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Forensic culture as epistemic culture: The sociology of forensic science

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ABSTRACT

This paper explores whether we can interpret the notion of 'forensic culture' as something akin to what Knorr-Cetina called an 'epistemic culture'. Can we speak of a 'forensic culture', and, if so, how is it similar to, or different from, other epistemic cultures that exist in what is conventionally called 'science'? This question has important policy implications given the National Academy Science's (NAS) recent identification of 'culture' as one of the problems at the root of what it identified as 'serious deficiencies' in U.S. forensic science and 'scientific culture' as an antidote to those problems. Finding the NAS's characterisation of 'scientific culture' overly general and naïve, this paper offers a preliminary exploration of what might be called a 'forensic culture'. Specifically, the paper explores the way in which few of the empirical findings accumulated by sociologists of science about research science seem to apply to forensic science. Instead, forensic science seems to have developed a distinct culture for which a sociological analysis will require new explanatory tools. Faithful sociological analysis of 'forensic culture' will be a necessary prerequisite for the kind of culture change prescribed by external reformist bodies like the NAS.

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1. Introduction

The notion of 'forensic culture', as explored in this special section and the conference that produced it is a flexible one. Various contributions deploy this term to mean a variety of things ranging from race and ethnicity to cultural (or media) representations of forensic science to the impact of forensic science on popular culture. In this paper, I take the term 'culture' to refer to something closer to Knorr-Cetina's 'epistemic culture' or, as a recent report on forensic science by the U.S. National Academy of Science (NAS) would have it, 'scientific culture'.[1] In this sense, 'culture' is a social medium that produces knowledge, knowledge that is often conventionally labeled 'scientific'. This paper explores whether and how it is possible to append the term 'forensic' to this notion of culture. Is it possible to speak of a 'forensic culture' that produces scientific knowledge? Would such a culture simply be 'scientific culture' or would it be different, and, if so, how?

An underlying assumption of the exploration undertaken here is the idea that sociology of science represents an appropriate framework from which to endeavour to answer this question. Sociology of science seeks to understand the social process of knowledge production, and over the past several decades, it has produced a corpus of empirical observations on this processes. Most, though by no means all, of these observations have been derived from studies of what we might call 'research science' aimed at producing new knowledge about the natural world at the expense of more mundane activities such as industrial science or 'regulatory science', despite full awareness that within the universe of things we call 'science' such 'mundane' activities may well exceed activities we call research science.[2] To be sure, one important impulse of early science studies was to contest precisely this equating of 'science' with high science experiments. This produced a line of research which valorised 'mundane', quotidian, practical, hands-on scientific work: tacit knowledge, invisible technicians, good hands, and so on.[3]

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1 Knorr-Cetina (1999), National Research Council (2009), p. 125.
2 Abraham (2002), p. 312, Shapin (2007), p. 183.
3 See e.g. Barley & Bechky (1994), Collins (1985), Doing (2004), Jordan & Lynch (1993), Shapin (1994).

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A couple of prescient early contributions aside, early sociology of science devoted little attention to forensic science.[4] Some legal scholars have drawn on sociology of science to examine forensic science and some psychologists and legal scholars have endeavoured to analyse—and, indeed, critique—forensic science from the general viewpoint of the social sciences.[5] One such piece is titled 'A Sociological Perspective …', though it was, in fact, written by a psychologist, not a sociologist.[6] While these works present useful observations of the social behaviour of forensic scientists and the institutional structure of forensic science, they tend to rely on explicit or implicit comparisons with the sort of idealised notions of 'real science' that sociologists of science have spent some decades debunking. Sociologists of science, meanwhile, have decried the failure of legal scholarship to better avail itself of the findings of sociology of science.[7]

The turn of the science studies gaze to forensic science is generally traced to an edited volume on expert evidence and a special journal issue, though mention should also be made of an important monograph on expert evidence and some brief discussion in Jasanoff's seminal book *Science at the Bar*.[8] Over the ensuing decade, a large body of excellent, broadly science studies, work on forensic science emerged. It can no longer be said that sociology of science ignores forensic science. Nor can it be said that law completely ignores sociology of science, though, perhaps not surprisingly, law appropriates sociology of science for its own ends.[9]

It may, however, still be argued that this literature still does not fully address the issue of how forensic culture impacts knowledge production. Its focus, to the extent that one can generalise, has been largely on the following themes: the ultimate contestability of all forensic knowledge claims and, despite this, the ability of forensic science to enact social resolutions of questions of truth;[10] the historical[11] or ethnographic tracing of the passage of forensic objects through laboratories and courtrooms and the instantiation of these objects into things like 'evidence' or even 'identities';[12] the effort to discipline forensic objects and practitioners alike through mechanisms like 'quality assurance';[13] the writing of attributes like 'race' into seemingly neutral forensic traces;[14] and the incorporation of profit into the analysis of forensic evidence.[15] Less present, however, in this body of literature is a return to the epistemological questions that—in part—motivated the sociology of science in the first place.[16] Do the findings of science studies about research science translate seamlessly to *forensic* science? Or, is there something different about forensic scientific knowledge and knowledge-making that requires different explanatory tools?

Thus, one motivation for this study is the possibility that the exploration of an 'epistemic culture' quite distinct from the 'epistemic cultures' usually studied by sociologists of science may contribute to the sociology of science itself. A study of forensic culture might contribute to sociology of science in much the way that the study of 'regulatory science' has.[17] A second motivation for exploring the notion of a forensic epistemic culture derives from a policy question. In its 2009 report, the NAS posited that there was a 'culture' problem in U.S. forensic science. The Report described the 'culture of science' as an important missing ingredient in at least parts of forensic science, and it devoted a substantial portion of one of its eleven chapters to this notion of 'scientific culture'. It stated 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'. Further, it touted 'scientific culture' as a potential antidote to what one of the Committee co-chairs called elsewhere 'The Problems That Plague the Forensic Science Community'.[18] 'The forensic science disciplines will profit enormously by full adoption of this scientific culture'.[19] Moreover, the Report asserted that 'a culture that is strongly rooted in science' is a 'minimum' criterion for the new federal agency it proposed creating, the National Institute of Forensic Science (NIFS). At least one of the members of the Committee that wrote the report has particularly emphasised the need for a 'culture of science' distinct from 'law enforcement culture'.[20]

