The Rightful Place of Science:

Science on the Verge

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THE RIGHTFUL PLACE OF SCIENCE:
Science on the Verge

The Rightful Place of Science series explores the complex interactions among science, technology, politics, and the human condition.

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WHO WILL SOLVE THE CRISIS IN SCIENCE?

Andrea Saltelli, Jerome Ravetz and Silvio Funtowicz

Endangered science

The integrity of science has been the subject of increasing concern, among scientists and the media, over the past decade. Although science is still among the most trusted of public institutions, the crisis of quality within science is now threatening to erode that trust.

Attempting to explain "Why most published research findings are false", Ioannidis (2005) expresses "increasing concern that in modern research, false findings may be the majority or even the vast majority of published research claims". The same author has created a Meta-Research Innovation Centre (METRICS) at Stanford to combat "bad science" (*Economist*, 2014). In a later paper Ioannidis (2014) estimated that as much as 85% of research funding is wasted as a result of shoddy science—a serious claim for an enterprise that absorbs sizeable portions of public expenditure. According to *The Lancet* (2015)—which in 2014 ran a series on "Research: increasing value, reducing waste"—an estimated $200 billion was wasted on science in the U.S. in 2010 alone.

"Unreliability in scientific literature" and "systematic bias in research" have been denounced by Boyd (2013); in the field of organic chemistry Sanderson notes that "Laboratory experiments cannot be trusted without verification" (2013); Begley (2013: 433-434) decries "Suspected work […in] the majority of preclinical cancer papers in top tier journals". In an earlier paper Begley and Ellis (2012) note that a team of researchers working to reproduce 'landmark studies' in haematology and oncology were able to reproduce only 11% of the original findings.

Natural and medical sciences are not the only domains concerned. Nobel laureate Daniel Kahneman was among the first to sound the alarm in the field of behavioural sciences, warning that he saw "a train wreck looming" (quoted in Yong, 2012). Another Nobel laureate, Joseph Stiglitz, described the mathematical work associated with the financial instruments at the origin of the present recession as having been corrupted by "perverse incentives and flawed models—accelerated by a race to the bottom" (2010: 92).

Fraud and misconduct may be only part of the greater problem of integrity in science, but they are the most visible signs of a crisis. The retraction by the journal *Science* of findings according to which political activists could convince conservative voters to change their minds on same-sex marriage in brief face-to-face conversations prompted the *New York Times* (2015) to headline with "Scientists who cheat", and *Nature* (2015), with "Misplaced faith". Citing the finding that nine in ten British people would trust scientists to follow the rules (according to an Ipsos MORI poll), the *Nature* editorial asked pointedly, "How many scientists would say the same?"

Various initiatives have been launched to record misconduct and track retractions (for example, "Retraction Watch"[1]), while problems of reproducibility have been addressed by, for example, the "Reproducibility Initiative"[2] (*Nature Biotechnology*, 2012). However, the process of checking which results are reproducible and which are not is arduous, as reluctance to share data hampers the reproducibility effort (Van Noorden, 2015).

Four World Conferences on Research Integrity have been held between 2007 and 2015 (see *The Lancet*, 2015, for a discussion); the issues are debated in think tanks (Horton, 2015); and as recently as October 2013 the weekly news magazine *The Economist* dedicated its cover page to the subject, with an editorial exploring "How science goes wrong":

> *Science still commands enormous—if sometimes bemused—respect. But its privileged status is founded on the capacity to be right most of the time and to correct its mistakes when it gets things wrong. […] The false trails laid down by shoddy research are an unforgivable barrier to understanding. (*Economist*, 2013: 11)*

The crisis in reproducibility must imply a dysfunction in science's own quality control mechanisms. In 2015 the publisher Springer and the Université Joseph Fourier released 'SciDetect', software designed to detect fake scientific papers (Springer, 2015): "The open source software [SciDetect] discovers text that has been generated with the SCIgen computer program and other fake-paper generators like Mathgen and Physgen". In China, "A *Science* investigation has uncovered a smorgasbord of questionable practices including paying for author's slots on papers written by other scientists and buying papers from online brokers" (Hvistendahl, 2013).

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[1] See http://retractionwatch.wordpress.com

[2] See http://www.reproducibilityinitiative.org

A superficial reading of the evidence could yield the impression that the main cause of the reproducibility crisis is the deficient statistical competence of scientists themselves. This is the line taken by *The Economist* (2013) in the issue quoted above and which is mostly based on a reading of Ioannidis (2005). According to this reading, the theory and practice of P-values, false positives and false negatives should be taught more rigorously to practicing scientists. However, for some commentators, "P-values are just the tip of the iceberg" (Leek and Peng, 2015), and more fundamental problems pervade the whole data-based evidential chain. In addition to statistical skills, issues surrounding incentives are also prominent in the literature quoted above (Ioannidis, 2005; Stiglitz, 2010).

