessarily produces true knowledge. Lots of statements have high truth value without using the “scientific method.” In short, it is neither necessary nor sufficient to show that latent print identification is hypothesis testing in order to show that it is valid or reliable. Even setting this point aside, however, these efforts seem misguided. In addition to some more minor misstatements, all of these models suffer from the flaw of conceptualizing the individual case as the hypothesis, rather than the validity of the overall assay. In other words, they conceptualize the null hypothesis as “the unknown friction ridge impressions come from the same source as those of the known print.”<sup>91</sup> This hypothesis is then supposedly tested by a comparison of details until the examiner decides that the null hypothesis can be rejected. This conclusion is then supposedly “confirmed” by the replication of this process by another examiner.

But this is not the question that scholars, lawyers, judges, and the NAS have been asking about latent print identification. The question that they have been asking is, “Is latent print identification valid?” Here the hypothesis would be that ACE-V correctly discriminates the true source of latent prints with X degree of accuracy. While not everything that we consider “science” is subject to hypothesis testing, *this* hypothesis is actually susceptible to experimental testing. Such experiments, however, have not been conducted.<sup>92</sup> Therefore, hypothesis testing is reconfigured as a process that applies to individual cases. This is a distortion of the notion of hypothesis testing as classically understood, which is aimed at testing “a general law, rather than a singular statement.”<sup>93</sup>

The misuse of this notion of hypothesis testing is demonstrated by the fact that the analogy chosen by the most recent of these articles concerns plant identification.<sup>94</sup> The authors suggest that the classification of an individual sample of plant as a particular species is an example of the “scientific-

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91. Reznicek et al., *supra* note 87, at 96.

92. See Haber & Haber, *supra* note 28; Simon A. Cole, *Is Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents' Discourse*, 28 LAW & POL'Y 109 (2006).

93. HILARY PUTNAM, MATHEMATICS, MATTER AND METHOD 265 (1979). It has been suggested that specific statements may be amenable to a specific type of hypothesis testing, through a Bayesian likelihood ratio approach. Although I am not sure that assessing the relative probabilities of competing hypotheses through a Bayesian approach should necessarily be characterized as “hypothesis testing,” it is sufficient here to note that none of the articles discussed *supra* adopt a Bayesian approach. Rather, they are conceptualizing “hypothesis testing” in an “old-fashioned” Popperian sense in which hypotheses are tested through “crucial,” potentially falsifying “experiments,” in which each successive observation is conceptualized as an “experiment.”

94. See Reznicek, *supra* note 87, at 87.

ic method” and “hypothesis testing.”<sup>95</sup> This is incorrect. The identification of an individual sample of plant as a particular species would be more appropriately characterized as the application of an accepted set of heuristics. The generalizable hypothesis about the natural world in this area of science would be something much broader, like “application of this set of taxonomic rules produces correct plant identifications at X rate.” In short, these authors have confused the task of analysis with the task of discovery research, and they treat an analytic activity as if it were an experimental activity.

These efforts seem misguided, not merely because they make erroneous assertions about the “scientific method,” and not merely because they seem to distort that “method” in their efforts to make it “fit” forensic science, but, more importantly, because they are fundamentally unnecessary. To answer the questions raised by the NAS Report, it is neither necessary nor sufficient to show that forensic science uses the mythical “scientific method.” Showing that forensic science uses “the scientific method” does not answer the NAS Report’s claims that it lacks validation, certification, accreditation, oversight, and basic research, nor does it answer the simpler, and yet crucially important question: how accurate is it? Likewise, forensic science could show that it *does* have validation, certification, accreditation, oversight, and basic research without showing that it uses the “scientific method.” Importantly, method is unrelated to validity and accuracy. There could be forensic assays that use the “scientific method” in the way that is laid out by these articles, and yet have very low accuracy, resulting in the seemingly paradoxical conclusion that the technique uses the “scientific method” and is usually wrong. Likewise, there could be techniques, perhaps some forensic techniques, that do not use the “scientific method” but are highly accurate.

### III. SCIENCE AS WORK

Scholars have already made the point that lawyers and judges tend to invoke philosophical and sociological models of science that, among professional philosophers and sociologists of science, would be viewed as obsolete.<sup>96</sup> Popper’s falsificationism has been widely discarded as, at best, a normative description of how science *should* operate, rather than a histori-

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95. *Id.*

96. David S. Caudill & Lewis H. LaRue, NO MAGIC WAND: THE IDEALIZATION OF SCIENCE IN LAW 3 (2006); David S. Caudill & Richard E. Redding, *Junk Philosophy of Science?: The Paradox of Expertise and Interdisciplinarity in Federal Courts*, 57 WASH. & LEE L. REV. 685 (2000); Edmond & Mercer, *Conjectures and Exhumations*, *supra* note 61; Gary Edmond & David Mercer, *The Invisible Branch: The Authority of Science Studies in Expert Evidence Jurisprudence*, in EXPERTISE IN REGULATION AND LAW 197 (2004).

cally or sociologically accurate model of how it, in fact, does operate.<sup>97</sup> Even more importantly, most contemporary philosophers view falsificationism as inadequate to account for the material and epistemological success of modern science.<sup>98</sup> Likewise, Merton's norms have been widely discarded as idealized notions of how science should operate.<sup>99</sup>

My point here, however, is not merely to take the NAS Committee to task for invoking obsolete models of science. In and of itself, this invocation does no real harm. Rather I want to explore whether there is indeed any harm wrought by these obsolete models of science. In other words, in what ways does the NAS Report's model of science impede the achievement of its purported goal: the "adoption," by forensic science, of a vaguely articulated "scientific culture"?

The most obvious problem is, once again, the mismatch between "discovery science" and "mundane science." Popper's theory of falsificationism was developed in order to explain how scientists made discoveries about the natural world. One aspect of falsificationism that makes this particularly clear—an aspect tellingly missing from the NAS Report's discussion of the "scientific method"—concerns the notion of "boldness." Popper argued that theories should be "bold," that only by thinking big, taking risks, and making "bold conjectures" would scientists advance knowledge.<sup>100</sup> Popper viewed "boldness" as harmless to the enterprise of science (if not to the reputation of the individual scientist) because, he claimed, the rest of the scientific community was busily engaged in attempts at refutation.<sup>101</sup> Thus, bold false hypotheses would be quickly refuted.<sup>102</sup>

It should be clear from this, however, that Popper's theory applies to the sort of theory-generating scientist who works at the apex of the academic establishment. We want boldness from these scientists, but we do not desire boldness from a host of other scientific workers. We do not, as a society, necessarily desire boldness from laboratory technicians, civil engineers, primary care physicians, environmental engineers, public health scientists, and so on. For that matter, we probably do not desire boldness from most forensic scientists. As will be discussed *infra*, most technoscientific work-

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97. CHALMERS, *supra* note 70.

