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OVERVIEW

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Is the (traditional) Galilean science paradigm well suited to forensic science?

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Abstract
For more than 10 years, forensic science has been at best, criticized for its lack of scientific foundations and at worst, presented as an oxymoron. An exclusive focus on standard operating procedures and quality management could cause forensic science to fall short of addressing the epistemological issue initiated by judges. This is particularly so in rapidly changing times, including digital transformation of society and decentralization of forensic services. As a consequence, the present understanding of forensic science by both scientists and its stakeholders is questioned. It is argued that that forensic science fundamental principles and, more broadly, forensic science philosophy are pivotal to the reliable application of science to address security and justice questions.

This article is categorized under:
Forensic Science in Action/Crime Scene Investigation > Epistemology and Method
Jurisprudence and Regulatory Oversight > Communication across Science and Law
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KEYWORDS
epistemology, forensic science, paradigm, security & justice, understanding

1 | INTRODUCTION

For more than 10 years all forensic science disciplines have been criticized, at least in part, for providing formal identification conclusions or more broadly conclusions that apparently lack scientific foundation (Ciedel, 2012; Dror, 2012; Koehler & Saks, 2010; Mejia, 2017; Neufeld & Scheck, 2010; Page, Taylor, & Blenkin, 2011; Saks, 2010; Saks & Faigman, 2008; Saks & Koehler, 2008). This situation led to a stream of quality management solutions with the intent of eliminating "junk" science from the courtroom. Although the intent was sound, this approach reinforced the silo culture in forensic science, by restricting forensic inputs to well-established identification techniques. However, it did provide an improved but still disparate academic involvement (Canter, 2004; Fleming, 2012; Fyfe & Wilson, 2012; Guillaume, Sidebottom, & Tilley, 2012; Kelty & Julian, 2011; Paterson, 2011; Rojek, Alpert, & Smith, 2012; Ross, 2015; Roux, Talbot-Wright, Robertson, Crispino, & Ribaux, 2015; Steinheider, Wuestewald, Boyatzis, & Kroutter, 2012). The current situation could fall short of satisfying both the scientific and legal communities, without a better understanding of science and its forensic offspring (Crispino, Ribaux, Houck, & Margot, 2011; De Forest, 1999; Evett, 1996; Judge Edwards, 2012). Although provocative, this claim could be highly relevant at a time when the combined digital transformation of society and the decentralization of forensic services challenge the traditional position of the forensic science laboratory, specifically its position in relation to field investigation. A consequence of this is the rapid merging of digital traces with their chemical, physical and biological siblings (Casey, Ribaux, & Roux, 2018a, 2018b; Huey & Nhan, 2013; Kloosterman et al., 2015) (Box 1).

BOX 1 From digital forensics to digital forensic science

A new field, digital forensics, is emerging from digital transformation. A new community, peer-reviewed journals and conferences promote knowledge sharing in this rapidly developing domain. The new community is mostly composed of computer scientists and investigators with a technical background. So far, this new field has essentially been absent from traditional forensic science conferences. In many ways, it is reinventing the wheel and failing to take into account the transversality of forensic science and what has gone before.

Forensic science laboratories struggle to integrate digital activities, beyond specific high-tech services (e.g., signal and image processing or data extraction from many types of devices). Digital activities are mainly organized by the police for reasons of timeliness and contextualization.

The context surrounding the rapid development of digital forensics could pose the same problems that forensic science has recently faced in many areas (e.g., feature comparison methods), however, on a much larger scale incorporating privacy issues and extended risks of miscarriages of justice. Indeed, what is changing today with traces of a new nature to interpret and value are the orders of magnitude of volume, variety, and rapidity.

The next challenge in this space is how *digital forensics* can become *digital forensic science*. That is how the epistemological debate on forensic science can integrate digital transformation. This will rely on the capacity of the criminal justice system, as a whole, to develop solid, transparent, proportionate, and balanced approaches to manage the "information society."

Reflecting on the roots of the criticisms leveled at forensic science in recent years, this paper proposes a contextual epistemological approach to address them, challenging the mainstream paradigmatic view of forensics and opening the debate about its current practice.

