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Chapter 8

**Misconduct hunting**

Research integrity *via* law, science and

technology[[1]](#footnote-1)

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Today many elite scientists and experts from different parts of the world are part of virtual global networks, sharing more with one another than with many colleagues from their own countries (Haas 1992; Boulton *et al.* 2012; Riles 2011; Nielsen 2012). Actors within elite science receive similar education, read and write in the same journals, attend the same conferences, obey the same courtesy rules (Lam 2010: 15); in other words, they share a culture (Strathern 2000). A notable element of this shared culture is well-disseminated guidelines about ‘good research practice’.[[2]](#footnote-2) There is a lot to be said about what ‘good research practice’ has come to mean in the last few decades, and why it has been so minutely articulated in some contexts and left purposefully vague in others. In fact these ‘good research practice’ norms are currently being renegotiated, and hence should not be taken for granted (Biagioli 2003, 2012; Jacob 2013). For instance, there have been calls for expanding the definitions of research misconduct comprised of Fabrication, Falsification and Plagiarism (FFP) to include self-plagiarism, ghost authorship, non-disclosure of conflicts of interest, amongst others (Farthing and Wells 2008; Goodstein 2010). Montgomery and Oliver (2009) have traced three ‘institutional logics’ mobilised to deal with deviance in science in the United States. They note that prior to 1975, the discourse was about norms and counter-norms of ‘normal practice of science’.[[3]](#footnote-3) It then moved, between 1975 and 1990, to a focus on the prevention of scientific misconduct; and from the 1990s to the present, to promoting research integrity.

 In the last couple of years in the UK, various consultation papers and reports have been lamenting that the regulation of research is too burdensome, bureaucratic, and expensive.[[4]](#footnote-4) There have been calls from the government to cut the bureaucracy, with prominent actors and institutions advocating a ‘radical simplification’ (DoH 2010: 9) of the legal frameworks regulating research. The Department of Health ‘Report on the arm’s-length bodies review’ (2010) – which followed the publication of the White Paper ‘Equity and excellence: Liberating the NHS’ (2010) – has set out proposals to make the health sector more efficient and less bureaucratic by increasing accountability and transparency and reducing the number and cost of quangos. Amongst other things, the Report mandated the Academy of Medical Sciences to review current regulatory frameworks and plan a new pathway for regulating research. The Academy’s subsequent 2011 report suggested increasing the speed of decision-making, reducing complexity, and eliminating unnecessary bureaucracy and costs (see also Laurie and Harmon, this volume). In this context – where bureaucracy almost became a dirty word, and where traditional modes of state regulation by law are seen as inefficient and expensive – self-regulation through the use of science and technologies takes on special relevance (Lessig 1999; Black 2001). But when it comes to regimes of governance, do legal tools and motifs ever recede in the background? In contrast to the efforts of those interested in preventing misconduct, this chapter will instead examine how the idea of ‘research integrity’ itself has been translated into a regulatory agenda. What interests me is how ‘legal’ and ‘technological’ tools that govern contemporary science act to differentiate research ‘integrity’ from ‘misconduct,’ and hunt for misconduct. I shall further argue that these research integrity tools cannot, in fact, be neatly classified as either legal or technological. By drawing parallels between legal practices and peer deliberations by scientists, I also show how the specificity of the ‘legal’ and the ‘scientific’ is bound to get blurred. This has consequences for our own theoretical (or disciplinary) perspectives regarding the study of research integrity.

STS scholar Mario Biagioli suggests that norms within science, such as norms on scientific authorship, act as ‘a para-legal discourse predicated on the absence (perhaps the impossibility) of a Law’ (Biagioli 2007: 147). A first objective of this chapter is to modulate this claim, by showing *how* methods of regulation and moderation are not only used as a substitute for a law in this area but also how, in the area of research integrity, these methods themselves mobilise typically legal resources and modes of enunciation. Drawing upon ethnographic fieldwork in the Committee on Publication Ethics (COPE) over 2010–2012, I suggest that the work of participants in present-day research integrity agendas resonates with an old kinship shared between legal and scientific practices.