While at the beginning of the integrity movement (marked by the first World Conference in 2007) the main concerns were misconduct and fraud, the proliferating number of individual cases was soon recognized to be symptomatic of a more general malaise. The focus then shifted to "strengthening research integrity and responsible conduct of research worldwide" (*Lancet*, 2015).

Solutions to the crisis from within the scientific community

In this section we look at 'remedies from within'—that is, recipes that have been put forward from the affected community itself (*e.g.* scientific editors, scientists and research institutions) to tackle the crisis. There has been no lack of effort to find remedies for the perceived problems in science. Worthy and ingenious suggestions for repair and reform of the quality assurance system are regularly produced and widely discussed. A notable example is the San Francisco Declaration on Research Assessment, a manifesto drafted by a group of editors and publishers of scholarly journals who convened during the Annual Meeting of the American Society for Cell Biology (ASCB) in San Francisco in December 2012[3]. As of June 2015 the declaration has been signed by 12,377 individuals and 572 organizations. For *The Lancet* (2015) the most relevant recommendation in the Declaration is, *verbatim*, "Do not use journal-based metrics, such as Journal Impact Factor, as a surrogate measure of the quality of individual research articles to assess an individual scientist's contributions, or in hiring, promotion, or funding decisions." The link between the use of better and more diverse (including more qualitative) metrics to appraise researchers' work on one hand, and transparency in the rules for grant allocation, hiring and promotion on the other, is central to the recommendations contained in the Declaration. The recommendations are arranged into five groups: general, for institutions, for publishers, for organizations that supply metrics and for researchers. Several recommendations are common to two or more groups.

While some of the recommendations in the San Francisco Declaration would be straightforward to implement (for example, enlarging the list of the metrics commonly used to appraise journals, to include '5-year impact factor, EigenFactor […], SCImago […], h-index, editorial and publication times'), others would be more difficult and potentially contentious, as they concern the measurement of the impact of research on practice and policy:

> *For the purposes of research assessment, consider the value and impact of all research outputs (including datasets and software) in addition to research publications, and consider a broad range of impact measures including qualitative in-*

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[3] See http://am.ascb.org/dora/

dicators of research impact, such as influence on policy and practice.

Examples of recommendations for publishers are:

Whether a journal is open-access or subscription-based, remove all re-use limitations on reference lists in research articles and make them available under the Creative Commons Public Domain Dedication. […] Remove or reduce the constraints on the number of references in research articles, and, where appropriate, mandate the citation of primary literature in favor of reviews in order to give credit to the group(s) who first reported a finding.

As mentioned, there are parallels between the recommendations addressed to the various groups, so those specific to researchers include suggestions on how to behave when involved in committees making decisions about funding, hiring, tenure or promotion. Also recommended are: the citing of primary literature; the use of a broad range of criteria to evaluate the research of peers; and the questioning of research assessment practices that rely inappropriately on 'Journal Impact Factors'.

A checklist of good practice from the perspective of a researcher is offered by Ioannidis (2014) in his paper, "How to Make More Published Research True". His recommendations start with scientific practices:

To make more published research true, practices [should] include the adoption of large-scale collaborative research; replication culture; registration; sharing; reproducibility practices; better statistical methods; standardization of definitions and analyses; more appropriate (usually more stringent) statistical thresholds; and improvement in study design standards, peer review, reporting and dissemination of research, and training of the scientific workforce.

Another of his suggestions is to apply a scientific method (specifically counterfactual verification) to what works in science itself:

> *Selection of interventions to improve research practices requires rigorous examination and experimental testing whenever feasible.*

Two further recommendations from Ioannidis target the system of incentives:

> *Optimal interventions need to understand and harness the motives of various stakeholders who operate in scientific research […].*
>
> *Modifications need to be made in the reward system for science, affecting the exchange rates for currencies (e.g., publications and grants) and purchased academic goods (e.g., promotion and other academic or administrative power) and introducing currencies that are better aligned with translatable and reproducible research.*

Most of the recommendations listed above by way of example are unproblematic: who would argue against more transparency, less reliance on metrics that can be manipulated, or a less stringent licensing system to protect authorship? It also seems logical that the system of 'currencies' regulating research careers should be in alignment with the type of science one wishes to foster. At the same time, we suggest that a tendency to focus on the issues surrounding incentives as if these were core of the problem may lead us to overlook deeper and more fundamental factors; if the prevalence of low-quality research were in fact a manifestation of a state of corruption in science, the cure would not be only a matter of improved arrangements for collaboration, inspection or regulation (Ravetz, 1971: 407).