Of course, this use of the term 'culture' by the NAS raises many questions for science studies. What does the NAS mean by this term, and is its meaning consistent with way the term is used broadly within science studies, to the extent that even social scientists use this term consistently?[21] I have argued elsewhere that science studies scholars would find the NAS Report's discussion of 'scientific culture' inadequate. The discussion consists largely of the sort of recitation of Popperian hypothesis testing and Mertonian virtues of scientific behaviour that are so common in lay discussions of science today. The Report repeats the common error of treating these features as universal attributes of everything meriting the label 'science' and as demarcation criteria through which 'science' can be clearly distinguished from non-science. Even on the rare occasion on which the Report recognises that Popperian concepts do not map well onto forensic practice, it quickly dismisses the objection, asserting, absurdly, that Popperian hypothesis testing can easily be mapped onto routine forensic practices.[22]

There is nothing especially surprising about this. Nor should this point be construed as a criticism of the Committee or the Report. The important work of the Report is accomplished elsewhere, and philosophy of science was probably among the least important things for this particular Report to get right. Moreover, the mere fact that the Report drew on what would be considered by philosophers a naïve appropriation of outmoded philosophy of science

[4] E.g. Hamlin (1986), Jordan & Lynch (1993), Oteri et al. (1982).
[5] For the former, see e.g. Redmayne (1997); for the latter, Moriarty & Saks (2005), Risinger & Saks (2003a, 2003b), Saks & Faigman (2008).
[6] Thompson (1997).
[7] E.g. Caudill & LaRue (2003), Edge (2003), Edmond & Mercer (2002, 2004), Jasanoff (1992).
[8] Smith & Wynne (1989) and Lynch & Jasanoff (1998), Jones (1994), Jasanoff (1995b), p. 42ff.
[9] Cole (2009), Edmond & Mercer (2002, 2004).
[10] E.g. Bal (2005), Dahl (2009), Derksen (2000), Edmond & Roach (2011), Golan (2000), Halfon (1998), Innes, Fielding, & Cope (2005), Jasanoff (1998), Kruse (2010), Lynch (1998), Lynch, Cole, McNally, & Jordan (2008), Mnookin (2001), Pugliese (1999, 2002), Rees (2010).
[11] E.g. Alder (2007), Aronson (2007), Burney (2000, 2006), Caudill (2009), Finn (2009), Golan (2004), Johnson[-McGrath] (1994).
[12] E.g. Jordan & Lynch (1998), Timmermans (2006), Toom (2006), Williams (2007).
[13] E.g. Leslie (2010).
[14] E.g. Cole (2007), Kahn (2008), M'charek (2000, 2008).
[15] E.g. Daemmrich (1998), Lawless & Williams (2010).
[16] But see, e.g., Edmond (2000, 2001, 2002, 2011a, 2011b), Edmond et al. (2009), Edmond & Mercer (2002), Edmond & Roach (2011), Haack (2001, 2005).
[17] Abraham (2002, p. 312), Jasanoff (1995a).
[18] National Research Council (2009), pp. 39, 114, 125, Edwards (2009a).
[19] National Research Council (2009), p. 125.
[20] Mearns (2010), p. 433.
[21] E.g. Pickering (1992), p. 8.
[22] Cole (2010), p. 440.

does not necessarily mean that the notion of a forensic culture is not worth considering. The gist of the NAS Committee's concern was, in my view, clear: There is something fundamentally different about what we call 'forensic science', something that makes it different from much, if not all, of those multitudinous and variegated activities to which we assign the more general term 'science', and this difference has a lot to do with why so many relevant actors are dissatisfied with the current state of things in forensic science.[23] The NAS Report posited far too easy an answer when it suggested that all that was needed was that forensic scientists 'adopt[] scientific culture' by following Popper's recipe for hypothesis testing and adhering to Mertonian virtues, but that does not mean that some empirical investigation of forensic culture might not be of modest assistance in thinking about how to, as the Report described it, 'strengthen[] forensic science', just as sociological analysis of 'regulatory science' might help produce 'good' regulatory science.[24]

In other words, I am proposing an exercise embodied by the field known as 'social epistemology':[25] how does the current social environment of forensic science affect the production of forensic knowledge, and how can we design a social environment that maximises the production of accurate forensic knowledge? The application of social epistemology to forensic science has been explored most thoroughly thus far by Koppl.[26] However, where Koppl has focused deeply on issues of competition and capture, here I am exploring somewhat more broadly the impact of forensic culture on knowledge production.

In this paper, I propose to take on this problem by approaching forensic science as what Knorr-Cetina called an 'epistemic culture'. To be sure I am making a somewhat broader use of the term 'epistemic culture' than Knorr-Cetina makes in her book of that title. But, in that book, Knorr-Cetina acknowledges this broader notion: 'On one level, the notion of epistemic cultures simply refers to the different practices of creating and warranting knowledge in different domains'.[27] Similarly, I am proposing to explore the culture of forensic science, much in the way that other science studies scholars have explored the culture of 'regulatory science'.[28] In mustering the collective findings of sociology of science, I will be drawing broadly from both what might crudely be characterised as the 'old' sociology of science that followed in the tradition of Merton and the 'new' sociology of science that followed in the tradition of the strong programme in the sociology of scientific knowledge (see Lynch, this volume). While these traditions have often been at odds, both, of course, have made recognisable contributions to our understanding of science as a social institution. As we shall see, for purposes of identifying those characteristics that distinguish forensic science from research science, it will usually be unnecessary to arbitrate whether it is the 'old' or the 'new' sociology of science which offers the more accurate account of modern science. Forensic science will be distinct from the 'science' described in both accounts.