2 | NEAR-TO-BE-RESOLVED FORENSICS CRISIS?

Despite previous, sometimes old, warnings of the limits of forensic science conclusions (Appell, Darboux, & Poincaré, 1904; Huber, 1991), it is obvious that the NAS report, followed by the PCAST report, have prompted international reaction to reform forensic practices (NAS, 2009; PCAST, 2016), even though these reports focused on the U.S. criminal justice system and its more than 400 city, county, state, and federal forensic facilities. Private forensic science facilities and practitioners were not included (Evett, Berger, Buckleton, Champod, & Jackson, 2017; Giannelli, 2008; Kaye, 2010). Canada made a general, comparable introspective critical analysis of forensic practice in the Hart House report. It proposed a number of solutions to cure similar symptoms of deficient forensic disciplines and appeared more balanced than the U.S. reports (Pollanen, Bowes, VanLaerhoven, & Wallace, 2013). For instance, the Hart House report did not address the required separation of forensic laboratories from prosecutor offices as in the NAS report (although still not implemented in the USA). However, both the U.S. and Canadian reports supported the development of quality management schemes such as certification and accreditation, a better understanding and management of human biases, a greater investment in research, training, and graduate education and a closer association with universities—even if the best model for education and research remains unclear (Denmark & Hackman, 2016; Margot, 2011; Robertson & Roux, 2018). Similarly, both the U.S. and Canadian reports largely excluded crime scene management as a field of interest. This exclusion of crime scene management from the scientific domain is quite disturbing. The crime scene is the space where the most critical decisions are made and these decisions should be scientifically-led. Further, crime scene examiners are generally the main, if not the only evidence collectors in the forensic analytical chain. Potentially, they are also first providers of forensic analysis (Bitzer, Albertini, Lock, Ribaux, & Delémont, 2015; de Gruijter, Nee, & de Poot, 2017; Julian, Kelty, & Robertson, 2012).

The existence of fragmented scientific communities involved in forensic science is presented in these reports as a major reason for failing practices. While this is a reasonable claim, it is quite difficult to understand how the reports' main focus on quality management could help defragment these communities, and how it would directly improve justice and security (Crispino & Roux, 2017; Evison, 2018; Hazard, Stauffer, & Margot, 2013; Ross, 2013; Willis, 2014). By bringing the new digital forensics field (and all its subfields) into an already fragmented environment, the complexity of the situation increases. Different systemic constraints governing police, science, and justice organizations potentially amplified by the marketization of forensic science are exacerbating factors (Kolowski, 2015; Lawless, 2011; Lawless & Williams, 2010).

These institutional wake-up calls did lead to some positive outcomes such as detecting miscarriages of justice, enforcing traceable analytical protocols, identifying the lack of empirical support for some forensic conclusions, and more confident sharing of forensic data (Gerard et al., 2017; Gill, 2008; McCartney, 2015). However, they fell short of addressing the area of interpretation of evidence, which is obviously of overlapping concern for lawyers and scientists (Biedermann, Bozza, Taroni, & Aitken, 2017; Cheng, 2017; McQuiston-Surrett & Saks, 2009). Communication and transparency of expert opinion are presented as key components to resolve some of the issues raised. However, the interpretation challenge still remains a point of friction between the legal and scientific communities, questioning the boundaries of the responsibility of science versus the law and how best we can deal with the interface (Berger, Buckleton, Champod, Evett, & Jackson, 2011; Champod & Vuille, 2011, 2015; Kaye, 2012; Roberts, 2015; Thompson, 2012).

Of interest, neither of the afore mentioned reports, nor the scientific community have adequately addressed serious criticisms related to police management and justice administration that are at the forefront of overseeing the admissibility of evidence and administrating its resources. It could mean that forensic scientists, be it during the course of investigation, or as expert witnesses in the courtroom, rightly consider themselves as only auxiliaries to the trier of fact or decision-maker. As such, forensic scientists are hardly in a position to criticize their stakeholder's understanding or misunderstanding of science. This could be a main point of friction, as the definition of science may differ between these communities, reinforcing the confusion between the "scientificity," admissibility and even the instrumentality of science (Black & Daeid, 2015; Brodeur, 2008; Ludwig, 2016; Mousseau, Baechler, & Crispino, in press; van Brakel & De Hert, 2011).