98. *Id.*

99. Mitroff, *supra* note 52.

100. See generally KARL POPPER, CONJECTURES AND REFUTATIONS (1965).

101. *Id.*

102. Hence the title of one of Popper's books, *Conjectures and Refutations. Id.* There is a scientific adage that while bad data is harmful, there is nothing harmful about a bad theory.

ers, known as “forensic scientists,” fall into the categories of technoscientific workers from whom we would probably *not* desire boldness.

Likewise, Merton’s norms were designed to apply to scientists engaged in open-ended discovery activities. It is far from clear that the paramount virtues for laboratory technicians, civil engineers, primary care physicians, environmental engineers, and public health scientists are Merton’s norms. For these categories of scientific workers, it is possible that other virtues, like adherence to procedures and “good hands” are more valuable virtues.

The NAS Report, therefore, uses a philosophy of science designed for discovery scientists to lay out normative goals for a field that is primarily populated by individuals who are not engaged in discovery science. I will argue that this is a serious problem in that it turns the urging that forensic science “adopt scientific culture” into empty sloganeering, rather than a practical set of measures appropriate to the sort of work most forensic scientists do.

Paradoxically, the NAS Report explicitly recognized this mismatch. The report acknowledges that the methods, principles, and virtues it touts “have been discussed here in the context of creating new methods and knowledge,”<sup>103</sup> rather than in the context of routine, quotidian laboratory work. The NAS Report, however, immediately dismisses any concerns about this mismatch in the very same sentence, declaring that

the same principles hold when applying known processes or knowledge. In day-to-day forensic science work, the process of formulating and testing hypotheses is replaced with the careful preparation and analysis of samples and the interpretation of results. But that applied work, if done well, still exhibits the same hallmarks of basic science: the use of validated methods and care in following their protocols; the development of careful and adequate documentation; the avoidance of biases; and interpretation conducted within the constraints of what the science will allow.<sup>104</sup>

Did the eminent scientists on the NAS Committee really believe that formulating hypotheses is analogous to preparing samples? Does one need to apply, or even understand, falsification in order to prepare a sample? Does one need to adhere to organized skepticism in order to prepare a sample? Did they really believe that the methods, principles, and virtues necessary to use a validated method are the same as those necessary to develop or validate a method? Or did the committee, lacking ready access to a philosophy and sociology of laboratory *work* that is as easily digestible as

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103. NAS REPORT, *supra* note 3, at 113.

104. *Id.*

Popper and Merton's accounts of scientific discovery, simply decide to graft accounts of scientific discovery onto scientific work and hope for the best?

The irony, of course, is that plenty of sociological literature on scientific work is available.<sup>105</sup> Again, my purpose here is not to engage in the rather empty exercise of criticizing the NAS Committee for paying inadequate attention to the sociology of science. The NAS Committee had a broad ambit, and it did a fantastic job with many important matters. As a sociologist of science myself, I do not believe that the Committee's limited time would have been well spent parsing the intricacies of the sociology of scientific and technical work. I do believe, however, that the NAS Report's rather cursory engagement with the nature of forensic scientific work demands that others—and who better than sociologists of science?—think more carefully about what “adopting scientific culture” might mean, given the nature of forensic scientific work. I also believe that, if such careful thinking is not done, the NAS Report's call to adopt scientific culture risks becoming an empty gesture.

#### IV. FORENSIC WORK

I have argued that it is important to understand the practice of science as *work*, and I have argued that what we call “scientific work” encompasses a wide variety of different tasks. How might we begin to describe forensic science as work? We might begin by describing forensic work as set of tasks that are reasonably distinct. As a first attempt at such a classification, it would seem that what we refer under the broad ambit of “forensic science” would consist of the following tasks:

1. *Basic Research.* This would include activities such as developing new methods and technologies of forensic analysis, such as new chemical detection methods as well as the validation of these techniques. Some of this research occurs at universities or national laboratories,<sup>106</sup> and bench personnel, whose capacity is primarily dedicated to other tasks, conduct

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105. See generally BETWEEN CRAFT AND SCIENCE, *supra* note 57; H.M. COLLINS, CHANGING ORDER: REPLICATION AND INDUCTION IN SCIENTIFIC PRACTICE (1985); MICHAEL POLANYI, THE TACIT DIMENSION (1966); STEVEN SHAPIN, A SOCIAL HISTORY OF TRUTH (1994); Stephen R. Barley & Beth A. Bechky, *In the Backrooms of Science: The Work of Technicians in Science Labs*, 21 WORK & OCCUPATIONS 85 (1994); Park Doing, *'Lab Hands' and the 'Scarlet O': Epistemic Politics and (Scientific) Labor*, 34 SOC. STUD. SCI. 299 (2004); Kathleen Jordan & Michael Lynch, *The Mainstreaming of a Molecular Biological Tool: A Case Study of a New Technique*, in TECHNOLOGY IN WORKING ORDER: STUDIES OF WORK, INTERACTION, AND TECHNOLOGY 162 (Graham Button ed., 1993).

106. See generally Victoria A. Smith et al., *The Reliability of Visually Comparing Small Frontal Sinuses*, 55 J. FORENSIC SCI. 1413, 1413-1680 (2010).

some of it.<sup>107</sup> The NAS Report is not critical of the quality of the current performance of this task, but it clearly states that there is not enough of it in terms of personnel, resources, or results.<sup>108</sup> It is fairly clear that U.S. funding agencies have tended to neglect forensic science.<sup>109</sup> Though basic researchers seem to have done reasonably well at validating new technologies and new detection methods, they have neglected one large area: the validation of pattern recognition techniques based on human visual interpretation (such as handwriting identification, bitemark identification, latent print identification, and firearms and toolmark identification, among other things). These activities are, of course, analogous to the sort of “discovery science” that has been the focus of most philosophical and early sociological accounts of “science.”

2. *Evidence Collection.* This would include all activities that provide “inputs” to forensic laboratories, analyses, and assays. It is, of course, crucially important, in accord with the well-known “garbage in, garbage out” principle.<sup>110</sup> It is not clear how much scientific or philosophical knowledge is necessary to perform these tasks. Indeed, in many cases the task is performed by personnel without scientific training and without pretensions to being “scientists,” such as sworn law enforcement officers.<sup>111</sup>

3. *Technical Management.* This would include work within a forensic laboratory overseeing the application of various forensic assays and ordering and coordinating various forensic assays in particular cases, but not necessarily actually performing the assays themselves. Increasingly, it

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107. See generally Marcel de Puit, *An Alternative Trinity: Objectivity, Subjectivity, and Transparency*, 60 J. FORENSIC IDENTIFICATION 1 (2010).