In order to proceed, a number of caveats are in order. First, it should be noted that when I refer to 'forensic science' and 'forensic scientists' in this paper, I am referring to individuals who primarily work on cases. Most, but not all, of the individuals so described do this work employed by a forensic laboratory, and most, but not all, of those laboratories are state-run. My discussions of 'forensic scientists' are not intended to describe individuals whose primary occupation is scientific research whose application happens to lie in forensic science. So, to choose an arbitrary example, Sargur Srihari, who is employed full-time as a Professor of Computer Science at the University at Buffalo and has done research on the individuality of twins' friction ridge skin (fingerprint) patterns and on likelihood ratio models for assessing the value of latent print associations, is not a 'forensic scientist' for purposes of this paper; he is a 'research scientist'. He does controlled laboratory research, not casework, and he is employed by a university, not a forensic laboratory or law enforcement agency. While Srihari may experience slight 'cultural' differences from his colleagues in areas like funding opportunities and the reception of his work, for purposes of this paper, it is argued that the culture in which he works is very much like the culture experienced by his faculty colleagues.

To be sure, the distinction I am drawing is an oversimplified one, and there are many individuals who occupy a liminal space between the categories I have drawn: 'scientist' and 'forensic scientist'.[29] These would include, for example, both individuals employed by forensic laboratories and independent or privately employed individuals who have the luxury (or burden) of dividing their time between casework and research. While I acknowledge the existence and importance of these liminal individuals, their existence is ignored for purposes of the schematic discussion of a distinct 'forensic culture' which I wish to lay out. What is said in this paper about 'forensic culture' probably applies only partially to such individuals and institutional settings.

Second, I want to draw attention to another meaning of the term 'culture'. If there is a 'forensic culture', it is unlikely to be a unitary thing, but rather multiple 'forensic cultures' that intersect with national cultures. My remarks in this paper pertain primarily to American 'forensic culture', and they may pertain more or less to other national forensic cultures. Third, in positing a notion of 'forensic culture', I am necessarily lumping together a variety of different disciplines that differ from one another in various ways. Such a discussion will not necessarily apply equally to all these disciplines, and DNA profiling, in particular, will often, but not always, be an exception to some of my comments.[30] Nonetheless I do assert that the forensic disciplines have enough in common to be discussed coherently under the auspices of the term 'forensic culture'. Fourth, by using the term 'culture', some readers may assume that my data is ethnographic. My data primarily consist of texts, such as published articles and legal briefs, opinions, and transcripts. Some of my observations, however, derive from my role as what might be called a 'participant-intervener' in debates surrounding the validity and legal admissibility of forensic science over more than a decade.

Finally, and perhaps most importantly, the term 'research science' is used here to distinguish science which seeks to produce new knowledge from forensic science. However, there are vast swaths of activity that we label 'scientific' that are also not necessarily 'research science': regulatory science, industrial science,

23 Pyrek (2007).
24 Abraham (2002), p. 331.
25 E.g. Fuller (1992).
26 E.g. Koppl (2005, 2010), Koppl et al. (2008), Whitman and Koppl (2010).
27 Knorr-Cetina (1999), p. 246.
28 E.g. Abraham (2002), Jasanoff (1995a).
29 I am here following the practice of Jasanoff (1995a), p. 282 who treated 'regulatory science' and 'research science' as 'ideal types'. I hope it goes without saying that these are analytic, not pejorative, terms. My intent is to describe different cultures associated with social activities that I label 'science' and 'forensic science'. None of this discussion should be construed as defending the superiority, epistemic or otherwise, of one or the other. Likewise, I am explicitly *not* trying to express the commonplace notion that 'the problems' with forensic science can be adequately described simply by saying that it is 'not science' without further specifying what those supposed problems are.
30 See Lynch (2012).

medical science, engineering science and so on. My argument in this paper is not merely that forensic science differs from research science; such a claim would be trivial and tell us little about what is distinct about forensic science. After all, much 'science' that goes on in a university, such as routine laboratory assays, is *also* distinct from 'research science'. Rather, my argument is that 'forensic culture' can be characterised as a culture that is distinct *even from* other epistemic cultures that are not research science: regulatory science, industrial science, medical science, engineering science, routine laboratory procedures, and so on.

2. Specificity of knowledge claims and data

Perhaps the most commonly made observation about forensic science focuses on the contrast between the open-endedness of scientific inquiry and the temporally limited nature of legal truth-finding processes. While law must settle on a truth within a period of time set by the end of the case, research science, in principle, recognises no temporal limits on inquiry into truth. Forensic culture enacts the former principle; it does not enjoy the luxury of temporally open-ended inquiries, though such extraordinary practices as cold case review and post-conviction litigation to some extent cut against this generalisation.[31] This point is well taken, but it is in some sense only one aspect of larger differences between research science and forensic science with regard to the specificity of their knowledge claims and the nature of the data they employ.

Philosophers and sociologists of science have both endeavoured to explain how scientists can make *general* knowledge claims. The twentieth century philosophy of science advanced both by Popper and his critics has focused on the making of general statements about the natural world.[32] While there is great disagreement among those who study the production of scientific knowledge as to how these general knowledge claims can actually be made, broadly speaking, general knowledge claims rest upon converging lines of evidence from a variety of different scientific activities (experiments, observations, models, etc.), each of which is, ideally, based upon large amounts of data. Recent debates within philosophy, sociology, and history of science have focused on how and when these converging lines of evidence become what we call 'scientific knowledge'.

Forensic scientists, in contrast, do not seek to develop general knowledge claims at all. They do not seek to produce generalisable truths about the natural world that transcend space and time. Rather, they seek to make only *specific* knowledge claims that pertain to a particular place and time, what Shapin has called 'sciences of the particular'.[33] The data they work with are similarly specific. Forensic scientists do not enjoy the freedom to generate and collect data enjoyed by scientists in more conventional specialties.[34] Instead, they are usually limited to the data recovered by police or technicians in the investigation of a particular incident. Essentially, they must develop knowledge from a *closed set* of data, a posture that most academic scientists would find unfamiliar. The criminal incident itself may be conceptualised as a single data point from which the forensic scientist seeks to construct knowledge. Since so much of contemporary science is based on the statistical analysis of large data sets, contemporary scientists would find the notion of producing knowledge from a single data point unsettling.