The US Federal Rule of Evidence 702 (FRE 702) states that a witness qualified as an expert by knowledge, skill, experience, training, or education may testify in the form of an admissible opinion if (Library, 2017):

a. The expert's scientific, technical, or other specialized knowledge will help the trier of fact to understand the evidence or to determine a fact in issue.
b. The testimony is based on sufficient facts or data.
c. The testimony is the product of reliable principles and methods.
d. The expert has reliably applied the principles and methods to the facts of the case.

Outside the United States, similar criteria to admit science in courts exist (Edmond & Vuille, 2014; Vuille, 2013), for example, section 79 of the Uniform Evidence Law in Australia.

Points a and b were relevantly addressed by the NAS report recommendations (Zimmerman, 2011). Points c and d tackle the question of the reliability not only of scientific methods (covered by quality management process), but also of the ground principles. In other words, we could argue that such rules require scientists to understand (1) the fundamental principles of their science, that is, forensic science and (2) the very nature of the object of interest of forensic science, the trace. However, in practice, it appears that most practitioners and stakeholders focus on methods or, at best, include some discussion about principles of the methods as opposed to principles of the science. The question ought to be asked: is there a gap in practitioners' and stakeholders' fundamental understanding of forensic science and, if yes, can we address it?

3 | THE CASE FOR AN EPISTEMOLOGICAL ANALYSIS OF FORENSIC SCIENCE

FRE 702 was adopted in response to the Supreme Court decision with respect to Daubert v. Merrell Dow Pharmaceuticals, Inc. (Supreme Court of the United States, 1993a). A brief return to this hearing could help understand not only the reason of a new proposed U.S. framework to accept science in court (so much has been written to challenge the Frye (1923) and Daubert (1993) rulings), but also the concept of science held by judges of the Supreme Court. Before questioning the definition of science depicted by this hearing, it seems relevant to remind that dissenting opinions of Chief Justice Rehnquist and Justice Stevens occurred, fearing the consequences of judges defining what science is: "I do not doubt that Rule 702 confides to the judge some gatekeeping responsibility in deciding questions of the admissibility of proffered expert testimony. But I do not think it imposes on them either the obligation or the authority to become amateur scientists in order to perform that role" (Supreme Court of the United States, 1993b).

Relying on Hempel's logical empiricism and Popper's falsifiability principle, the opinion of the Court identified a methodology as scientific when scientists took the position of "generating hypotheses and testing them to see if they can be falsified," but also insisted that the overarching subject of FRE 702 was the "scientific validity […] of the principles that underlie a proposed submission."

It then provided some criteria to help the trier of fact to differentiate science from pseudoscience (whether a theory or technique can be [and has been] tested, whether the theory or technique has been subjected to peer review and publication, the consideration of known or potential error rate and then the general acceptance of the relevant scientific community). Although not exhaustive in the eye of the Court, these criteria are all clearly at the methodological level, rather than the level of principles addressed by the Daubert hearing. It confuses the very nature of science with its method, and makes it difficult to identify science in its own right. The Popper-Hempel theory is at the core of the US Supreme Court's definition of science. How does it accept as scientific an explanation of a past event, which defines historical sciences (Cleland, 2001) and is essentially the basis for a forensic science investigation? Hempel and Popper agree that, although such an explanation would depend on a singular premise(s) (the initial conditions), some explicit or implicit universal covering laws of nature have also to be present to logically deduce the observed singular event at hand. The scientificity of these covering laws is the key feature to accept the conclusion as scientific, that is, the explanation of the past event. They claim that historical sciences raise issues for the theory of scientific meaning: few of the innumerable historical explanations put forward are founded on covering laws, whether explicit or implicit. When they are, the proposed covering laws are either spurious or untrue (Donagan, 1964). Hence, whatever the acceptance by the court of the principles of experimental sciences (chemistry, physics, optics, electricity, thermodynamics, biology, computer science, etc.), forensic conclusions could still be scientifically questioned, because their principles, that is, the covering laws of forensic science (e.g., Locard's transfer principle and Kirk's individuality principle), could be spurious or untrue!