The regulatory agenda of scientific research integrity is characterised by striking similarities between the practices of regulatees and that of regulators. This phenomenon is distinct, and arguably far more intense, than that of ‘regulatory capture’ (Braithwaite and Ayres 1992) as it implies instead the sharing of the very same activities, tools, documentation practices and apprentices, between both parties to the regulatory encounter. Hence a second objective of this chapter is to explore the implications of one such example of simultaneous sharing of practices between regulators and regulatees, coming from the domain of the regulation of scientific conduct. My third objective is to compare past and current regulatory strategies to hunt scientific misconduct, and reconsider what sets them apart. In particular, the chapter seeks to provoke a re-thinking of the difference between so-called ‘old’ legal tools, and so-called ‘new’ scientific-technological regulation (Brownsword 2005; Lessig 1999) that have been applied to the governance of scientific work. Using the example of moderation of nineteenth-century medical publishing, I deflate the novelty (and hype) of regulation by technologies, and then reflect back on the evolving practices of COPE.

Law and society scholars Silbey and Ewick (2003) have declared that the new ‘authenticating’ site of science, where scientific truth is lodged, is no longer the lab, but the text, the publication itself. The milieu of scientific publication (and of publication ethics) has also become the site where some of the most interesting regulatory and self-regulatory initiatives within science take place. Before turning to this regulatory archipelago (Rose and Miller 2008: 218) that moderates the conduct of researchers, and hunts instances of misconduct, let me briefly contextualize my chapter within the movements that inspire this collection.

**Engaging socio-legal studies and STS**

STS and socio-legal scholars are increasingly exchanging their tools, methods and concepts, in order to study their respective objects of inquiry (Valverde 2005; Lezaun 2006; Cloatre and Dingwall 2013). The scrutiny of STS scholarship has opened some of the black boxes of law and regulation, and cast fresh light on socio-legal domains more generally (e.g. Prainsack, Turkmendag, Shapiro, this volume). Socio-legal scholars, on their part, have effectively unpacked various ways of ‘making order’ in science. ‘Legal-pluralist’ analyses that decentre ‘enforcement by the state’ (Braithwaite 1993; Black 2001) have made socio-legal approaches eminently relevant to study normative orders beyond state law, and tackle regulatory webs that involve all sorts of actors, including scientific ones.

Perhaps because of their trained eye and sensitivity to tailored legalities, lawyers and socio-legal scholars are well situated to study multiple forms of ‘disputing’ (Abel 1973) within science. For example, their acquaintance with texts that ‘tell us what to do’ (Constable 2008), may turn out to be useful when it comes to unpacking standards (Bush 2012) and other forms of ‘regulatory objectivity’ (Cambrosio *et al.* 2006) that proliferate within, and increasingly typify, biomedicine. Their familiarity with matters of statecraft and legislative modes of enunciation may help to tackle the power and authority of the government in managing dissent, consensus-making and reordering in science.

Perhaps most importantly, what the joint streams of STS and socio-legal works have shown us is that if one ‘attempts to capture the dynamics of knowledge processes’, rather than labelling knowledges statically or categorically as either scientific, or legal, or social, ‘one sees new things’ (Valverde

2005: 421). The cross-fertilization between the two fields gives hope for less of pre-conceived separation of units and more open-ended inquiries into what law and science can mean.

In the following pages, I use my object of inquiry to foreground a found intimacy between scientific and legal reasoning. Misconduct hunting, as we will see, is an evocative interplay between law, science and society.

**‘Witnessing’ and deliberating science**

I start by sharing some initial thoughts about innovative methods of deliberation used by the Committee on Publication Ethics (COPE), an organisation in which I have been conducting ethnographic observations on a quarterly basis since 2010. I will show that the deliberations of these participants can be analogised with forms of experimental work performed in laboratories (cf.

Bogner 2012), and that through their ways of ‘witnessing’ science, these participants juggle scientific but also long-standing legal traditions in their deliberations.