108. See NAS REPORT, *supra* note 3, at 22.

109. *Id.*

110. A proverb from computing and information technology referring to the principle that nonsensical inputs will necessarily yield nonsensical output, attributed to the IBM computer technician George Fuchsel in the early days of computing. See *Garbage In, Garbage Out*, WIKIPEDIA, [http://en.wikipedia.org/wiki/Garbage\\_in,\\_garbage\\_out](http://en.wikipedia.org/wiki/Garbage_in,_garbage_out) (last visited Dec. 15, 2010).

111. KEITH INMAN & NORAH RUDIN, PRINCIPLES AND PRACTICE OF CRIMINALISTICS: THE PROFESSION OF FORENSIC SCIENCE 62 (2001) (“Perhaps the most bitter and persistent complaint throughout the history of criminalistics has been the lack of adequate training in crime scene procedures. In fact, most of the ‘police science’ or ‘crime detection’ books specifically address, at least in part, the audience of nonscientifically oriented police officers and detectives who are virtually always the first to arrive at a scene. More often than not, a law enforcement officer or evidence collection technician with minimal scientific training is the person tasked with the all important charge of recognizing and collecting evidence. Less and less often will a criminalist from the laboratory be called to the crime scene, and the decision to do so is usually that of those already there. The individual making decisions about what evidence to collect and the person given the responsibility to collect it vary widely between jurisdictions, so it is difficult to generalize.” [citations omitted]).

might involve cost-benefit analyses of what assays should be conducted, taking into account not only the costs of the assays, but also the potential probative value of the results of such assays.<sup>112</sup> It might also involve piecing together the results of various assays in order to develop a holistic forensic “account” of the crime. Historically, technical management does not appear to have been a particularly prominent task and may not have even been explicitly defined as a task distinct from what I below call “Analysis.” A number of converging trends, however, including increasing emphases on quality control, increasing attention to cost-benefit analyses, and heightened concerns about observer bias,<sup>113</sup> suggest that the profile of this task may be expected to grow.

4. *Analysis.* This would include all activities in which a forensic technique is deployed upon an item of evidence: such as a handwriting, bite mark, latent print, tool mark, DNA, or toxicological analysis.

5. *Interpretation.* This would include the interpretation and reporting of the meaning of an analysis or a set of analyses. As with “Technical Management,” this task has not historically been regarded as particularly prominent or even as a task distinct from Analysis.<sup>114</sup> The NAS has, however, called for increasing attention to reporting of results, and recent scholarship has called attention to the crucial importance of this aspect of forensic work.<sup>115</sup> These trends suggest that we would do well to treat this as a distinct task.

Even based on this rudimentary definition of forensic tasks, it should be clear that the same principles, methods, and desired virtues are probably not appropriate to all five tasks. Furthermore, the standard principles, methods, and virtues that are habitually invoked for discovery science are not necessarily appropriate for all five tasks. It does not seem helpful, for example, to tell a scientific worker engaged in evidence collection to apply Popper’s theory of falsificationism or to apply the “scientific method” as-

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112. Christopher J. Lawless & Robin Williams, *Helping With Inquiries or Helping With Profits? The Trials and Tribulations of a Technology of Forensic Reasoning*, 40 SOC. STUD. SCI. 731 (2010).

113. Dan E. Krane et al., *Sequential Unmasking: A Means of Minimizing Observer Effects in Forensic DNA Interpretation*, 53 J. FORENSIC SCI. 1006 (2008); D. Michael Risinger et al., *The Daubert/Kumho Implications of Observer Effects in Forensic Science: Hidden Problems of Expectation and Suggestion*, 90 CAL. L. REV. 1 (2002).

114. NAS REPORT, *supra* note 3, at 111-26.

115. See generally Simon A. Cole, *Who Speaks for Science? A Response to the National Academy of Sciences Report on Forensic Science*, 9 LAW PROBABILITY & RISK 25 (2009); ; Dawn McQuiston-Surrett & Michael J. Saks, *The Testimony of Forensic Identification Science: What Expert Witnesses Say and What Factfinders Hear*, 33 LAW & HUM. BEHAV. 436 (2009).

sociated with Sir Isaac Newton. Somewhat more controversially, I will also argue *infra* that it is not helpful to tell these things to a worker engaged in Analysis either. We may, however, want to impose a quality control regime upon workers engaged in Analysis, but we do not necessarily want to impose a quality control regime upon a basic researcher.

## V. NORMATIVE GOALS FOR FORENSIC TASKS

If the standard set of principles, methods, and virtues invoked by the NAS Report are neither necessary nor helpful for all forensic tasks, then what principles, methods, and virtues *do* we need for each task? What “scientific culture” do we want for each task?

1. *Basic Research.* Presumably, we want forensic basic researchers to behave like basic researchers in other areas of science. We want them to innovate and to subject their innovations to rigorous scrutiny. We want the individuals who do this work to be trained scientists, much as they are in other areas of science. We want a “culture” that is as much like a university—or at least like an industrial research laboratory—as possible.

2. *Evidence collection.* Here, we do not want a culture centered around hypothesis testing, falsificationism, or organized skepticism. What seem to be most needed are care, accountability, meticulous documentation, and ethics. By far the greatest concern expressed in the forensic literature about evidence collection concerns the potential for contaminating the crime scene.<sup>116</sup> It seems that the main concern is that we want people who are careful, meticulous, and honest. They need not know philosophy of science or be competent to practice science. Primarily, they need to be aware of the current capabilities and limitations of forensic science, so as to know what to collect and what not to collect, what sorts of actions might lead to contamination, and so on. In this area, therefore, we would expect “scientific culture” to mean something very different: care, meticulousness, and attention to detail, among others.

3. *Technical management.* Here we want an individual to think scientifically about a case as a whole. Due to the widespread popularity of Popper, many have suggested that this “thinking” should be conceptualized in terms of hypothesis testing.<sup>117</sup> This may not necessarily be wrong, but it is probably not ideal. Contemporary philosophers of science would probably suggest that the Technical Manager should evaluate which explanation of the case appears most robust as evidence emerges. Among scholars who think about forensic science, however, the dominant approach has been to

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116. INMAN & RUDIN, *supra* note 111.

117. *See supra* note 87.

evaluate a case from a Bayesian framework.<sup>118</sup> Thus, a Technical Manager would need to be aware of the capabilities and limitations of the tools used by Analysts (whether or not she has practiced as an Analyst herself) and would need to know some philosophy of science, most likely a Bayesian approach given the current state of the field. The Technical Manager would not, however, need to know how to do basic research. The “scientific culture” would be one of rigorous open-mindedness and critical thinking with an emphasis on avoiding traps in reasoning, such as circularity and the transposed conditional.<sup>119</sup>

4. *Analysis.* While there have been attempts to conceptualize forensic analyses in terms of hypothesis testing, as discussed above,<sup>120</sup> this is neither necessary nor, probably, appropriate. Instead, we should think of analysts much in the way we think about laboratory technicians in a university, industrial, or medical setting. We do not want, or need, these analysts to think about the validity of the assays they perform. What we want is for analysts to do well at performing assays that someone else has validated.<sup>121</sup> We want them to be careful, meticulous, and honest in their application of these assays. We want them to document their work, and to adhere to protocols. The “scientific culture” we want has far more to do with care, meticulousness, documentation, and honesty than with hypothesis testing, falsification, or organized skepticism.