Kirk's individuality principle (Kirk, 1974, pp. 9–10) is not natural. It is first and foremost logical as previously established by Wittgenstein: "5.5303 [...] To say that two things are the same is devoid of meaning, and to say that it is identical to itself is to say nothing at all." (Wittgenstein, 1922, p. 88). The case is more serious for Locard's principle according to the epistemology dictated by the US Court: "As much as the Locard transfer theory has been invoked, no peer-reviewed literature exists that tests, supports, or refutes it. It is axiomatic in forensic science; it is accepted as true without proof" (Inman & Rudin, 2001, p. 94). Worse than any criticism raised by the NAS or the PCAST reports, isn't forensic science an oxymoron (Kennedy, 2003)? It would be better to first understand where this epistemology comes from, before throwing the baby out with the bathwater.

Hempel and Popper were close to, while not members of the Vienna Circle (1924–1936), a group of mathematicians, logicians, physicists, economists, and researchers, who regularly met, to formalize and issue a universal theory of knowledge, according to the views of Russell and Wittgenstein. That is making philosophy scientific with the help of modern logic (Carnap, 1966; Hempel, 2001; Reichenbach, 1951; Russell, 1961; Stadler, 2005; Wittgenstein, 1922). This group emerged as a reaction to German metaphysics, led by Kant, Marx, Freud, Adler, Heidegger to name a few, who, themselves tried to counter Hume's rejection of the ability to discover general laws of nature though induction.

Popper was particularly struck by the a priori risk that Einstein would have taken asking for the rejection of his entire theory if the curvature of sun's rays was not observed during the 1919 eclipse. The adoption of this Einsteinian approach is all the stronger for Popper, as he turned dialectic materialism or scientific marxism into a pseudoscientific dogma in the absence of systematic confirmation of the predictive power of this social theory. The analogy with Freudian psychoanalysis leads to a similar conclusion: marxism and psychoanalysis are unscientific, as they are irrefutable, or capable of always finding unverifiable explanations for the nonrealization of their predictions. Tricks in pseudoscience to mimic this attitude is to propose ad hoc hypotheses, as such as imprecision of measuring instruments or insufficient controlled samples to observe the awaited effects (Popper, 1985a). In line with experimentation, Popper seeks a criterion for distinguishing science from other human activities (Popper, 1973). He concludes that the ultimate goal of science is the search for an interesting truth, that is, with high explanatory power, which cannot be reached directly by inductive experience of confirmation: "What we are trying to do in science is to describe and (as much as possible) to explain the reality. We do it using conjectural theories, i.e. theories that we hope are true (or close to the truth), but it is not possible for us to establish that they are certain, or even probable" (Popper, 1991, p. 94). In the end, logically speaking, scientific certainty is always negative: we can only be certain of what does not work. The goal of his falsifiability criterion was to address the logic behind the discovery of principles deserving a scientific label. However, he recognized the difficulty of applying it as researchers are attached by tradition, formation, and belief (possibly false) to theories they learned (Popper, 1985b, 1991). Moreover, as exciting as this methodology of acceptance of a scientific theory through falsifiability seems to be from a logical point of view, its construction seems purely philosophical and offers no concordance with the chronology and methodology of scientific discoveries that marks the history of science (Kuhn, 1977; Lakatos & Musgrave, 1999). Even Einstein's proposal of refutation of his theory is questioned from the historical point of view (Ackermann, 2014; Cartwright, 1983; Feyerabend, 2000).