A number of organisations are involved in preventing misconduct, many of them groups of editors, including the International Committee of Medical Journal Editors (ICMJE), the World Association of Medical Editors (WAME), and the European Association of Science Editors (EASE). One of the most notable is certainly COPE, an international organisation which started in April 1997 as an informal group of medical editors convened by Michael Farthing (then editor of *Gut*, and now Vice Chancellor of the University of Sussex), Richard Smith (then editor of the *British Medical* *Journal*), and Richard Horton (of *The Lancet*). At that first meeting, about 15 medical editors gathered together at the British Medical Association (BMA) House in London: ‘We discussed cases, and I think that we found it interesting and all learnt something. It was a very informal atmosphere, and we laughed and had fun’, recalls Smith (Horton *et al.* 2012: 5). They kept meeting regularly to ‘tell each other stories’ (Horton *et al.* 2012: 5) but soon feared being perceived as a ‘kangaroo court’ (Horton *et al.* 2012: 5) and being sued for libel and slander. Following the advice of eminent professor of medical law Ian Kennedy, they began to anonymise all the cases under discussion, and made clear that they ‘weren’t making decisions […] simply offering advice’ (Horton *et al.* 2012: 5).

Initially a local group, today COPE is a large international network (though still physically operating from the UK). It is well known and consulted by the scientific and publishing community across the English-speaking world. Its Code of Conduct, and flowcharts ‘designed to help editors follow COPE’s Code of Conduct and implement its advice when faced with cases of suspected misconduct’,[[5]](#footnote-5) have been translated into many languages including Spanish, Portuguese, and Persian. A truly cosmopolitan organisation, COPE has Council members from Europe, Australia, Brazil, China, and Iran, and an ombudsman from Australia, who is flown over to London quarterly for Council meetings.

COPE is funded by annual subscriptions from a very wide range of publishers and individual journals, and works on the basis of membership (as of 2012, it had more than 7,000 members). Originally, journals would become members individually, but now publishers like Blackwell and Elsevier have registered all their journals, and therefore some journal editors might now be officially members of this organisation without knowing about it. Indeed, I myself became an Associate Member (available for those not working as editor but who have an interest in publication ethics) of COPE, for £50 per year, in order to gain better access to its materials and benefit from reduced rates to attend its seminars.

In 2012, COPE received an award from the Council of Science Editors (CSE) for its work on improving scientific communication and promoting high editorial standards. The award was displayed to all participants at a COPE meeting. The CSE award tells something about how COPE sees itself and is seen by others in the field: as a crucial actor in the promotion of good practice in science. The prize is also indicative of the increasing professionalization of the field of misconduct studies, as Biagioli has discussed. (Biagioli 2012: 467). Further to this, Richard Horton has recently said that COPE ‘has become a national treasure’ (Horton *et al.* 2012: 7). This might also indicate the implicit and under-examined Britishness of COPE (in spite of its international membership).

COPE’s distinctive activity is its Forum (where my on-going ethnographic fieldwork is taking place). COPE members call it a ‘self-help’ group for journal editors. The Forum is a place where suspicions of questionable conduct are discussed openly, and where participants share their experiences and offer suggestions of responses and advice as to what to do next: ‘It’s an open forum for the sharing of views, we don’t decide or judge’, as one member explained to me. The Forum meets quarterly – on the day of the administrative meetings of the Council – to discuss cases submitted to them by journal editors. Members of the Forum include Council members of COPE (the Council includes medical writers, journal editors, publishing consultants, academics, one lawyer), but anyone who has editorial functions for a journal member of COPE, or who works for a publisher member of COPE, can also come and contribute to the exchange in the Forum.