3. Reproducibility

Reproducibility, often called 'replication', has been considered by many philosophers to be a key—if not *the* key—hallmark of 'science' precisely because it tests the generalisability of scientific knowledge claims. A scientific knowledge claim, if valid, should hold in places and times other than where and when the claim was initially proposed. Replication has also played a key role in sociology of science. Sociologists pointed out that replication studies provide little social capital in the prestige economy of academic science. Therefore, replications were rarely actually performed. Among the most celebrated findings of sociology of science is Collins' notion of the 'experimenter's regress'. Collins showed that no single replication experiment could function as a definitive referendum on a theory in the way proposed by some philosophies of science. Instead, he showed that cherished theories could survive failures to replicate by positing *ad hoc* explanations and that there was no natural limit to scientists' ability to resort to such explanation (hence the notion of a 'regress').[35] For our purposes, it is not necessary to arbitrate whether it is philosophers or sociologists who give a correct account of the role of replication in the development of scientific knowledge. It is sufficient for us to note that whereas replication often *does* not occur in science, it almost always *cannot* occur in forensic science. This is because forensic knowledge claims are specific, rather than general, and the data upon which such claims may be based are limited to the evidence collected pursuant to a particular criminal event.

It may be thought that the process of double-checking, sometimes called 'verification', which is quite common in forensic science protocols today, constitutes replication. However, such activities produce what philosophers of science call 'repeatability', rather than 'reproducibility'. Repeatability refers to repeating the same analysis on the same materials by the same researchers, in the same laboratory. Reproducibility refers to reproducing the original researcher's experimental or analytic setup and procedures by other researchers, with different materials, in another laboratory.[36] Reproducibility is necessary to ensure the results are 'universal' or 'general' and not somehow explained by some peculiarity of the original researchers, materials, or laboratory. Reproducibility, thus, is rarely possible for forensic knowledge claims.

4. Reward structure

Sociologists of science have thoroughly discussed what has been called the 'reward system' of contemporary science.[37] Academic scientists—producers of basic scientific knowledge—are not supposed to be motivated primarily by money.[38] Of course, this is not to minimise the role of money or to deny that it does play a modest role in motivating scientific work. There are numerous ways in which money is deployed to reward scientific work: patents, technology transfer, and so on. It is well known that the influence of these incentives even on contemporary academic science has increased enormously in recent decades.[39] Nor is it to fail to recognise that the certain stereotypical contrasts between 'industrial' and 'academic' science, while never all that tenable in the first place,

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[31] Innes & Clarke (2009).
[32] Putnam (1979), p. 265.
[33] Shapin (2007), p. 183.
[34] This is, again, a generalisation and is not meant to minimise the difficulties of data generation and collection in all areas of science, ranging from physics to social science to medicine.
[35] Collins (1985).
[36] Radder (1992), p. 64, Radder (1993).
[37] E.g. Ben-David & Sullivan (1975), p. 206, Cole & Cole (1967), p. 382, Merton (1957), p. 642.
[38] Zuckerman (1970), p. 236.
[39] Krimsky (2003), Owen-Smith (2005).

are becoming increasingly blurred.[40] Nonetheless, it may still reasonably be argued that even today money is not the primary incentive in contemporary science, which operates as a prestige economy in which reputation, or 'professional recognition', is the currency that matters[41]—'the coin of scientific realm'.[42] Scientists earn 'social capital' through such activities as publishing highly cited research reports in prestigious outlets, presenting research reports at prestigious conferences, earning prestigious academic posts, winning accolades from their peers, and so on.[43]

The high value of this 'currency', of course, underlies many of the self-regulatory mechanisms that supposedly govern and police the production of scientific knowledge. In theory, it is because of the deterrent effect of the implicit threat of losing this currency that scientists avoid plagiarising, falsifying results, corrupting the peer review process, or conducting other forms of scientific fraud and misconduct.[44] The existence of these implicit threats, in theory, serves as society's guarantee of the validity of scientific knowledge: understanding that scientists live in a complex prestige economy surrounded by implicit threats to reputation for bad behaviour and rewards to reputation for good behaviour supposedly gives us reasons to trust the knowledge claims that emerge seemingly 'certified' by the scientific community through superficial markers of prestige like journals, books, conferences, and so on.

The reward structure of forensic science is completely different. Forensic scientists' 'productivity goals' are forensic reports, not academic papers and conference presentations. Rather than largely autonomous free agents operating within a prestige economy, most forensic scientists, like the police that some of them used to be or still are, are employees of bureaucratic organisations and are subject to the sort of reward structure typical of such organisations.[45] As such, they would appear to be even less autonomous than industrial scientists, who themselves are thought to be less autonomous than academic scientists, even if that difference has perhaps been exaggerated.[46] Their 'productivity goals' appear to be the number of reports produced over time, rather than, as for scientists, their impact on knowledge production as evidenced by such measures as citation, much as the performance of regulatory scientists appears, at least to some extent, to be measured by the rate of production of, say, drug evaluations.[47] The audience for these reports is not other scientists but courts. Even the rather halting trend toward privatisation of forensic laboratories has not noticeably altered this situation.[48] This difference in audience itself generates several other cultural differences between research science and forensic science.

5. Adversarialism

Science is supposed to be self-critical, to engage in what Merton called 'organized skepticism'. Scientists are supposed to constantly question knowledge claims, to take nothing on faith. To be sure, sociologists of science have shown that this principle often functions more as an ideal than as a practice: most scientific work would be either impossible or unmanageably slow if every knowledge claim were questioned, if assumptions were not made, if some cherished 'facts' were not accepted on faith. Nonetheless, the spirit of the ideal continues to play a large role in organised practices designed to vet scientific knowledge claims, such as the anonymous peer review that serves as the gatekeeper for admissions to scientific journals (and in some fields conference presentations), the awarding of grants, and appointment to academic posts and promotions. To be sure, no one is naïve enough to believe that peer review exposes all, or even most, faulty scientific knowledge claims. Rather, most sober observers understand peer review as a modest quality assurance procedure that functions as a mild deterrent against poor, or even fraudulent, scientific work.