This use of the word 'corruption' above does *not* imply that most, or even many, scientists consciously purvey false or faulty results for some undeserved reward. Rather, it refers to a situation in which, because of changing social arrangements and ethical frameworks, and the consequent discrepancy between the image and the reality of scientific life, it becomes increasingly difficult for scientists to do the good work to which they would normally aspire. In every socially organized activity there are pressures on individuals to pursue short-term personal gains (or to seek protection) at the expense of higher goals. The quality assurance system in science has the function of protecting its practitioners from those corrupting pressures, by implementing testing routines which are supported by incentives and sanctions, backed up by informal peer pressure and validated by the leadership of exemplary individuals (Ravetz, 1971: 22-23). This is clearly a complex social system whose effective functioning cannot be guaranteed by administrative means alone. An apparent confirmation of the systematic character of the corruption problem has been provided by the leaked dossier on fraud in British science reported by the *Times Higher Education* (Matthews, 2015).

The fact that it appears to be extremely difficult to find effective solutions (Horton, 2015) suggests that the problem of quality in science may have its roots in a "structural contradiction" in the very system of production of scientific knowledge (Ravetz, 2011).

Rethinking the problem

In this section we suggest that the crisis in science has not yet been accurately described or diagnosed and that real insight into the situation will require a deeper analysis of its causes.

The root of the crisis could well lie in the very success of science. In his 1963 work *Little Science, Big Science*, de Solla Price anticipated that science would reach saturation (and in the worst case senility) as a result of its own rapid exponential growth, in terms of numbers of researchers, publications, volume of outlays, *etc*. (1963: 1-32). De Solla Price was a perceptive witness of the transformation of science. Though the distinction between little and big science was inspired by an earlier work of Weinberg (1961), De Solla Price had a clear understanding of how the social organization of science and quality control systems would have to change under the new conditions of post-war industrialized science.

De Solla Price is considered today the father of scientometrics, although his forewarnings of the impossibility of the endless growth of science and the implicit dangers in the change in the status of researchers received relatively little attention at the time. Without a doubt, a major cause of the present difficulties is the sheer scale of big science. As personal contact among researchers in the same field has become impossible, scientific communities are less cohesive than before. Big science also engenders the need for 'objective' mass metrics of quality, which are inevitably imperfect and often perverse and corruptible. These effects are compounded by new economic and commercial pressures, in a social and cultural context in which the idealism that motivated 'little science' is no longer compelling.

Disruptive qualitative changes in the conduct of research have been identified by historian and philosopher of economic thought Philip Mirowski as the effects of the 'commoditization' of science. Mirowski offers his detailed aetiology of the predicaments of science in his 2011 book *Science-Mart: Privatizing American Science*. He makes reference in the title to the aggressive giant supermarket chain Walmart, which epitomizes a culture and ideology that, in Mirowski's diagnosis, represent a large part of the problem. According to Mirowski, since the 1980s neoliberal ideologies have succeeded in establishing the principle that the market is the best answer to the question of how best to allocate resources—including in the scientific domain. State funding of research has accordingly decreased and major corporations have closed down their in-house laboratories and begun to commission research, first to universities and then to contract research organizations (CROs), which operate under significant budget and time pressures.

Mirowski's argument is that the quality of science suffers under conditions of commoditization and that this is now undermining the capacity of science to produce innovation. The effects are most obvious in the intrusion of property rights into the materials and outputs of research, to the extent that scientists are often mired in application processes to obtain the requisite permissions, while administrators are preoccupied with processing such applications. In the terms of Mirowski's analysis, the perverse or distorted system of incentives described above is a collateral effect of the prevailing neoliberal ideology. Such a regime is favourable to the 'entrepreneurial scientist', whose career is defined by successful grants enabled by adequate projects, rather than the other way around as in the days of 'little science' (Ravetz, 1971: 46). The use of the term 'currency' by Ioannidis, cited above, suggests that the commoditization of science is so thoroughgoing that even suggested correctives are expressed in the discourse of a neoliberal paradigm. As Mirowski notes, even the quantitative indicators of quality are controlled by the private sector, meaning that we cannot really know how well, or how ill, science is doing. This problem is reaffirmed in a recent discussion in *Nature* (Wilsdon, 2015).