Any member can submit a case to COPE in advance of the Forum meeting, after which the case is anonymised, summarised in writing and circulated to all members prior to the Forum. At the Forum itself, the person who submitted the case presents orally, in person or by phone or videoconference, to the participants, and then participants openly discuss the case, share their own experience with similar cases, raise questions, share views, and debate – but, as the Chair pointed out to me, ‘without rendering a judgment’. The open-endedness is further highlighted by the format with which COPE publishes the cases: ‘Anonymised Text of the Case submitted’; ‘Advice’; ‘Follow-Up’; sometimes an ‘Advice on the Follow-up’; and ‘Resolution’. ‘COPE is an experiment’, wroteMichael Farthing in his first annual report (1998).[[6]](#footnote-6) Drawing on the seminal works of Shapin and Schaffer (1985), and Shapiro (2002), I now show how COPE can be understood as a regulatory space, and a laboratory.


 *Figure 1.1 Committee on Publication Ethics (COPE) Forum*

In *Leviathan and the Air-Pump*, Shapin and Schaffer show how, in the seventeenth century, Robert Boyle and his colleagues from the Royal Society formulated ways of conducting and reporting experiments that propelled ‘not only new regimes of fact-finding but also new regimes of governance’

(Jasanoff 2007: 22). The ‘witnessing’ of science practised through peerreview, for instance, helped to democratize science, and gradually imposed itself hand in hand with other democratic means of deliberation that would relieve citizens from the absolutisms of the monarchy. Shapiro (2002) points out that within the Royal Society, during this transformation of the conduct of experiments in the laboratory, long-standing legal conventions of legal witnessing and fact-finding were integrated in scientific enterprise and reasoning. Experiments and specific observations were debated within the language of law. Boyle himself often used legal terminology, for example arguing that ‘matters of fact had to be brought to trial’ (Shapiro 2002: 243). Shapin and Schaffer describe how he defined the laboratory:

In Boyle’s programme there was to be a special space in which experimental natural philosophy was done, in which experiments were performed and witnessed. This was the nascent *laboratory*. (Shapin and Schaffer 1985: 334, their emphasis)

Shapin and Schaffer also draw a connection between experimental knowledge, and legal knowledge produced in criminal court, in that both are based on ‘collective witnessing’. In law and science ‘the reliability of testimony depended upon its multiplicity’ (Shapin and Schaffer 1985: 334).

They explain further:

The thrust of the legal analogy should not be missed. It was not merely that one was multiplying authority by multiplying witnesses (although this was part of the tactic): it was that *right action* could be taken, as seen to be taken, on the basis of these collective testimonies.

(Shapin and Schaffer 1985: 56, their emphasis)

The Royal Society debated ‘matters of fact’ not only with the language of truth, but also of moral certainty. Members were interested in normative questions about thresholds of credibility themselves. Latour (1993), discussing Boyle’s use of legal metaphors (Shapin 1984), describes the scene almost ethnographically: ‘credible, trustworthy, well-to-do witnesses gathered at the scene of the action can attest to the existence of a fact, the matter of fact, even if they do not know its true nature’ (Latour 1993: 18).

I suggest that the COPE Forum similarly uses a legally-influenced mode of ‘witnessing’, as well as deliberation, as tools of regulation. The Forum appears to constitute a kind of informal dispute resolution arena, but also, a laboratory, an experimental space, a ‘place where this multiplicity of interests [is] addressed, acquitted, and drawn together’ (Shapin and Schaffer 1985: 340).

Circa 2010–2012, members of the COPE Forum came to examine ‘matters of fact’ and matters of conduct in specific contexts of disputes. They try to do both: determine what is, what has happened, but also how to characterise (or categorise) what is now to be done. They ask what is and what has happened by following the plot closely, asking follow-up questions about chronology, dates of exchanges, specific locations and jurisdictions. For example, members are interested in knowing where the research takes place, where the researchers come from (this alludes to the controversy over whether different scientific standards of conduct might apply to different countries; see Jacob 2013). COPE members also use analogies with previous cases to feed into and shape their discussion. In the course of case discussions, the first question the Chair addresses the Forum with is, recurrently: ‘has anyone experienced something similar before?’