Scientific peer review may, to some extent, be characterised as 'adversarial'. Proper peer review is understood to require a certain critical stance on the part of the reviewer. The reviewer is expected to look for flaws. The homology between this critical stance and the adverarialism that lies at the heart of Anglo-American legal systems—in which the truthful account of events is supposed to be revealed as such by its ability to withstand vigorous efforts to undermine it—is clear.[49] This superficial similarity notwithstanding, however, the adversarialism of scientific peer review is not entirely equivalent to legal adversarialism. As Jasanoff found for 'regulatory science', we might say that forensic science is often even more adversarial than research science.[50] Journal referees, for example, are expected to be rigorous, but not unreasonably so: they are expected to make warranted and justified criticisms, but not to engage in radical scepticism or to criticise simply for criticism's sake. Academic laboratories are not subject to the sort of 'audit culture' that is increasingly being demanded of forensic laboratories.[51] In countries with adversarial legal systems, however, forensic scientists' conclusions are thrust into an adversarial legal process in which some actors (typically the defence attorney) may be ethically bound to engage in any possible form of criticism. Though these criticisms are only rarely enacted in any particular case, forensic scientists operate under the continuous awareness that they are possible; they operate, as it were, in the shadow of adversarialism.[52] A result is that forensic scientists tend to think defensively, anticipating possible adversarial attacks.

Adversarialism has a number of practical consequences. Disciplinary actions, failed proficiency tests, failed laboratory accreditations, audits, exposed errors, and scandals all can become fodder for adversarial cross-examination. Defence attorneys are trained—and, arguably, ethically bound—to seek to use any of the above such incidents from the past to impugn the conclusion offered in the present. Whether such tactics are effective or not, forensic scientists internalise a perception that, if they do not have spotless records, they will be subject to withering cross-examination. Legal adversarialism thus creates an atmosphere in which any admission of error is seen as forever impugning the credibility of the scientist, the laboratory, the discipline, or all three. This is even more the case for techniques, such as latent print analysis, which historically made exaggerated and implausible claims to being 'infallible' or error free. An ethos emerges in which any error or disciplinary action is viewed as too much, as permanently

40 Shapin (2008).
41 E.g. Latour & Woolgar (1979), pp. 187–230, Mulkay et al. (1975), p. 195.
42 Merton (1957), p. 644.
43 Bourdieu (1999 [1975]), p. 33, for social capital. More generally see e.g. Abraham (2002), p. 323, Cole & Cole (1967), Latour & Woolgar (1979), pp. 69–88, Zuckerman (1970).
44 Merton (1957).
45 E.g. Sylvestre (2010), p. 451.
46 Shapin (2008), Varma (1999).
47 Abraham (2002), p. 323.
48 Lawless & Williams (2010).
49 Roberts (2013).
50 Jasanoff (1995a), p. 288.
51 Power (1997), Giannelli (2007), National Research Council (2009).
52 Smith (1989).

marring the credibility of the witness, the laboratory, or even the whole discipline.

Of course, no human endeavour can proceed without mistakes, so forensic science as an institution has historically turned to defensive measures in a perhaps well-intentioned effort to forestall adversarial defence attorneys from thwarting justice by magnifying exposed errors, disciplinary actions, and so on into spurious 'problems' with the credibility of the scientist, laboratory, or discipline. Forensic scientists—with some good reason—have historically become accustomed to suspect that any request for information from a defence attorney, or even from the media, from academic scholars, or from an expert retained by the defendant, has the potential to become fodder for impeachment of them or their colleagues.

The way to prevent this from happening is, of course, to keep such materials out of the hands of 'outsiders'. There are many techniques for accomplishing this. Much information is simply never compiled in the first place. For example, the number of internal disagreements within the laboratory is almost never recorded. Records of errors are often not compiled at all. Even if they are compiled by some laboratories, they are not made publicly available and thus must be requested through discovery in a particular legal case. The discovery process is itself problematic. Defendants, who do not know what information the laboratory holds, tend to issue broad discovery requests that prosecutors, with some justification, characterise as 'fishing expeditions'. However, without such broad discovery requests, fundamental items of information, like the number of exposed errors committed by a particular laboratory in a particular discipline, will never be known.

Some materials, like individual analysts' results on proficiency tests, are treated as personnel records and thus are subject to elaborate legal protections. Whatever the legal merits of this position, the effect is that neither defendants, jurors, nor any other 'outsider', such as a researcher or the press, can obtain information about the analyst's performance on such tests. Since the analyst's performance tests would seem to be a relevant piece of information for weighing the value of their conclusion, this would seem to deprive defendants and jurors of crucial information.

Forensic scientists have no way to distinguish requests aimed at impeachment from 'legitimate' requests for information from, say, academic researchers or the press that are not intended to be used for impeachment. The result is that forensic scientists and forensic institutions view all requests for information from 'outsiders' with equal suspicion. The belief among some forensic scientists that their work helps solve crimes may contribute to the belief that actions like discovery requests represent obstructionist tactics that may endanger public safety, rather than necessary measures to ensure justice.[53] Forensic culture is thus characterised by a tendency toward 'obscurity' which is precisely contrary to principle of 'transparency', data-sharing, or the Mertonian norm of 'communism' to which mainstream scientists adhere—at least in principle, if not fully in practice.[54]

Of course, transparency is an ideal for mainstream science, not a practice, and thus it can be viewed equally well as a rhetorical resource in debates over claims to being 'scientific'.[55] It is not at all uncommon for scientists to refuse to share data with one another.[56] Because transparency *is* an ideal, though, it can function as a rhetorical resource for scientists engaged in disputes over data. Scientists can shame their adversaries by publicising their refusal to share data, and other scientists will sometimes interpret such refusals as evidence that there is something suspicious about the data. Forensic scientists, in contrast, are relatively impervious to accusations of lack of transparency. Such accusations are made, of course, chiefly through discovery requests by defence attorneys, but also occasionally by academic researchers or the media. But forensic scientists are not subject to the sorts of reputational consequences that might befall an academic scientist who persisted in a well-publicised refusal to share data. Indeed, forensic scientists appear, with good reason, to view the release of information as a greater threat to their reputations than refusals to release.