5. *Interpretation.* As numerous scholars have documented, there are a host of tricky issues raised by the interpretation of forensic evidence.<sup>122</sup> Handling these issues requires expertise in logical reasoning that seems to most commonly be acquired through training in philosophy, law, science, mathematics, or statistics. Here “scientific culture” would essentially be centered around logical reasoning of the highest order. The primary values might be a determination to report the evidence as accurately and precisely as possible, as well as a sense of self-restraint that would allow the Inter-

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118. See, e.g., COLIN G.G. AITKEN, *STATISTICS AND THE EVALUATION OF EVIDENCE FOR FORENSIC SCIENTISTS* (1995); BERNARD ROBERTSON & G. A. VIGNAUX, *INTERPRETING EVIDENCE: EVALUATING FORENSIC SCIENCE IN THE COURTROOM* (1995).

119. See generally William C. Thompson & Edward L. Schumann, *Interpretation of Statistical Evidence in Criminal Trials*, 11 *LAW & HUM. BEHAV.* 167 (1987) (explaining fallacies in the interpretation of statistical evidence to which lay people are susceptible).

120. See *supra* note 87.

121. This is a broad generalization that would need to be discussed more specifically for different assay, laboratories, countries, and so on. For some assays, “local” validation of specific instruments is necessary. This is a separate issue from what we might think of as “global” validation of the ability of certain technique to detect certain things.

122. See generally AITKEN, *supra* note 118; DAVID J. BALDING, *WEIGHT-OF-EVIDENCE FOR FORENSIC DNA PROFILES* (2005); ROBERTSON & VIGNAUX, *supra* note 118.

prefer to resist the temptation to make more inferences than are warranted by the analytic results. Interpreters would have to think much more carefully about probability and proof than do Basic Researchers, and they would have little use for the skills and virtues associated with Basic Researchers.

## VI. THE CURRENT STATE OF AFFAIRS

Having laid out an ideal model for forensic science, let us see how the current state of affairs, as the NAS Report found it, measures up. In most of the pattern recognition disciplines, the various task categories articulated above are blurred, blended, and often performed by the same individual. Some basic research is performed in university, industrial, or government laboratories, but not enough. Therefore, it has been left to a few intellectually curious analysts to perform the balance of basic research, often in their personal time since they are still expected to perform their normal workload of analysis.<sup>123</sup> It is not to demean the contributions of these self-sacrificing individuals to say that they are often not trained to do basic research, lack the resources typically available to researchers at university, industrial, and government laboratories, and lack the professional networks basic researchers use to test their research and generate innovation.<sup>124</sup>

Evidence collection, in contrast, *is* sometimes separated as a task from the other aspects of forensic science.<sup>125</sup> But, in many other cases, the same individual performs evidence collection and analysis.<sup>126</sup>

The task I have called here “technical management” is often not performed at all and is sometimes handled by a non-scientist detective or an experienced analyst who has become a manager.

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123. Examples of this in pattern recognition include Tuthill and Ashbaugh, practitioners with virtually no formal scientific education who essentially remade the conceptual foundations of pattern recognition. *See generally* ASHBAUGH, *supra* note 51; HAROLD TUTHILL, *INDIVIDUALIZATION: PRINCIPLES AND PROCEDURES IN CRIMINALISTICS* (2004). The achievements of these individuals are all the more remarkable given their *lack* of formal scientific education; my point is not to criticize their impressive achievement, but rather to argue that it is a poor institutional structure that relies on such individuals for basic research.

124. *Id.*; NAS REPORT, *supra* note 3, at 14 (“[F]orensic science and forensic pathology research, education, and training lack strong ties to our research universities and national science assets.”).

125. INMAN & RUDIN, *supra* note 111.

126. ASHBAUGH, *supra* note 51, at 136; Pat A. Wertheim, *Integrity Assurance: Policies and Procedures to Prevent Fabrication of Latent Print Evidence* 48 J. FORENSIC IDENTIFICATION 431, 435 (1998); Interpol European Expert Group on Fingerprint Identification, *Method for Fingerprint Identification*, INTERPOL (2005), available at <http://www.interpol.com/public/Forensic/fingerprints/WorkingParties/IEEGFI/ieegfi.asp#> (last visited Dec. 2, 2010).

Analysis and interpretation are currently not separated at all for pattern recognition disciplines. The notion that these tasks should be separated is simply a proposal; it does not currently exist.<sup>127</sup>

And then there is the crucial issue of validation. The NAS Report noted that validation was lacking for nearly all of the non-DNA forensic identification technologies.<sup>128</sup>

### A. Historical Explanation for the Current State of Affairs

How did this happen? How did we come to a point where techniques were used in court, in some cases for a century, without any validation studies?<sup>129</sup> How were Analysts permitted to report conclusions in a way that, the NAS Report now tells us, was unsupported for decades?<sup>130</sup> It would seem that we can explain the current state of affairs historically—that we can discern historically how we arrived at the position we are in today.

Basic researchers, who largely set their own research agendas, never set out to do validation studies. There were probably a number of reasons for this. There was little intellectual incentive to conduct validation studies of techniques that were already being used and had been accepted as valid by courts.<sup>131</sup> Funding agencies provided little material incentive to do such research.<sup>132</sup> Most forensic basic researchers tend to have expertise in a

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127. This is actually somewhat curious. For example, one of the oft-noted problems with latent print analysis is that there is no metric for “sufficiency”; no objective measurement that would allow the Interpreter to know that the amount of friction ridge detail found consistent between two prints is sufficient to conclude that one should expect only one piece of friction ridge skin in the universe to produce prints consistent with that detail. Instead, the practice is to rely on Analysts to make intuitive judgments about the rarity of various configurations of friction ridge detail in the universe. This is obviously inferior to estimates of rarity based on objective data from sampled populations. Nonetheless, the system might be improved by separating analysis from interpretation—that is, having one experienced latent print analyst determine whether the ridge detail is consistent between two prints, and then asking another experienced analyst to make a separate intuitive estimate of the rarity of the consistent detail.

128. NAS REPORT, *supra* note 3, at 87.

129. *Id.* at 43.

130. *Id.* at 87.

131. Simon A. Cole, *Is Fingerprint Identification Valid? Rhetorics of Reliability in Fingerprint Proponents' Discourse*, 28 LAW & POL'Y 109, 129 (2006).