The Royal Society was commonly confronted with conflicting testimony: gentlemen could conflict in the Royal Society ‘as they often did in the course of lawsuits’ (Shapiro 2002: 56).Members and spokesmen often compared the work of the Royal Society to the work of the court. However, in practice they carefully ‘avoided making collective judgments about matters of fact’ (Shapiro 2002: 56). Specific scientists may have asked the Royal Society to rigorously ‘investigate, examine, compare and also to declare judgment’ (Shapiro 2000: 126) in cases of disputes and controversies. However, the Society preferred to suspend judgment, instead making statements about the instruments used by the disputed scientists, their propensity or not as to ‘willingly falsify an observation’ (Shapiro 2002: 257). This suggests that the Society wished to restrict their mandate to examining the facts of methods and the facts of conduct of scientific experiments, but *not* scientific merit *per se*.

Likewise, COPE members are explicit about their wish not to take the role of an adjudicator: as one member observed to me, ‘We don’t judge, it’s very open-ended’. In the deliberations, issues of scientific merit are not discussed. What is key is the facts of conduct and of potential misconduct of scientists, not the worth of the science. Donald Kennedy (1997) points to that distinction as a feature of the tension between legal and scientific approaches to scientific conduct in the context of adjudication of scientific fraud allegations. In the context of such inquiries, he notes, the subject matter itself never gets disputed. It is the sequence of events, the order of manuscript drafts (or of email exchanges), and the minute details of records that come under scrutiny. Kennedy alleges that these are the kind of things that scientists absorbed with the technical content (of their science) might miss (or misinterpret); however they would hardly be missed by lawyers (Kennedy 1997: 232; see also Latour 2002).

I interpret the COPE Forum as a contemporary practical case that makes Boyle’s thoughts and experiment look suddenly much less part of a distant past. COPE emphasises that it is precisely because there are many views expressed, and many testimonies, and experiences shared, that it can offer the right advice (they are careful to state that this is not a *judgment*). Moreover, in the COPE Forum, it seems that there is an epistemic affinity between research and the mode of moderating it. Here it is deliberation, discussion of a case on the basis of the sharing of multiple accounts by multiple witnesses: an old legal technique, in renewed form? In other words, the scientific, experimental method proposed by Boyle and which, according to Shapin and Schaffer, can be analogised to natural philosophy and to criminal law, is re-performed in the Forum, to control and moderate deviations in science.

Until recently, the COPE Forum had been meeting face to face around a large oval table, in a boardroom in central London, inviting editors to present their cases in person, and also taking cases via phone from editors working all over the world. In March 2013 COPE held its very first virtual Forum meeting via webinar. The head of COPE, Virginia Barbour from *PLoS Medicine*, chaired the Forum from an office in Cambridge, with the technical assistance of COPE staff. For two hours, 49 participants from different parts of the world, presumably sitting in front of a computer, headsets on, read cases and guidelines on screen as pointed to by the Chair, and listened to editors presenting their cases. Some took a more active role, by ‘raising their hand’ with one-click button in order to technically be ‘unmuted’ by the moderator and to share their thoughts to the group via microphone. I was conducting my ethnographic observation sitting at a desk in my university office in the UK. The meeting seemed to have gone well and similarly to previous meetings in many ways, in the sense that opinions and disagreements were aired politely and firmly, in the usual manner. Parts of the flow of the Forum’s collective discussion, including spontaneous reactions, the occasional joke and ensuing collective laughter, the raised eyebrows and head-shaking that also characterise the Forum table, were for sure clogged by the media of the webinar. From COPE’s perspective, this meeting was a success, and made the Forum more accessible to a broader range of international members. COPE expressed that its intention to hold both virtual and face-to-face Forums from now on.

People make new social media in their own image, and not the other way around; yet, I cannot help but wonder: what will such virtual meetings mean for the future of this type of legally-influenced, ‘Royal Society style’ face-to-face deliberation about scientific research conduct? How will the ‘conduct of conduct of conduct’ (cf. Rose 2000) be reshaped in the process?

**Regulating (with) science and technologies**

What happens when science and technology are *simultaneously* (rather than alternatively) tools and targets? Can the governance of scientists’ conduct gain legitimacy, become more workable and efficient, precisely because it is grounded on science and technology (Miller and Rose 2008: 218)? Or: can control, regulation and moderation of research conduct be effective because, and perhaps only because, it is effectuated by a method of moderation that is grounded on, and mimics research itself?