It is evident that the system of incentives applying to medical researchers in a CRO will differ from the system governing researchers working in a national laboratory. Nevertheless, it could be argued that neoliberalism has simply accelerated a process which was already underway and which had long been foreseen by scholars of science and technology. Indeed, Mirowski's historical account could be interpreted as the institution of American science struggling to maintain its high rate of growth, first by cashing in on the prestige it earned from World War II, and then by finding industrial sponsorship. But as De Solla Price had anticipated, the limits of federal largesse were eventually hit, at some moment in the 1990s. Since then the constriction in funding has intensified the crisis in the conditions of work for researchers, and (more worryingly) has contributed to an ageing of the population of working scientists. As Colin Macilwain says (2015: 137), "it is increasingly older people, who know how to work the system, who get funding: people under 40 are finding it harder and harder to get their foot on the ladder". A life in science has evolved from being a vocation, to being a career, and finally, in the 21st century, to being an insecure job.

In his book *Scientific Knowledge and its Social Problems* (1971), Ravetz foresaw that serious trouble for science's quality assurance mechanisms and for morale in the scientific community would follow from the debased ethos of industrialized science:

> *[…] with the industrialization of science, certain changes have occurred which weaken the operation of the traditional mechanism of quality control and direction at the highest level. […]The problem of quality control in science is thus at the centre of the social problems of the industrialized science of the present period. If it fails to resolve this problem […] then the immediate consequences for morale and recruitment will be serious; and those for the survival of science itself, grave. (1971: 22)*
>
> *Two separate factors are necessary for the achievement of worthwhile scientific results: a community of scholars with a shared knowledge of the standards of quality appropriate*

> *for their work and a shared commitment to enforce those standards by the informal sanctions the community possesses; and individuals whose personal integrity sets standards at least as high as those required by their community. If either of these conditions is lacking – if there is a field which is either too disorganized or too demoralized to enforce the appropriate standards, or a group of scientists nominally within the field who are content to publish substandard work in substandard journals – then bad work will be produced. This is but one of the ways in which 'morale' is an important component of scientific activity; and any view of science which fails to recognize the special conditions necessary for the maintenance of morale in science is bound to make disastrous blunders in the planning of science.* (1971: 22-23)

The same section of the book discusses the technocratic view of science to argue that the assimilation of the production of scientific results to the production of material goods can be dangerous, and indeed destructive of science itself. One of the main thrusts of this early work is in fact the illustration of the paradox that the successful production of objective scientific knowledge depends critically on the individual moral commitment of scientists themselves. The question, "*Quis custodiet ipsos custodes?*" (Who guards the guardians?), haunts the quality assurance system in science, the vulnerability of which is exacerbated by its reliance on iterative, informal and ultimately judgemental procedures.

It should be stressed that the "shared commitment" quoted in Ravetz (above) is a far cry from the "better incentives" advocated by would-be reformers within the scientific establishment (*e.g.* in the San Francisco Declaration). While material rewards certainly have their place in the maintenance of morale in a professional or fiduciary activity, it would require social engineering of a high order to make them the sole wellspring of ethical commitment.

At the heart of Ravetz's reflections is the recognition of the need for good morale in science:

> *The need for good morale is never mentioned in general discussions of science directed to a lay audience; and this is evidence that hitherto its presence could be taken for granted. For doing good scientific work is strenuous and demanding, and the quality of the work done in any field of science is dependent, to a great extent, on the integrity and commitment of the community of scientists involved. (1971: 58)*

Ravetz goes on to outline the hypothetical situation that the peer review mechanism is intended to avert:

> *If there were not a test of each paper before its acceptance by a journal, then every intending user would be forced to examine it at length before investing any of his resources in work which relied on it. Under such circumstances, the co-operative work of science as we know it could not take place. (1971: 176)*

In effect, this highly undesirable situation has to some extent materialized: some chemists have felt themselves obliged to replicate organic syntheses since they cannot trust published results (Sanderson, 2013).

Can science's predicaments be resolved, and how? Ravetz's understanding of the matter is as follows:

> *No formal system of imposed penalties and rewards will guarantee the maintenance of quality, for the tasks of scientific inquiry are generally too subtle to be so crudely assessed; nor will the advantages to an individual of a good reputation of his group be sufficient to induce a self-interested individual to make sacrifices to maintain it. Only the identification with his colleagues, and the pride in his work, both requiring good morale, will ensure good work. (1971: 407)*

> *The conditions of industrialized science present [leading scientists] with problems and temptations for which their inherited 'scientific ethic' is totally inadequate. (1971: 408)*

Our conclusion thus far is that the problems in science will not be fixed by better training in statistics, better alignment of incentives with objectives, better regulation of copyright or the elimination of impact factors, although these and other measures discussed in Section 2 all have their uses. We are not dealing with isolated crises in reproducibility, peer review mechanisms or hiring practices; rather, we are facing what we could call a generalized crisis in the epistemic governance of science.