132. There is the now well-known scandal of the 2000 National Institute of Justice Request for Proposal (“RFP”) for fingerprint validation research. The RFP was allegedly suppressed by the FBI in order to avoid jeopardizing its position in an admissibility hearing on fingerprint evidence and the proposal was never funded. By all accounts, one of the Principal Investigators on the most credible proposal was retained by the defendant in that same admissibility hearing. NAT'L ACADEMIES, REPORT TO CONGRESS (2009), <http://www.nationalacademies.org/annualreport/eng09.html> (last visited Dec. 2, 2010).

specific scientific discipline, such as chemistry or biology,<sup>133</sup> which may be appropriate for developing new detection methods, but not necessarily for conducting validation studies. As FBI Laboratory Director Christian Has-sell put it, validation research is “the ‘valley of death’ because ‘nobody wants to pay for it, nobody really wants to do it.’”<sup>134</sup>

Because the mainstream scientific community historically showed little interest in forensic science, the “validation question” was never posed within the scientific community. It was left to criminal defendants to ask, “Where is the study showing this technique is valid?” The *Daubert* decision in 1993<sup>135</sup> provided an opening for defendants to ask precisely this question.

Because analyses have been blurred with interpretation, it was the Analysts who appeared in court. And, so the question, “Where is the study showing this technique is valid?” was posed to Analysts, not to Interpreters (who barely, if at all, exist),<sup>136</sup> Technical Managers, or Basic Researchers. Basic Researchers had long been focused on such matters as developing new detection techniques and therefore had not built up either a body of knowledge, or even thinking, about the question of validation.<sup>137</sup> Moreover, connections between Basic Researchers and Analysts were weak.<sup>138</sup> For all these reasons, Basic Researchers were of little use as resources when the validation question was posed to Analysts in court. Analysts were left to answer the question alone.

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.<sup>139</sup> 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.

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133. Dep’t of Chemistry, Cal. State Univ., Fresno, *Forensic Science*, <http://www.csu.fresno.edu/chem/facstaff/forensic.shtml> (last visited Dec. 2, 2010).

134. Laura Spinney, *Forensic Science Braces for Change*, NATURENEWS (July 22, 2010), <http://www.nature.com/news/2010/100722/full/news.2010.369.html>.

135. *Daubert v. Merrell Dow Pharm., Inc.*, 509 U.S. 579 (1993).

136. One might argue that forensic consultants, who reinterpret the results of other Analysts, are Interpreters.

137. See Nat’l Academies, *supra* note 19.

138. NAS REPORT, *supra* note 3, at 14.

139. David L. Grieve, *The Identification Process: Traditions in Training*, 40 J. FORENSIC IDENTIFICATION 195 (1990).

Under these circumstances, Analysts offered answers in court to the validation question that would not have been either offered or accepted by a professional scientist. These answers included the following: what I have called the “fingerprint examiner’s fallacy,” that the claimed uniqueness of the target of analysis demonstrates the validity of the analysis;<sup>140</sup> what Saks and Koehler have called the “individualization fallacy,” that a finding of consistency between two images warrants the conclusion that the images must derive from the same source;<sup>141</sup> what I have called “casework validation,” that longevity of use in court constitutes proof of the accuracy of the technique;<sup>142</sup> and the separation of error rates into “methodological” and “human” categories such that the “methodological error rate” can be meaningfully designated as zero.<sup>143</sup>

These answers were unfortunate but predictable outcomes of the social structure of forensic science in the pattern recognition disciplines. The courts got as good answers as they could have expected from a group of Analysts with the training, background, orientation, and, yes, *culture* that they had. But what really compounded the problem was that the courts accepted these answers hook, line, and sinker.<sup>144</sup> There ensued then a decade long debate in the pages of law journals and legal opinions.<sup>145</sup> Nearly a decade was lost seeking to establish that the uniqueness of friction ridge skin was not the right empirical question to ask about the validity of latent print identification, that use in court could not substitute for scientific validation testing, and that it was misleading to call the error rate of forensic assays “zero.” While these canards would seem to have been put to rest by the NAS Report—which rejected them all—in fact, so firmly are they now ensconced in Analysts’ and courts’ way of thinking that they are proving very difficult to dislodge, and they continue to live on.<sup>146</sup>

My point is less that Analysts gave poor answers than that Analysts should never have been asked to defend the epistemological underpinnings

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140. Simon A. Cole, *Forensics Without Uniqueness, Conclusions Without Individualization: The New Epistemology of Forensic Identification*, 8 LAW PROBABILITY & RISK 233, 236 (2009) [hereinafter Cole, *Forensics Without Uniqueness*].

141. Michael J. Saks & Jonathan J. Koehler, *The Individualization Fallacy in Forensic Science Evidence*, 61 VAND. L. REV. 199, 204-05 (2008).

142. Simon A. Cole, *‘Implicit Testing’: Can Casework Validate Forensic Techniques?*, 46 JURIMETRICS 117 (2006).

143. Simon A. Cole, *More Than Zero: Accounting for Error in Latent Fingerprint Identification*, 95 J. CRIM. L. & CRIMINOLOGY 985 (2005); see also NAS REPORT, *supra* note 3, at 143.

144. MODERN SCIENTIFIC EVIDENCE, *supra* note 13.

145. *Id.*

146. Cole, *Forensics without Uniqueness*, *supra* note 140.

of their assays. Given the social structure of forensic science, there was no alternative; there was no one else. That a mess resulted should surprise no one. The situation was somewhat analogous to haling radiological technicians into court to testify about the epistemological validity of radiological examinations. Of course, that would never happen because radiological technicians could draw on the support of physicians who interpret the results in light of medical knowledge and the researchers who developed and validated the technologies.

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 most of the pattern recognition forensic identification techniques 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, in the NAS Report's words, "critical questioning."<sup>147</sup> The creation of reporting regimes which mandated the reporting of conclusions in terms that were illogical and unsupported (e.g., "individualization") can be understood as a cultural failure of epistemological modesty, a failure of, in the NAS Report's words, a "scientific culture [that] encourages cautious, precise statements and discourages statements that go beyond established facts."<sup>148</sup> And, the defensiveness with which the forensic community reacted to the challenges posed by outsiders—challenges that, according to the NAS Committee, turn out to have been, in fact, warranted—can be understood as a cultural failure of "openness to new ideas, including criticism and refutation," not to mention a failure of the virtue of collegiality.<sup>149</sup>

## VII. BUILDING A FORENSIC SCIENTIFIC CULTURE

It should now be clear that building a scientific culture in forensic science will require more than a blanket recitation of Popperian theories and Mertonian virtues. Instead, we need to think carefully about the roles played by various actors in forensic science, the virtues desired by each, and the principles and methods we want each to follow. We need not, however, necessarily start this process of thinking from scratch. Other areas of science—arguably all areas—accommodate multiple roles. We

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147. NAS REPORT, *supra* note 3, at 125.