Technologies of research integrity verification, such as electronic tracking, textual similarity algorithms and statistical analysis of submissions, are now well integrated in the machinery of scientific publishing. Based on the premise that technology can detect, prevent, repair and undo the mistakes (see Prainsack, this volume) but also the misconduct and improprieties people make, these technologies and the body of quantitative studies that has developed on and with them (e.g. Fanelli 2010) receive a lot of attention in research integrity circles.

‘Technologies of integrity verification’ exemplify what lawyers Brownsword and Yeung, borrowing from STS, call regulatory ‘technological fixes’ (2008), in that they make use of knowledge and technology to regulate and thus cure the problem of research integrity. Technoscience can now alternatively be a *tool* as much as a *target* of regulation (Brownsword and Yeung 2008; see also Flear and Pickersgill 2013; Cloatre and Dingwall 2013). Framed as tools to detect fraud in research, technologies of integrity verification entail what Biagioli calls a ‘depersonalisation and delocalisation of the evidence of misconduct’ (Biagioli 2012: 468–469): instead of focusing on the grievance of a particular author who has been plagiarised or otherwise wronged by misconduct, these technologies prevent misconduct at large by disciplining all scientists (see also Lewis and Atkinson 2011). In other words, these regulatory technologies aim at upholding an ethos, rather than preventing or compensating for harm. This is consistent with other contexts of professional self-regulation,[[7]](#footnote-7) as it emphasises the requirement that professional values (of integrity and ethics) be *shown* (Jacob 2012: 7).

One example of a technology of verification is statistics. It is currently a standard procedure for the most important medical journals to have a statistician on their editorial board, and to have any papers with numerical figures reviewed by statisticians. A statistician also sits on the board of the UK Research Integrity Office. Statisticians are able to detect data fabrication and falsification, on the basis of the concept of digit preference (Evans 2008). Digit preference theory asserts that particular numbers are preferred to be recorded or chosen, rather than having a uniform distribution. It is thus difficult to invent, or alter data without leaving ‘fingerprints’ (Evans 2008: 164). Statistics can also be useful to verify data integrity in the case of multi-centres treatment trials: here, the statistician can compare the data sets and to detect anomalous distribution of data.

Technologies of integrity verification also seem to have ‘computerized the detective’s role’ (Biagioli 2012: 468). Perhaps more dramatically, in a collection edited by prominent figures in the UK milieu of medical research integrity, statistician Evans describes his own discipline as an ‘omniscient

adjudicator’ so convincing that ‘no corroboration is needed’ (Evans 2008: 161). Statistics, as a discipline, is thus being self-promulgated by its practitioners as a judge. This judge behaves secretly, disinclined to reveal its precise methods of detection. The rationale for the vagueness found in writings about statistical methods to detect fraud in medical research, claims Evans, is that it is important to prevent fraudsters becoming more sophisticated and thus able to circumvent detection methods; when there is a code, there is a code breaker (Machado and Prainsack 2012: 57–72).

Textual similarities software is another mode of integrity verification. Focusing on the large-scale quantitative detection of textual similarity, these tools search for textually similar articles, whether published by different authors, or by the same authors. Hence these electronic tools are used to detect plagiarism, and also ‘self-plagiarism’ (i.e., ‘redundant’ or duplicate publications). Because there are far more instances of duplicate publications than of plagiarised articles, the engineers and proponents of textual similarity algorithms significantly increase their markets and audiences by mixing the ‘apples of plagiarism with the oranges of self-plagiarism’ (Biagioli 2012: 467) without pausing and asking what these forms of deviance have in common and what sets them apart.