A rather severe judgement—taking the epistemic governance crisis to an extreme conclusion—is offered by Millgram (2015: 21-53). To Millgram, the success of the Enlightenment is the root cause of its ultimate failure—that is, the failure of man's free will and independent judgement to triumph over irrational principles of authority, religion and superstition. What the Enlightenment has in fact generated is a society of "serial hyperspecializers" (26), a world in which all knowledge and products are the result of some form of extremely specialized expertise, and in which expertise is itself highly circumscribed, since experts depend in turn on other experts whose knowledge claims and styles of argumentation cannot be exported from one discipline to the next. Experts thus become "logical aliens" (32) to one another and humans become incapable of forming judgements. The author describes this as "the great endarkenment", characterized by "commitments (both decisions and views as to how the facts stand) whose merits no one is in a position to assess—where […] each decision, […] is made on the basis of further decisions, whose merits no one can assess either. Trusting in the outputs of this process is on a par with settling what you are going to do by reading entrails or casting hexagrams" (36-37). To make decisions in this environment, one needs constantly to wrestle quality "from the jaws of entropy". Millgram contends that we simply have no idea of how bad the situation is, likening it to the era of premodern medicine, under which patients endured pain and suffering because "they weren't equipped to assess the theories, inferential practice, and effectiveness of the procedures performed by members of a specialized professional guild" (37). The world he depicts could well be a portrayal of De Solla Price's vision of the senility of science. Ravetz (2011) provides an analogous historical perspective, in terms of the maturing of the structural contradictions of modern science.

What can be done

The main impediment to a possible cure of the present disease is the belief that the system will straighten itself out: that the scientific community can use its own craft to mend itself. This is implausible because the assumptions, structures and practices out of which the crisis arose are not likely to produce its solution unaided. Some help from without is in order.

For *The Lancet* (2015), "The coming together of the three themes—research integrity; research reward systems; and increasing value and reducing waste in research—is helpful and has greater potential in effecting change than each on its own."

There is no denying the importance of those themes, but some crucial issues remain unaddressed: primarily that of 'who' will launch and pursue this process and, even more importantly, 'how'. The institutional response has not yet been adequate. As Colin Macilwain notes in *Nature* (2015), the peer-reviewed paper, "the main yardstick for success or failure in almost all academic research careers", and the peer-reviewed, singleinvestigator grant, another pillar in the mechanics of the scientific system, have been left untouched (*pace* the San Francisco declaration). In Macilwain's view, both the European Commission and the American National Institutes of Health (NIH) prefer to muddle through with obsolete structures and mechanisms. When top-level medical scientists gather to ponder these problems and their possible systemic solutions, it seems that they are content to be advised by a physicist who recommends standards of statistical significance (1 in 3.5 million) that may be appropriate for high-energy experiments but are hardly applicable elsewhere (Horton, 2015).

If purely technical or instrumental solutions are unlikely to be adequate to solve the crisis, then the intuitions and endeavours of concerned scientists are likely to benefit from the contributions of other voices, including reflective scholars, journalists and members of civil society.

Scientists will certainly play a crucial role in the construction of the future of science. Timothy Gowers's campaign against Elsevier with the slogan "Academic Spring" shows how effective a scientist can be when a new consciousness is achieved (Whitfield, 2012). The "Science in Transition" discussion in the Netherlands[4] is an example of committed scientists taking the initiative. Courageous librarians such as Jeffrey Beall at the University of Colorado, Denver, help in the fight against "predatory publishers" who charge authors for publishing but do not provide any control or peer review. Beall also keeps an eye on other degenerations[5]:

> *The Misleading metrics list includes companies that "calculate" and publish counterfeit impact factors […] The Hi-*

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[4] See http://www.scienceintransition.nl/english
[5] See http://scholarlyoa.com/2015/01/02/bealls-list-of-predatory-publishers-2015/

jacked journals list includes journals for which someone has created a counterfeit website, stealing the journal's identity and soliciting articles submissions using the author-pays model (gold open-access).

Beyond these initiatives, we urgently need a philosophy of science in which science's imperfections and vulnerabilities are acknowledged and explored. An early attempt at this was made by Thomas Kuhn (1962), who broke with a venerable philosophical tradition which held that science produced truth or its best approximation. His insights into the contingency of scientific truths were built on by Ravetz (1971), with a focus on quality and on the personal, moral element in the achievement of objective scientific knowledge. A further philosophical development, taking into account complexity, ignorance, abuse of mathematics and corruption in its broad (not personal) sense, is now overdue.