The complexity of contemporary science renders it hardly testable through such a simple criterion, as scientific entities are never separate and distinct. The simple is always complex, at risk of illusion or error. The "wave-particle" and "space–time" associations bear witness to this. Rationalism becomes relative and conditional (Bachelard, 1960; Poincaré, 1968, 1990). Such a perception leads to another way of capturing science through the incompatibility between logical empiricism (e.g., Hempel, Vienna Circle, Popper) and the historical evolution of scientific knowledge. It introduces the Kuhnian epistemology of scientific paradigms encompassing a discipline that is necessarily historically and socially dependent (Kuhn, 1970; Polanyi, 1964). A paradigm is a matrix of the achievements of a discipline that defines a community. It can be anything as (Masterman, 1999; Mayo, 1996):

- A universally recognized scientific achievement.
- A myth.
- A philosophy or a constellation of questions.
- A complete tradition.
- A model.
- An analogy.
- A successful metaphysical speculation.
- A tool accepted in the common law.
- A background of conceptualization.
- A standard applied to quasi-metaphysics.
- An organizational principle that can govern perception itself.
- A new way to see or something that defines a large part of reality.

Such a meta-modeling of a scientific discipline is more understandable for forensic science, as "*the uniqueness of the trace forbids any knowledge, operating from this type of object, to constitute itself as a Galilean science, i.e. to give oneself models, theories, laws leading to deductive reasoning*" (Dulong, 2004, p. 267). It questions the relevancy, if not the understanding of the justice of one epistemology against another. Finally, it is worth remembering here that as induction and deduction are the routine reasoning types for experimental science to test a hypothesis, abduction is mandatory to propose hypotheses. It is a central tenet of the scientific investigation of crime where apparently improbable associations of traces (vestiges or remnants of a presence or an action(s) from the past (Margot, 2014)), are better explained by a common cause (Ginzburg, 1984; Kind, 1999; Nickolls, 1956). This preliminary scientific work looks like the "search for a smoking gun" (Cleland, 2013) (Box 2).

BOX 2 From experimental science to historical science

Experimental scientists evade the underdetermination of the future by testing false positives and false negatives. Their aim is to confirm or refute their hypotheses. Getting a false result is always a risk. Historical scientists exploit the overdetermination of the past by searching for a sign, a "smoking gun" (Cleland, 2011). Their aim is to discriminate amongst competing hypotheses. The challenge for them is recognizing traces for what they represent. Although the experimental and the historical methodologies are different, none is superior to the other (Cleland, 2002).

In forensic science, it is common to use instruments and analytical methods that have been developed and validated by experimentation. However, the value of forensic science resides in its ability to show how details of an event can often be reconstructed from the surviving traces. Experimental science provides the required tools to extract reliable information from theses traces. However, these traces must be appropriately detected, recognized, analyzed, and interpreted. The historical reconstructive nature of forensic science is obvious. The question should be asked: could the challenges faced by forensic science be partly due to the confusion *between experimental science* and *historical science* and the fact that the discipline is not assessed according to its most appropriate methodology?

Wouldn't it be better to recognize the forensic historical paradigm, assess its richness and potentials, and finally work constructively with it rather than attempting to ill-fit forensic science into the (traditional) Galilean science paradigm?

4 | CONCLUSION

Forensic science fundamental principles and, more broadly, forensic science philosophy are pivotal to the reliable application of science to address security and justice questions. This is particularly true and noticeable with the current digital transformations dramatically expanding the scope of the discipline. In this rapidly changing context, it is simply not possible to focus on separate or specialized methods anymore. A scientific, holistic and integrated approach is necessary to mitigate the risk of arbitrarily increasing the silo approach to forensic science. If the latter prevails, could we still speak of scientific investigation and wouldn't the Criminal Justice System be tainted by arbitrary decisions?

Raising the siege of forensic science only through technical management measures and without addressing its philosophy could fall short of satisfying both its stakeholders and practitioners. A better understanding of a science in its own right is needed to logically serve security and justice, through education, research, and collaboration transcending Galilean disciplines (Crispino, Rossy, Ribaux, & Roux, 2014; Robertson, 2014; Roux, Crispino, & Ribaux, 2012). We call for a paradigm change for forensic science; a change that the legal community could also benefit from (Pietro, Kammrath, De Forest, & Crim, 2018; Roux, Ribaux, & Crispino, 2018); a change that could also mitigate our final interrogation: is it not logical for a discipline that primarily learns from and mainly reacts to increasingly rare mistakes to progressively reduce its application domain up to the point where it will disappear?

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CONFLICT OF INTEREST

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