The idea of self-plagiarism is noteworthy in two key senses. First, the term itself indicates again this notion that misconduct self-regulation is not about protecting others from harm, but about a scientific ethos and, here more particularly perhaps, about the relationship of a scientist to herself. The category ‘self-plagiarism’ suggests that a scientist’s respect for professional standards is a matter for regulatory remit. Second, with self-plagiarism, we might wonder whether the regulator, at least in part, might be technologically constructing its own regulatee and inflating its own regulatory target, hence creating the need for more technological fixes (cf. Biagioli 2012).

**Guarding against the quacks**

In spite of the alluring novelty of integrity verification entrepreneurship, nineteenth-century medical publishing practices suggest that the use of science and technology to regulate knowledge production is no recent innovation. In this sense it is helpful to examine medical journals’ past responses to the phenomenon of quackery. Throughout history, the quack has often been referred to as cunning, and as a confidence trickster, fraudster, and faker (Bartrip 1995: 191; Porter 2003: 313). It was every practitioner’s responsibility to guard the gullible public against it (Porter 2003: 312; see also Warner 2005), just as today scientists are responsible for making sure that people are not deceived by poor science (Biagioli 2012: 465).

There were dilemmas among medical reformers of the nineteenth century as to whether to suppress or eradicate quackery, or try to educate the quacks (Porter 2003: 309). The editor of *The Lancet*, Thomas Wakley, accused the medical establishment itself of being the source of quackary, and physician Thomas Percival had taken a tough approach towards quackery in his influential 1803 *Medical Ethics*. A petition to parliament was presented to outlaw unqualified practice, to protect legally qualified practitioners and suppress quacks (Porter 2003: 309). The British Medical Association’s work against quack medicine included campaigning for legislation forcing manufacturers to reveal ingredients of secret remedies, the restriction of sales, prosecution of those in breach, as well as public relations and persuasion of mainstream newspapers not to publish advertisements for ‘imposters’. Practitioners, politicians, corporations and medical editors allied together to propel legislative changes, but also felt there was a need to move beyond the law.

Campaigns against quackery related to the ‘politicisation of medicine’ (Porter 1989: 222). The late nineteenth and early twentieth century was a critical period for the professionalisation of medicine, and medical innovators and the ‘old guard’ of the traditional elite were fighting for control over the provision of medical services and cures (Bynum and Porter 1986). This was also a period of growth for medical journals, which, for financial reasons, included plenty of advertisements of quack medicines. Bartrip (1995) reports that in 1904 Dawson Williams, then head of the BMA, commissioned an analysis of proprietary medicines by reputable pharmacist Edward Harrison in order to identify and reveal the ingredients of these medicines. Harrison’s results, along with data on the price of these medicines were published by Williams as a series in the *British Medical Journal* in 1904–1908, thus uncovering that a range of medicines with little ingredients of value were sold at

high prices. Bartrip notes how great an achievement this had been for the *British Medical Journal*.

One constant between twenty-first-century technologies of research integrity verification and nineteenth-century pharmaceutical science’s detection of counterfeit medicines is that science and technology themselves are, and were, being used in self-regulation settings, to regulate and monitor the behaviour of scientists. The alleged reasons for doing so are, in both settings, to protect the public, and (hence) maintain public trust. More salient here is the epistemic affinity between the surveillance of research and the research itself, between the methods of controlling and the conduct to be controlled (cf. Rose and Miller 2008). Through such internal audit, science also succeeds in keeping positive law at bay.

According to recent fieldwork by Lewis and Atkinson, laboratories have already become sites where ‘technologies of surveillance, measurement and monitoring’ (Lewis and Atkinson 2011: 382) make visible the methods (as opposed to the object) of scientific work. Could we imagine operational means – and the ideological and legal set of mind – that would allow us to prevent fraud in research from happening altogether? In the name of security, integrity and so forth, regulation envisaged by ‘technologies of integrity verification’ could eventually get beyond the paperwork, audits, and checks on the reported material of the scientists, and immerse itself into the interstices of the scientific work itself. For instance, technologies could regulate science *ex ante* (Kerr 2013) by preventing human interventions, in certain components of the research. Or alternatively, as is already happening in the field of bioinformatics (for purposes of protection of intellectual property), there could be engines that trace everything the scientist does in the lab, instead of relying on the human scientist to record *post-facto* what she does aside from her work, on a log sheet.[[8]](#footnote-8) This form of built-in regulation could even look like what Larry Lessig (1999) refers to as regulation through ‘code,’ meaning that the scientists might eventually not know that their lab work is being regulated in such ways. What would this all mean for regulation? But more intriguingly, what would this mean for science and for scientists as critical human agents (cf. Rouvroy 2013)? What would science look like, stripped of the *possibilities* of human misconduct and fraud?