One necessary step on this path will be to review the traditional assumption of the separation between science and society. This has been described elsewhere as the "demarcation model" (Funtowicz, 2006) and as the "Cartesian dream" of infinite perfectibility driven by autonomous science (Guimarães Pereira and Funtowicz, 2015):

For several centuries, the understanding of science has been conditioned by a belief in the separateness of knowledge and society. […] That simple faith is no longer adequate for its function of maintaining the integrity and vitality of science. (Ravetz, 1971: 405)

There is a rich body of scholarship warning against the delusion of such a separation, from Toulmin's *Return to Reason* (2001) and *Cosmopolis* (1990) to Feyerabend's *Against Method* (1975), Lyotard's *The Postmodern Condition* (1979) and Latour's *We Have Never Been Modern* (1993). Although 'science wars' were fought between the natural and human sciences in the 1980s and 1990s, belief in the integrity of science *vis-à-vis* social and political influence still prevails, not least in the institutions and apparatuses in charge of governing and funding science.

The ideal of a disinterested scientific practice which manages to isolate itself from the messiness of everyday life and politics is, of course, an abstraction. We deal not in pure facts of nature, but in 'hybrid', socially constructed objects (Latour, 1993), and one of the features of the present epistemic governance crisis is that "the more knowledge is produced in hybrid arrangements, the more the protagonists will insist on the integrity, even veracity of their findings" (Grundmann, 2009). Nowhere is such a crisis more evident than in the inappropriate use of mathematical modelling and quantification of the world (Saltelli and Funtowicz, 2014), which undermines the use of such evidence for policy (see Chapter 2, this volume)[6]. While it is clear that the demarcation model aims to protect science from political interference by preventing the potential abuse of science and scientific information for political agendas, the model relies on the ideal of separation between the institutions and individuals that 'do' science and those that 'use' it. Whether or not this ideal is even desirable in abstract terms, it is a chimera. The integrity of science will be better protected by heightened awareness of its vulnerabilities, not by fantasies of isolation.

One of the most hopeful signs in the present crisis is the tacit abandonment of the traditional image of science as a truth-producing machine. For centuries, philosophers and historians preached the inexorable progress of Truth. Students never saw incorrect statements in science other than those resulting from their own stupidity. Instances in which great scientists had been partly or

[6] See also https://ec.europa.eu/jrc/en/event/conference/use-quantitative-information

wholly wrong were glossed over. It became nearly inconceivable that research based on numerical data and mathematical methods could be wrong or futile. That has all changed quite recently, as we saw in Section 1 above. There is now a burgeoning literature that shows by example that science is fallible. This is easily demonstrated in the case of statistics, in which the practitioner may legitimately obtain any desired answer by choosing the parameters judiciously (Aschwanden, 2015).

We can therefore already point to some of what needs to be 'unlearned' in the prevailing model of science. Kuhn observed that education in science is "narrow and rigid" and comparable to orthodox theology (1962: 165). There is an 'implicit scientific catechism' that students learn by example but that working scientists must leave behind: chiefly, that every scientific problem has one and only one correct solution, precise to several significant digits; that quantitative data and mathematical techniques produce certainty; and that error in science is the result of stupidity or malevolence. Small wonder that when this Cartesian philosophy, with its reliance on the illusory precision of models and indicators, guides any attempt to understand and manipulate complex systems, it can go so spectacularly wrong.

Education will therefore clearly be an important facet of the necessary reform of science. There are signs that science education may already be changing, under the influence of the new social media. The growth of 'do-it-yourself (DIY) science', showing only a minimum of deference to established science, will eventually influence science education to good effect. When students conceive of a scientific exercise as a 'hack' rather than a 'proof', a new consciousness is being created. Kuhn's gloomy picture of science education may at last be on the way out.

In his quest for social solutions to the anticipated predicaments of science, Ravetz (1971) envisaged a new "critical science" which had considerations of participation and respect for the environment at its heart. These ideals were further developed in subsequent works (Funtowicz and Ravetz, 1990, 1991, 1992, 1993) and led to the concept of "post-normal science" (PNS), which is today relatively well known as an approach to deal with problems at the interface between science and policy. While PNS was designed to apply where facts are uncertain, values in dispute, stakes high and decisions urgent (Funtowicz and Ravetz, 1991, 1992, 1993), with the ecological movement as one of its driving forces, PNS is understood today as a system of epistemic governance of practical applicability to all domains in which science and society interact, *i.e.* by definition, to all settings where science operates, including reflexively to the operation of science itself. In PNS the focus is on participation, legitimacy, transparency and accountability. In the "extended participation model" (Funtowicz, 2006), deliberation (on what to do) is extended across disciplines—in the acknowledgment that each discipline has its own lens—and across communities of experts and stakeholders. In adopting this model, one moves from 'speaking truth to power' towards "working deliberatively within imperfections" (Funtowicz, 2006f).