Beyond fear of impeachment, forensic scientists' experiences with the adversarial systems lead them to have more negative views about the adversarial process than scientists do about peer review. While it is common for scientists to complain about peer review—its perceived unfairness, poor quality, and time-consuming nature—even these criticisms are not as wholly negative as forensic scientists' attitudes toward the adversarial process. Crucially, academic peer review is a mutual process. Most working scientists serve on both ends of the peer review process: as reviewers and reviewees. Scientists' negative experiences as reviewees are tempered by their experiences as reviewers, in which they themselves experience how difficult it can be to judge one's colleagues' work. Forensic scientists only play one role in the adversarial process: as reviewees. They thus experience adversarialism merely as a relentless barrage of disparagement, rather than as a difficult process of trying to ferret out truth through vigorous criticism. Forensic scientists only rarely express true belief in the ideal of adversarialism—the belief oft expressed in Anglo-American legal circles that adversarialism is perhaps unpleasant and imperfect, but is nonetheless the best process for determining truth in a legal forum. Instead, forensic scientists seem to view vigorous adversarialism as merely a method to thwart justice on behalf of guilty criminal offenders.[57]

There is perhaps another reason that forensic scientists view adversarialism as a negative attribute of their practice. In theory, scientists are taught to believe that adversarialism has salutary effects on knowledge production. Either adversarialism weeds out bad theories or data, through refusal of publication, funding, tenure, etc., or adversarialism improves knowledge; discussion among the principals leads to better knowledge. Such a view of forensic knowledge production is untenable. While forensic scientists may claim to have healthy discussions when they disagree on the interpretation of evidence, such disagreements inherently cause problems for the forensic laboratory. The laboratory must either report the disagreement, and thus undermine the value of the evidence, or conceal the disagreement, and thus be open to internal or external accusations of lack of candour.

6. Audience and reporting of results

As discussed above, the output of most academic research science is various forms of scientific communication (papers, conference presentations). The primary audience for these communications is other scientists. Most scientists' primary motivation is to impress other scientists, and, indeed, scientists insist that only fellow scientists are competent to judge their work.[58]

53 Charlton et al. (2010).
54 Risinger & Saks (2003a), Risinger & Saks (2003b), p. 1045.
55 Lynch et al. (2008), p. 332.
56 Fienberg (1994), p. 1.
57 Roberts (2012).
58 Bourdieu (1999 [1975]), p. 33, Mulkay et al. (1975), p. 195, Zuckerman (1970), p. 236.

Of course, this is not to deny that scientists frequently seek to impress other audiences: venture capitalists, policy-makers, and so on.[59] But even these non-scientist audiences often rely on indirect markers of scientific prestige (journal ranking, academic affiliation, etc.) to evaluate scientists' knowledge claims.[60] While it is true that most scientific papers are rarely cited, let alone read, on occasion scientific communications do influence other scientists, contributing to what philosophers of science would call 'robust research programmes'—incremental agglomerations of diverse sources of scientific knowledge that accrete into a some improved understanding of some aspect of the natural world, result in new technologies, and so on.[61] The *intended audience* for most scientific work is thus an audience of technical specialists, individuals who specialise in a small subfield of a scientific discipline, who are able to read and understand a highly technical language specific to that subfield, a group that Collins dubbed the 'core set'.[62] The primary attitude of this audience is indifference; as noted, most papers are simply ignored.[63] On rare occasions, however, the audience may take notice of a paper and engage with it. The reaction may sometimes be hostile—the paper will be challenged, attacked, criticised, or, on rare occasions, someone may even attempt to gather new data to refute its claims. Or the reaction may be friendly—the paper may be endorsed, celebrated, cited favourably, and, on rare occasions, fellow scientists may even take up those 'suggestions for further research' that, by convention, conclude most scientific papers but usually reflect more an adherence to an expected modality of scientific rhetoric than any actual expectation on the part of the author that such research activities will ever take place.

Indeed, scientists adhere to a host of rhetorical conventions in communicating their knowledge claims to this audience of colleagues.[64] One such convention is that scientists are very careful about what they claim. It is generally better to claim too little than to expose oneself to potential attack or refutation by claiming too much. This calls for what might be called 'epistemological modesty' in framing knowledge claims. This is not, however, *merely* a rhetorical convention; often the inherent limitations of the data in any particular scientific paper warrant very modest knowledge claims. Another common convention is the presentation of results in some sort of statistical form. Such presentations can take a variety of forms, but all may be characterised as trying to convey the uncertainty that surrounds any scientific finding or report. Thus, statistical presentation of results may, for example, serve to convey the measurement error inherent in the instruments used and experiments conducted, convey the likelihood, however small, of seeing the reported results if the articulated knowledge claim were *not* true, or convey the expected degrees of accuracy and precision that should be associated with any predictions made on the basis of the articulated knowledge claim.