**Conclusion**

As we saw above, deliberations amongst the citizens of ‘the republic of science’ (Jasanoff 1997: 93) as well as old legal techniques of collective decision-making, take on surprising, rejuvenated forms in COPE in London, circa 2010–2012. Taken together, the profile of the seventeenth-century

Royal Society’s and the COPE Forum’s respective members, rules of procedure, sponsors/benefactors/funders, physical setting and geographical location (London), elicit a unique, almost inescapable parallel between the two organisations.

In this context it seemed crucial to highlight the presence of good old legal tools and legal knowledge, found rejuvenated, in innovative forms, in the area of research governance and management of scientific misconduct. ‘Witnessing’ and deliberation do seem to survive and to show their sustained, renewed relevance. Legal knowledge and modes of enquiry can be found in innovative sites (Jacob 2011), for example in the COPE Forum, and hence are still very much pertinent to the governance of research conduct. This means that in spite of high-tech strategies deployed to govern scientific integrity, scientists and misconduct hunters perhaps do not quite live yet in the ‘post-regulatory world’ (Black 2001: 103).

The nature of COPE’s work for research integrity is itself changing, in that it increasingly works with technology, and is supplemented by a panoply of high-tech integrity verification tools. These tools in turn resonate with older technological responses to deviance in medicine. Many dimensions of the multivalent work of misconduct hunting have yet to be unpacked. I have only highlighted some aspects here, namely, the sharing of practices between the regulator and its regulatees, the kinship between ‘scientific’ and ‘legal’ means of regulation, and the commonalities between ‘traditional’ versus ‘novel’ engagement with deviance.

The discourses of ‘research integrity’ keep gaining momentum, and getting more and more professionalised. Studying research integrity will thus require the foregrounding of knowledge itself, without letting the mapping of that knowledge’s provenance (‘is this legal? or scientific? or is this ‘just’ social?’) pre-establish our grid of analysis (cf. Latour 1993), and also without letting single perspectives, and perspectivism (Lavi 2011: 815), condense our understanding of the multiple incarnations research integrity takes.

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2. ‘Good research practice’ norms aim to practically implement values pertaining to research integrity, such as honesty, transparency and fairness in research. ‘Good research practice’ refers to norms regarding, for instance, acknowledgement of authorship, data storage and data sharing, disclosure of conflicts of interests, or the use of images in scientific publications, to name a few (Jacob 2013). [↑](#footnote-ref-2)
3. These norms, proposed by sociologist Robert Merton (1979), were: communalism, universalism, disinterestedness, originality and scepticism, and the counter norms: solitariness, particularism, interestedness, and dogmatism. [↑](#footnote-ref-3)
4. Report of the UK Research Integrity Futures Working Group, chaired by Janet Finch, September 2010; Liberating the NHS: Report on the Arm’s Length Bodies Review, July 2010. The AMS Report, A New Pathway for the Regulation and Governance of Health Research, January 2011. [↑](#footnote-ref-4)
5. Committee on Publication Ethics Flowcharts: http://publicationethics.org/resources/flowcharts [↑](#footnote-ref-5)
6. See Committee on Publication Ethics <http://publicationethics.org/about/history> (accessed 10 January 2013). [↑](#footnote-ref-6)
7. For instance, the General Medical Council’s codes of practice and guidance evoke the importance of maintaining a good standard of practice and are in order to justify the trust placed upon registered doctors. [↑](#footnote-ref-7)
8. I thank Hyo Yoon Kang for drawing my attention to this fact. [↑](#footnote-ref-8)