McQuillan (2014) has recently remarked that the movement known as 'Citizen Science' could seize the opportunity created by the crisis in science. PNS is singled out by McQuillan as a promising framework for the work to be done. The remainder of this section will focus on the possibility that the principles of PNS, foremost that of the participation of extended peer communities, may furnish some elements of a solution to the crisis.

New forms of activity in science are appearing rapidly, due to the interaction of new information technologies and new political currents (Ravetz and Funtowicz, 2015). The introduction of the photocopying machine and then of the internet has dramatically changed the conditions for quality control in science. The gateways and the strictures of the printed page have been eliminated. Anyone can distribute information at an unprecedented speed. The present times could be seen as analogous to the period that followed the invention of the printing press and the publication of Gutenberg's Bible around 1450. Now, as then, the monopoly over the channels of knowledge dissemination is collapsing and new, expanded audiences are being created. Higher levels of literacy, increased use of information and communication technologies and increased awareness of complexity have been identified by the authors of the Stiglitz Commission for the Measurement of Progress (CMEPSP, 2009: 7) as a cause of increased use and production of statistical indicators by an ever wider public. Greater media interest is being given to all forms of scientific consumption, as discussed at the beginning of the chapter in relation to science-centred controversies. We would highlight one other powerful driver of change: increased levels of advocacy, by which we mean the critical activity whereby citizens of varying degrees of scientific literacy take it upon themselves to examine and pronounce on the 'goods' and the 'bads' of science and technology. "Is the internet to science what the Gutenberg press was to the church?" ask Funtowicz and Ravetz (2015).

Daniel Sarewitz (2015) also considers the participation of citizens essential if the application of science to policy is to work. In his paper "Science can't solve it", he argues that questions over crucial issues such as genetically modified (GM) organisms, nuclear power and the efficacy of cancer screening cannot and should not be decided by experts alone, lest the legitimacy of science's role in society be eroded.

Leadership that inspires by precept and example will also be needed. A parallel may be drawn with the introduction of principles of 'total quality assurance' into industrial quality control by W. Edwards Deming in the late 1940s. His concepts were successfully taken up by Japanese car manufacturers before being imported back to the USA several decades later. At the core of Deming's work was the establishment of quality circles, in which the assembly line was transformed into a participatory process. In this way the community of expert quality controllers and evaluators was extended to the entire work force, allowing the latter to rely on and convey its practical and possibly tacit knowledge, experience and commitment. The quality circles also encouraged the practice of 'whistle-blowing', whereby any member of the community, including the assembly line workers, could stop the process if they believed that quality had been compromised (Deming, 1986).

An additional justification for the cultivation of extended peer communities in the quality assurance of science is the importance of trust in the system. Science is at present deeply involved in technology and related policy problems that affect public health and welfare, to the extent that the traditional relations of trust can no longer be taken for granted. If there should be another scandal in high-stakes policy-related science—for example, along the lines of the unjustified assurances of the 'safety of British beef' given by official experts during the BSE ('mad-cow') epidemic—then public trust in scientific probity and science-based advice could be seriously affected. Maintaining the justified trust of the public in science is critically important; however, to do that, it will first be necessary to restore the trust of scientists themselves in their own community and practice.

Attempts to circumvent the need for trust by increasing the level of bureaucratic surveillance or quantified quality control criteria are likely to be counterproductive and ineffective. In effect, extended peer communities are already being created, in increasing numbers. They are called citizen juries, focus groups, consensus conferences, or a variety of other names. They come to life either when authorities cannot see a way forward, or when they know that without a broad base of consensus, no policy can succeed. In their early phases these have been largely top-down initiatives, subject to various pressures; but that scene is also changing.