Rather than other scientists, the *audience* for forensic science is the criminal justice system (police, prosecutors, judges, jurors), or, to put it more broadly, the state.[65] These audiences differ from an audience of scientists in several ways. First, unlike a scientist audience, they lack general scientific training in such things as how to frame an empirical question, how to statistically characterise the results of an analysis, how to pose competing hypotheses. Second, unlike a scientist audience, they lack specific training in the particular subfield from which the analysis derives—they are not members of the 'core set'. They are, instead, the 'law-set'.[66] Third, unlike a scientist audience—with the exception of one actor, the defence attorney, whose ethical commitment to 'adversarialism' was discussed above—they do not regard it as part of their job to be critical of the results that are presented to them. Police and prosecutors are inclined to view forensic results as helpful for building a case.[67] Judges are extremely welcoming of forensic science, more so, it would seem, than nearly any other form of expert evidence that is offered in court. A number of explanations for this have been offered, including trust in government and pro-prosecution bias .[68] Most judges in the U.S. are former prosecutors.[69] The favourability of judges toward forensic science may even derive from public safety concerns: judges view forensic science as crucial for protecting the public from crime. If this is indeed the case, it is understandable that judges may constitute an extremely friendly audience for forensic science. Particularly telling in this regard are the comments of Judge Edwards, former Chief Judge of the United States Court of Appeals for the District of Columbia Circuit and Co-Chair of the NAS Report that was so critical of U.S. forensic science, who noted that, despite *not* watching television programs like *CSI,* he spent his entire judicial career 'simply assum[ing]. .. that forensic science disciplines typically are grounded in scientific methodology and that crime laboratories and forensic science practitioners generally are bound by solid practices that ensure that forensic evidence offered in court is valid and reliable'.[70] Even without indulging overblown claims about the supposed 'CSI effect', it is fair to say that jurors attribute a great deal of credibility to forensic evidence.[71]

In sum, the criminal justice system provides an extremely welcoming forum for forensic science. Far from the indifference or organised scepticism from a highly technically trained audience faced by conventional science, forensic science presents its results to an audience with no commitment to the principle of organised scepticism, with no technical ability to scrutinise or question methods or results, which views forensic science as an extremely useful tool with which to further their own goals of efficiently delivering justice or even as a crucial bastion against criminal threats to public safety. Whereas scientists hope to generate social consensus around their knowledge claims within the 'core set' of scientists most technically capable of understanding their work and most trusted by other scientists to evaluate it, forensic scientists rarely need to win consensus from their peers. Typically, they merely need to win 'acceptance' from the friendly, non-technical audience constituted by the criminal justice system, or the state. To be sure, there may be rare occasions, especially in areas highly given to disagreement like forensic pathology, in which their work will be challenged by a peer. But we should not allow the few highly publicised instances in which this *does* occur obscure the

59 Shapin (2008).
60 Brewer (1998).
61 Zuckerman (1970), p. 237, for citation practices; Lakatos (1978), for research programmes.
62 Collins (1981)
63 Latour (1987), p. 400.
64 E.g. Bazerman (1988), Dear (1991), Shapin (1984).
65 Smith (1989), Thompson (1997), p. 1114.
66 Edmond (2001).
67 Of course, this is only true of forensic results consistent with their theory of the crime. Not all forensic results fall into this category. However, under the best of circumstances, such results are unlikely to proceed further in the case. Under the worst of circumstances, as has been documented in a number of notorious U.S. cases, such results may be minimised, dismissed with *ad hoc* explanations, or even concealed (Garrett & Neufeld, 2009, p. 76).
68 For trust, see Dwyer (2007), p. 391; for bias, see Cooley & Oberfield (2007), p. 285, Risinger (2000), Risinger (2007), p. 475; Rozelle (2007), p. 597.
69 Risinger (2010), p. 244.
70 Edwards (2009b), p. 2.
71 Cole & Dioso-Villa (2009), Podlas (2006), Tyler (2006).

vast majority of instances in which it does not. Moreover, this process differs from research science in that peer forensic scientists are rarely able to muster different data and methods to evaluate forensic knowledge claims. Due to the restricted nature of forensic evidence, peer forensic scientists are often limited to deploying the same methods upon the same data.[72]

In presenting their results to this audience, forensic scientists do not adopt conventions characteristic of conventional science like carefully-phrased epistemologically modest knowledge claims and statistical characterisation of results. Quite the opposite, forensic scientists tend to offer claims with an epistemological strength that most conventional scientists would find surprising—e.g., 'the defendant is the source of the fingerprint', 'this glass derived from this window'. And, instead of a statistical analysis, these claims are communicated in vernacular formulations that are remarkably vague—e.g., 'the evidence matches', 'the evidence is consistent', 'the pattern is rare', 'I've never seen anything so similar', 'the defendant is probably the source', 'the defendant is the source to a reasonable degree of scientific certainty', 'the defendant is more likely than not the source', and so on. Often these verbal characterisations are categorical in nature –'match' or 'non-match', 'source' or 'not source', and so on. Such dichotomous characterisations of results are, of course, directly contrary to the sort of statistical characterisation of results—a mode of communication that seeks to capture and convey uncertainty, rather than dismiss or erase it—that is so dominant in most research science. In this sense, the product of forensic science is its very certainty. While this is beginning to change, through the presentation of forensic DNA evidence, which has sometimes functioned as sort of lever to encourage the reporting of all forensic evidence in a statistical manner,[73] the slow progress and staunch resistance to such changes illustrates that what is being attempted is a change in culture.

7. Research Agenda

One consequence of this difference in audience is a difference in the setting of the research agenda. There has been much debate within philosophy, history, and sociology of science over how the agenda of research science is set. We need not arbitrate here the debates over whether the agenda of research science is set by intellectual curiosity, 'research programmes', 'paradigms', or even the needs of corporate capitalism, because forensic culture differs from all of these accounts.[74] The research agenda in forensic science is set largely by the needs of the audience, which is to say those criminal justice system actors which fund and employ forensic science. This would include primarily law enforcement agencies and prosecutors, though the needs of judges, who have ultimate say over whether the evidence can be used in court or not, are important as well. In practical terms, this has meant a fairly robust research agenda aimed at extending instrumental capabilities of forensic science: developing new detection, recovery, and imaging techniques, new analytic techniques, and new modes of deploying those techniques.[75] As noted by many commentators—and now by the NAS as well—the research problem that has been neglected has been validation: studies aimed at measuring the accuracy of the aforementioned techniques. As these commentators have noted, the explanation for this lacuna appears to lie with the needs of the audience for forensic science: courts have not demanded such validation studies. Thus, there was little incentive to do them—even perhaps a disincentive, given that the courts were willing to allow forensic scientists to testify without giving fact-finders any information about accuracy. The result has been a curious culture in which the validation of an assay, if performed at all, has tended to follow, rather than precede, the use of that assay. As examples, we can note the lack of validation studies of latent print identification, handwriting identification, firearms and toolmark identification, and bitemark identification, forensic DNA analysis, and comparative bullet lead analysis for years, or sometimes decades, after the inception of use of those techniques in court.[76] Consider also Thompson's arguments about research on forensic DNA profiling being driven by adverse legal rulings rather than by, say, intellectual curiosity or desire to disprove arguments about which one is sceptical.[77]