148. *Id.*

149. *Id.* at 113.

can perhaps draw on other areas of science for analogies that might help suggest models for an ideal social structure of forensic science.

Medicine would seem to pose a model of obvious relevance to forensic science.<sup>150</sup> Medicine, as a broad category, encompasses a broad range of technoscientific workers who play a variety of roles. For our purposes, however, we can focus on four roles that are analogous to the roles we defined for forensic science. First, there are biomedical researchers, who tend to hold Ph.D.'s, M.D.'s, or both. These researchers are engaged in basic research and the production of knowledge about the natural world. They may never see patients, never have seen a patient since medical school (like Oliver Sacks in a memorable scene in *Awakenings*), or (in the case of Ph.D.'s) never have seen a patient at all.<sup>151</sup> They may not be competent to administer medical treatments or perform medical procedures.<sup>152</sup>

Second, there are clinical physicians. Crucially, these individuals must be competent to read, digest, and apply the medical knowledge that is produced by medical researchers, as it is disseminated to them through such mechanisms as journals, conference presentations, (less optimally) pharmaceutical company advertisements, and so on.<sup>153</sup> These individuals are ethically and legally required to make decisions in light of the current knowledge produced by researchers.<sup>154</sup> They need not, however, perform

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150. I am hardly the first to suggest this. Dr. Stoney anticipated some of the arguments made here, though his focus is on forensic science education, rather than its institutional structure. David A. Stoney, *A Medical Model for Criminalistics Education*, 33 J. FORENSIC SCI. 1086 (1988). Another forthcoming article, on which I am a co-author, also explores themes quite similar to those mentioned here. Jennifer L. Mnookin et al., *The Need for a Research Culture in the Forensic Sciences*, UCLA L. REV. (forthcoming 2011). Not surprisingly, there may be some differences in nuance between that, jointly authored article and this sole-authored one, but the general thrust of the arguments is largely consistent.

151. OLIVER SACKS, *AWAKENINGS* (1973); Molly Cooke et al., *American Medical Education 100 Years After the Flexner Report*, 355 NEW ENG. J. MED. 1339 (2006).

152. Cooke et al., *supra* note 151.

153. HARRY H. MARKS, *THE PROGRESS OF EXPERIMENT: SCIENCE AND THERAPEUTIC REFORM IN THE UNITED STATES, 1900-1990*, at 231 (1997) ("Physicians are presumed to accept voluntarily either the rational dictates of scientific method or the judgments of constituted authorities."); Eliot Freidson, *The Reorganization of the Medical Profession*, 42 MED. CARE RES. REV. 11, 30-31 (1985) [hereinafter Freidson, *Reorganization*] ("Where once all practitioners could employ their own clinical judgment to decide how to handle their individual cases independently of whatever medical school professors asserted in textbooks and researchers in journal articles, now the professors and scientists who have no firsthand knowledge of those individual cases establish guidelines that administrators who also lack such firsthand experience attempt to enforce.").

154. MARKS, *supra* note 153, at 231 ("[M]embership in the republic of science was offered to those who would acknowledge the constituted authorities within medicine by allowing their deliberations and reflections to serve as a surrogate for the judgments of the individual physician."); Eliot Freidson, *The Changing Nature of Professional Control*, 10 ANN.

research themselves or even be competent to perform research.<sup>155</sup> Therefore, often they will “know” that a treatment has a given degree of effectiveness under given conditions because of some research.<sup>156</sup> What this means is that research findings have been disseminated to them in some form which states that some researcher has determined the treatment has that degree of effectiveness under those conditions. The clinician does not, of course, “know” the effectiveness of the treatment in the sense of having performed the research herself or even having seen it done. What we have in place in medicine is a trust-based system of knowledge dissemination, with trust invested in scientific institutions like journals and their peer review systems. While such systems are notoriously far from foolproof,<sup>157</sup> they have been adopted as reasonable pragmatic solutions to the problem of knowledge dissemination in socially important areas like medicine.

Third, there are laboratory technicians who perform a variety of procedures and tests, from biochemical assays to radiological imaging. These individuals rarely, if ever, hold M.D.’s or Ph.D.’s.<sup>158</sup> Although they do not have the knowledge that clinicians have acquired by attending medical school and functioning as physicians, a long tradition of sociological research has shown that these individuals often have very sophisticated knowledge that takes other forms.<sup>159</sup> In some cases, this knowledge has been referred to as “tacit knowledge.”<sup>160</sup> Some laboratory technicians may be more competent than physicians at performing certain laboratory procedures, and some radiological technicians may be more skilled at reading

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REV. SOC. 1, 1 (1984) [hereinafter Freidson, *Changing Nature*] (“Rank and file practitioners are no longer as free to follow the dictates of their individual judgments as in the past . . .”).

155. Freidson, *Changing Nature*, *supra* note 154 (“The technical standards” governing physicians “are devised by a separate group of professionals—the knowledge elite—who are based primarily in professional schools . . .”).

156. Stephen Harrison & Waqar I.U. Ahmad, *Medical Autonomy and the UK State 1975 to 2025*, 34 *SOCIOLOGY* 129, 138 (2000) (“[I]t has increasingly become the case . . . that clinical decisions must be justified by reference to external research findings . . .”).

157. *See generally* FIONA GODLEE & TOM JEFFERSON, *PEER REVIEW IN HEALTH SCIENCES* (2003).

158. *See generally* Stephen R. Barley & Beth A. Bechky, *In the Backrooms of Science: The Work of Technicians in Science Labs*, 21 *WORK & OCCUPATIONS* 85 (1994); Stephen R. Barley, *Technology as an Occasion for Structuring: Evidence From Observations of CT Scanners and the Social Order of Radiology Departments*, 31 *ADMIN. SCI. Q.* 78 (1986); Stephen R. Barley, *The Social Construction of a Machine: Ritual, Superstition, Magical Thinking and Other Pragmatic Responses to Running a CT Scanner*, in *BIOMEDICINE EXAMINED* 497 (Margaret Lock & Deborah R. Gordon eds., 1988).

159. POLANYI, *supra* note 105.

160. *Id.*

images than their clinician supervisors.<sup>161</sup> Crucially, however, even when technicians possess superior skills, we do not generally expect them to make diagnoses. The task of diagnosis is reserved for clinicians, largely because of the knowledge that clinicians acquire from researchers. Even if a radiological technician is more skilled at interpreting a film than the physician, we believe the physician is able to place that interpretation within the context of medical knowledge. In this sense, we want medical technicians to exercise their manual and interpretive skills, but we do not want them to make inferences about what those interpretations mean for the larger “case” as a whole, the diagnosis of the patient. Moreover, technicians’ knowledge about the validity of the techniques they apply is typically very limited.<sup>162</sup> Medical technicians would probably be quite hard pressed to cite the studies which validated the tests or instruments that they use.