8. Feedback

On rare occasions, the 'testimony of nature' itself may even be mustered in response to some scientific knowledge claims.[78] New experiments may be performed, independent data sources may be invoked, and other researchers may assess whether newly developed research findings are consistent with particular claims. Though this process may be rare, and some philosophers of science have been accused of overstating both its prevalence and its necessity for generating scientific knowledge, none of this is to say that the process never occurs or is not important for the production of scientific knowledge. At some points in the development of scientific knowledge, in other words, theory gets tested against independent data (though only rarely in the sorts of definitive ways that have become the stuff of popular myth): the scientist gets 'feedback', so to speak, from nature. This feedback may take many forms, such as the continuing failure of an experiment, continued difficulty explaining anomalous data, excessive necessity of modifying one's theory and so on. While claims that there is one unitary way in which theory is tested against data may have been overstated, there is little doubting that scientists in various disciplines have various mechanisms for 'sensing' that a line of scientific inquiry is 'on the wrong track'.

Forensic science is structured in such a way that valid feedback is rare. To be sure, there are odd occasions in which independent data does resist forensic science. Forensic scientists may occasionally come to conclusions that are refuted by mundane, common sense evidence. For example, a forensic conclusion that a suspect is the only possible source of a particular trace may be refuted by evidence that that individual was in prison at the time the trace was known to be deposited. Such occurrences are said to be infrequent—though no records of them are kept. In other cases, another analyst may disagree with the results of a forensic conclusion. Again, such occurrences are said to be uncommon—though no records of them are kept. Forensic results may also occasionally be refuted or challenged by the results of another type of forensic analysis.[79] Again, there are no records to indicate how often such events occur.

Far more often, forensic results are simply introduced into the criminal justice system as 'facts'. Other parties, such as the defen-

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72 Of course, there are exceptions to this generalisation. One obvious exception is voice identification in which a variety of different methods and expertises compete to perform the same task. Thus, the conclusions of a phonetician who tries to identify voices through experience-based listening might be challenged by an engineer who uses automated signal processing equipment. Morrison, (2009), p. 302).

73 Lynch (2013).

74 Varma (1999, p. 35), Lakatos (1978), Kuhn (1962), Krimsky (2003), Woodhouse & Sarewitz (2007).

75 Mnookin et al. (2011), p. 762, National Research Council (2010), p. 223.

76 Cole (2006), Risinger et al. (1989), Schwartz (2005), Beecher-Monas (2009), Thompson (1997), p. 1118, Imwinkelried & Tobin (2003).

77 Aronson (2007), Thompson (1997), p. 1122.

78 Lynch (2013).

79 E.g. Risinger & Saks (2003a).

Table 1
Contrasting Social Attributes of Research Science and Forensic Science

Social Dimension	Research Science	Forensic science
Time-frame	Open-ended	Limited
Data	Intentionally collected/generated (in most cases); in principle, unlimited	Adventitiously produced; inherently limited
Knowledge claims	General	Specific
Product	Papers and other scientific communications	Reports, affidavits, testimony
Reward structure	Prestige	Bureaucratic
Productivity goals	Volume and impact	Volume and speed
Accountability mechanisms	Scholarly peer review	Legal adversarialism
Audience	Peer scientists, 'core set'	Legal actors, the state
Reporting of results	Conservative, statistical, ambiguous	Ambitious, colloquial, unambiguous
Data sharing	Unlimited, in principle	Treated warily, sometimes prohibited by legal actors
Research agenda	Driven by 'research programmes', 'paradigms'	Driven by demands of courts, law enforcement, and the state
Valid feedback	Sometimes	Rarely

dant, may seek to challenge these 'facts' with other evidence or—often preferably—seek to posit an innocent explanation for them. But occasions in the adjudication of criminal cases in which independent data can be mustered as a sort of 'test' or 'check' on a forensic knowledge claim are scarce, with the most famous being those cases in which forensic DNA profiling is used 'post-conviction' to 'test' the conclusions reached by prior forensic analyses.[80] In short, justice systems desire results, but they have no way of determining the accuracy of the results they receive. This differs from academic or industrial science. Though the valid feedback may not be as common as some idealised models of science would claim, false results that form part of a larger body of knowledge may be expected to have their falsity detected by the end-user eventually at least reasonably often.

9. Conclusion

Table 1 summarises the social dimensions discussed above upon which forensic culture seems to be distinct from the more general 'scientific culture'. The similarity of this table to Jasanoff's table comparing 'regulatory science' and 'research science' will be apparent.[81]

This paper has sought to illustrate that if we are able to speak meaningfully of a 'forensic culture', it is a culture quite different not merely from the culture associated with research science but also from the epistemic cultures associated with other 'sciences of the particular'. It is hoped that this paper illustrates the value of both the existing sociological literature on this peculiar culture, as well of future research. However, this conclusion carries policy implications as well. This paper suggests that the solution posed by mainstream scientific institutions like the NAS—that forensic science 'adopt[] scientific culture'—while perhaps a noble idea, is unrealistic. It is unrealistic not merely for the oft-stated reason that forensic scientists and those who employ them have evinced resistance toward such goals. More importantly, the social structure of forensic science is fundamentally different from that of research science. Changing 'forensic culture', if indeed that is what is desired, will require far more than the recitation of Mertonian norms and recipes for hypothesis testing. Instead it will require something closer to an exercise in social epistemology: deliberate thinking about what sort of 'culture' will be conducive to producing whatever it is we want from forensic science. It is not at all clear that the result of such an exercise would be to set a goal of making forensic science as much like research science as possible. If, however, that were the goal, it would have a long way to go.

[80] Garrett & Neufeld (2009).
[81] Jasanoff (1995a), p. 283.

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