This division of medical labor was not always in place, but is the product of historical changes in medical education and practice. In the late nineteenth century, medical education famously became more “scientific.”<sup>163</sup> These educational changes were based on the presumption that “[a]lthough ‘not every student . . . can become an experimenter . . . every physician must be so educated that he may intelligently apply the knowledge furnished him by experimental medicine in the cure of such diseases as can be cured.’”<sup>164</sup> This is precisely the principle I am advocating for the relationship between Researchers and Technical Managers.

At this point, the thrust of the analogy should be clear: in 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. What we primarily want from them would be to exercise their skills well. Mediating between these groups would be a cadre of individuals with more scientific training and knowledge than technicians. These individuals would need to know enough science to be educated consumers of the knowledge produced by basic researchers. They would, however, 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 va-

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161. *See supra* note 158.

162. *See supra* note 158.

163. THOMAS NEVILLE BONNER, *BECOMING A PHYSICIAN: MEDICAL EDUCATION IN BRITAIN, FRANCE, GERMANY, AND THE UNITED STATES, 1750-1945* (1995).

164. *Id.* at 290-91; *see also* MARKS, *supra* note 153.

lid or not, not because they had validated them themselves or even seen it done, but because they would understand what a validation consists of, and would be capable of evaluating scientific literature to determine whether the studies that had been conducted were appropriate to support the claimed results.

The fourth role in forensic science is filled by crime scene technicians. I have not focused on this group because I don't think it is the focus of the issues identified by the NAS Report. For purposes of ensuring the neatness of the analogy with medicine, however, we can compare crime scene technicians to Emergency Medical Technicians ("EMTs"). Although the analogy is not perfectly apt, there are some similarities: they operate in the field; they do not have the broad base of medical knowledge held by physicians (or even nurses), but do deploy a small, highly specific corpus of medical knowledge; they operate according to strict ethical and legal controls regarding what they can and cannot do at "the scene."<sup>165</sup>

#### A. Hierarchy

Medicine is notoriously hierarchical, and, in drawing an analogy with medicine, we should be candid about the fact that we are proposing a hierarchical structure for forensic science. Hospitals are, of course, notoriously hierarchical, with physicians at the top of the hierarchy. The relationship between physicians and medical technicians, for example, is obviously hierarchical. There is, however, a sense in which the relationship between medical researchers and clinicians is hierarchical as well. I do not mean this in a political sense, so much as an epistemological sense. Clinicians are, in some way, required to consume the knowledge that medical researchers produce. The nature of this "requirement" has, of course, changed greatly in the last couple of decades and remains greatly in flux as evidence-based medicine ("EBM") and clinical practice guidelines ("CPGs") have become more common in medicine.<sup>166</sup> But even prior to the development of EBM there was an expectation that clinicians were required to know what was in the literature.<sup>167</sup> As they do today, clinicians retained a great deal of autonomy and discretion in applying what was in the literature,<sup>168</sup> yet there were points at which a treatment became so dis-

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165. STEFAN TIMMERMANS, *SUDDEN DEATH AND THE MYTH OF CPR* 184-205 (1999).

166. See generally STEFAN TIMMERMANS & MARC BERG, *THE GOLD STANDARD: THE CHALLENGE OF EVIDENCE-BASED MEDICINE AND STANDARDIZATION IN HEALTH CARE* (2003).

167. Freidson, *Reorganization*, *supra* note 153.

168. Freidson, *Changing Nature*, *supra* note 154.

credited in the literature that it would be malpractice to apply it (or, vice versa, so accepted that it would be malpractice not to apply it).<sup>169</sup>

Of course, no one suggests that this has been a recipe for harmony in medicine. To the contrary:

Since the standards of the knowledge elite are grounded in the abstract world of logic, scientific principles, and statistical probabilities rather than in the concrete world of work, in experimental designs and controlled laboratory findings rather than in the untidy, uncontrolled arena of practice, and in circumstances that are considerably less subject to the constraints of time, money, equipment, and other resources than is true of everyday practice, it is not hard to understand the skepticism of the rank and file professional.<sup>170</sup>

So we should be candid: the medical model we are proposing here is not a recipe for harmonious convergence. Rather, it is my assertion that there is a set of limited options: the status quo in which non-scientifically-educated practitioners are left to fend for themselves, a “harmony” model in which non-scientifically-educated practitioners and scientifically-educated researchers (and perhaps even lawyers and police) are treated as equal “stakeholders,” and a “hierarchical” model in which a “knowledge elite” of researchers exerts control over practitioners. We suggest being candid that, while hierarchy may not always be the most palatable thing to advocate, it appears to be the best model for society to get what it wants from forensic science, and it is the least bad option. It should be noted, for example, that society is reasonably content with the hierarchical model in medicine. While there is certainly great public resentment when a rank and file physician’s discretion is limited by a non-physicians, such as an insurer, accountant, or medical administrator, there is far less public sentiment behind the notion that primary care providers should be permitted to ignore the findings of medical research.<sup>171</sup>

We propose here the same sort of hierarchy for forensic science. Researchers would have the last word on whether a method or technique is valid. Technicians would no longer be put in the awkward position of having to defend the validity of the techniques they apply. Likewise, they would no longer have the power, as they have had for so long, to *ipse dixit* “dec-

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169. *Id.* at 2.

170. *Id.* at 16.

171. See, e.g., Physicians Warned Not to Ignore Suicide Clues, National Center for Biotechnology, Newsbriefs, 132 CAN. MED. ASSOC. J. 1404 (1985), available at <http://www.ncbi.nlm.nih.gov/pmc/articles/PMC1346111/pdf/canmedaj00263-0070.pdf>.

lare” or vouch for<sup>172</sup> the validity of the techniques they apply. Technical Managers, meanwhile, might be called upon to explain the validity of the techniques used in their laboratories, but they would do so based on research conducted by Basic Researchers.<sup>173</sup> Technical Managers, however, would be *required* to be cognizant of, and take into account, the scientific literature, much in the way that medical clinicians are. Technical Managers would, in a sense, be *required* to consume basic forensic scientific knowledge.

In proposing hierarchy, it should be noted that we are not proposing the creation of an elite “priesthood” that would have a monopoly on the legitimation of knowledge.<sup>174</sup> Recall that this vision would assume, as a fundamental precondition, the removal of NIFS and of forensic laboratories from the control of law enforcement. Basic Researchers would be expected to be a diverse group of scientists with diverse viewpoints, as medical researchers are today. While some of them might have “official” positions in forensic science (e.g., NIFS scientists), others might be independent of forensic science and employed by universities, industrial corporations, or non-government organizations. There would be an interaction between government scientists and those outside of government, much as there is between scientists who work for governmental scientific institutions like the National Institutes of Health or the Food and Drug Administration and

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172. See generally Michael J. Saks, *Banishing Ipse Dixit: The Impact of Kumho Tire on Forensic Identification Science*, 57 WASH. & LEE L. REV. 879 (2000).

173. We might distinguish here between the validation of instruments and the validation of what I call assays. All forensic assays require general validation. A study is necessary to show that, for example, a mass spectrometer is capable of distinguishing one particular set of chemicals from others. Many assays also require what we might call specific validation. In forensic disciplines involving instrumental analysis, such as DNA profiling and drug analysis, it is often necessary to validate instruments locally as well. For example, having established that mass spectrometers in general are capable of detecting particular sets of chemicals, laboratories typically also need to establish that a particular mass spectrometer is capable of detecting these chemicals, as used in the local laboratory by the local laboratory personnel. These specific validation activities would still need to be performed by Technical Managers or perhaps Analysts. I am, however, suggesting that general validation activities should not be located in a front-line laboratory.

The situation is somewhat different in the pattern recognition disciplines, such as latent prints, handwriting analysis, bite mark analysis, and so on, in which the “instrument” is a trained human being. In these disciplines, there is no specific validation of the “instrument,” and thus there would be no need for Analysts to engage in validation activities. Of course, one might think of quality control mechanisms as the equivalent of specific validation. Such activities would presumably be performed (or at least supervised) by Technical Managers and Laboratory Directors.

174. Roger G. Koppl et al., *Epistemics for Forensics*, 5 EPISTEME 141, 154 (2008).

those who work outside of government.<sup>175</sup> While such relationships are naturally fraught, problematic, and conflicted, with potential dangers that conventional wisdoms get entrenched or corporate interests get served,<sup>176</sup> I argue that they are less problematic than the status quo in forensic science.

### B. The Deskilling of Forensic Science

We also need to be candid about the fact that what we are proposing represents, in some sense a “deskilling”<sup>177</sup> of the profession of forensic science.<sup>178</sup> We are proposing to break the task of the forensic scientist, as classically understood, into segments that would be assigned to different individuals with different skill sets, educational backgrounds, expectations, and roles. We are proposing that some of the individuals, particularly the technicians, simply will not need to know, or even think much about certain things, and this may be construed as countenancing ignorance.

We need to be candid about this because an alternative, and reasonable, normative proposal exists which would move things in precisely the opposite direction. 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.”<sup>179</sup> The California School might argue that, rather than deskilling and segmenting the profession, we should be uplifting it. While they 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.” This is radically different proposal than the one I outlined above, in which, rather than differentiating roles, essentially, everyone in forensic

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175. See, e.g., ARTHUR A. DAEMMRICH, REGULATORY LAWS AND POLITICAL CULTURE IN THE UNITED STATES AND GERMANY, *in* REGULATION OF THE PHARMACEUTICAL INDUSTRY (JOHN ABRAHAM & HELEN LAWTON SMITH eds., 2003).

176. *Id.*

177. See HARRY BRAVERMAN, LABOR AND MONOPOLY CAPITAL: THE DEGRADATION OF WORK IN THE TWENTIETH CENTURY 424-47 (1974).

178. INMAN & RUDIN, *supra* note 111.

179. This is not to say that Inman, Rudin, and DeForest have identical views, but rather somewhat similar views that might be traced to a common orientation and approach.

science would have the skills and knowledge of at least Technical Managers and perhaps Basic Researchers. These Technical Managers would then approach *each case* as a scientific problem to be solved and deploy available forensic scientific tools as necessary to enhance our understanding of what might have occurred at the scene of the crime. Perhaps the best way to illustrate the difference between this mindset and the one that guides the proposal I outlined above, is to refer to DeForest's defense of "[o]ne person labs."<sup>180</sup> DeForest's claim, made not that long ago, that having a single individual with scientific training is a reasonable structure for forensic laboratories in small jurisdictions illustrates a very different conception of forensic science. In this view, cases are approached as problems by a single individual who possesses all of the tools encompassed by the term "forensic science"—bench skills, conceptual tools, and research background—and deploys these skills as appropriate to solve the problem.

This too is a compelling vision. I want to emphasize that I am agnostic as to whether it would be better to segment and differentiate the profession, as I have proposed above, or to uplift the entire profession to the doctoral (or near-doctoral) level. I think that having every individual working in forensic science trained to the level of Inman, Rudin, and DeForest would, by itself, solve most of the "the problems that plague the forensic science community."<sup>181</sup> I think that, to pose a historical counterfactual, had the profession developed so that everyone in it had the scientific training of Inman, Rudin, and DeForest, no one would ever have claimed that the error rate of latent print identification was "zero," and we would not have an NAS Report today.<sup>182</sup>

To be clear, I think either solution—differentiation or uplift—would be acceptable. I must note, however, that it seems clear to me that differentiation is a much more realistic proposal because it is more in tune with trends already widespread in forensic science that are independent of the issues raised by the NAS Report. These trends would include cost-cutting (the "uplift" strategy would be extremely expensive), managerial efficiency,

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180. Peter R. DeForest, *Recapturing the Essence of Criminalistics*, 39 SCI. & JUST. 196, 198 (1999).

181. Edwards, *supra* note 23.

182. I want to distinguish here between a philosophical principle, and a general impressionistic statement about the world. I am not claiming that the possession of a doctorate necessarily and automatically would prevent someone from making silly assertions, like "the error rate of latent print identification is zero" or "latent print identification is valid because it has been used in court for one hundred years." I am merely making the rhetorical point that I believe, based on little more than my own "sense of things," that, had the profession of forensic science been entirely staffed by individuals with doctorate, such uncharacteristic statements would have been far less likely to have appeared.

consolidation of laboratories, and so on. So, while I would applaud a mandate from Congress that all forensic science be performed by doctoral-level forensic scientists, I do not think such a proposal is realistic.

As sociologist of medicine Eliot Freidson notes, it is precisely this hierarchy of control through *internal differentiation* within the profession (i.e., a “knowledge elite” and rank and file practitioners) that makes medicine a “profession” rather than a “craft.” Crafts, in contrast, are subject to *external* regulation and control: “Without physicians serving in both roles, the profession could only sustain a position that is at best like that of the crafts, dependent on its organization but at the mercy of others’ technological innovations and administrative practices.”<sup>183</sup> Thus, I would suggest, it is perhaps only through adoption of this medical model that forensic science can live up to the subtitle of Inman and Rudin’s book, “The Profession of Forensic Science.”<sup>184</sup>

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183. Freidson, *Reorganization*, *supra* note 153, at 30.

184. INMAN & RUDIN, *supra* note 111.