These communities assess the quality of policy proposals, including their scientific justification, on the basis of the science that they master, combined with their commitment and their knowledge—often local and direct—of the subject matter. They ask the sorts of questions that do not occur to the experts, who necessarily conceive of problems and solutions from within their professional paradigms. The form of the questions can be “what if?”, “what about?”, and “why this and not that?” The moral force of these extended peer communities, which have the ability to create their own ‘extended facts’ by questioning the framings of the issue proposed by the incumbent powers, can translate into political influence. The extended facts may include craft wisdom and community knowledge of places and their histories, including their history of interaction with the authorities (Lane *et al.*, 2011), as well as anecdotal evidence, neighbourhood surveys, investigative journalism, and leaked documents (Funtowicz and Ravetz, 2015). Local people can imagine solutions and reformulate problems in ways that the accredited experts do not find ‘normal’. Their input is thus not merely that of quality assurance of a scientific evidential base, but of problem-solving in general (Funtowicz and Ravetz, 2015). Sheila Jasanoff (2003, 2007) speaks in this respect of “technologies of humility”.

Similarly, the DIY science movement puts scientific matters literally into the hands of interested citizens—the embodiment of Giovan Battista Vico's philosophical programme, conceived in opposition to René Descartes. For Vico, "*verum et factum convertuntur*"—literally 'the true and the made are convertible', thus 'I know it if I can make it'. These movements seem to be driven from within and to operate without need of incentives. Two wings can be identified within the citizen science movement: 'amateur-citizen' and 'activist-citizen', depending on how they interact with established science. The former assist professional scientists in folding proteins or classifying galaxies on online interfaces, while the latter take matters into their own hands when they feel that the existing institutions have failed them (the classic example being Lois Gibbs in the case of the Love Canal toxic waste dump).

The rapidly developing citizen science movement may also provide the elements of a solution to the crisis in quality within science. In principle, a less well organized (and occasionally anarchic) movement should have even greater problems of quality assurance than traditional science, with its established structures of control. But there is evidence of two redeeming features of citizen science. One is high morale and commitment among the citizen-scientists (Newman, 2015); the other is the establishment of appropriate systems of quality assurance (Citizen Science Association, 2012). It is, of course, far too early to say how effective these will be; but at least there are encouraging signs.

This mainly external movement may also eventually lead to the emergence of a 'scientist-citizen' movement within established science itself. Scientist-citizens could engage both with the internal problems of science, such as trust and quality assurance, and with the external challenges relating to the use of science to solve practical (*i.e.* policy) problems. An eloquent argument for the recognition of such scientist-citizens has been made by Jack Stilgoe (2009). The means for this development are already being put into place, with the rapid development of new social media techniques in the communication system of science. With the breaking of the effective monopoly of peer-reviewed journals, participation, transparency and openness are flourishing within the communities of science. This new social reality of free internal criticism is antithetical to the so-called 'deficit model', which assumes that public acceptance of a perfect official science is obstructed only by lay people's deficient scientific literacy (Wynne, 1993). A scientist-citizen approach would commit scientists *qua* citizens to criticism, reflection and action.

Conclusion

This chapter started with the identification of a crisis in the governance of science. We are not referring only to the frequency of retractions, which could be an artefact of a more rigorous editorial policy and increased scrutiny by media, bloggers and practitioners. We also observe the increased frequency of warnings, many mentioned in this chapter, by scientists who are concerned about a future blighted by predatory publishers, fraudulent peer reviewing, and the manipulated, and thus often ineffective, use of scientific evidence for policy. We have offered elements of a possible solution, among which we have privileged avenues external to science's own institutions.

Could citizen science and scientist-citizens together perform the rescue of quality and trust in science? It is much too early to say, and the evidence proposed here in support of the thesis is largely based on anecdote, metaphor and analogy and on the predictions of a book that is now more than forty years old (Ravetz, 1971; see also Chapter 3, this volume, for a circumspect view of citizen science). It is clear that action is being taken on many fronts, both in established science and in its new forms of practice. The restoration of quality in science, and the preservation of trust, will not be accomplished by 'scientific' means alone. We are therefore facing one of the greatest challenges for science of our times.

References

Ashwanden, C., 2015. "Science isn't broken. It's just a hell of a lot harder than we give it credit for". FiveThirtyEight. http://fivethirtyeight.com/features/science-isnt-broken

Begley, C. G., and Ellis, M. E., 2012. "Drug Development: Raise Standards for Preclinical Cancer Research", *Nature*, 483: 531–533.

Begley, C. G., 2013. "Reproducibility: Six red flags for suspect work", *Nature*, 497: 433–434.

Boyd, I., 2013. "A standard for policy-relevant science. Ian Boyd calls for an auditing process to help policy-makers to navigate research bias", *Nature Comment*, 501: 160, 12 September.

Citizen Science Association, 2015. "Introducing the Data and Metadata Working Group", 12 November. http://citizenscienceassociation.org/2015/11/12/introducing-the-data-and-metadata-working-group/

CMEPSP, 2009 (Commission on the Measurement of Economic Performance and Social Progress), http://www.insee.fr/fr/publications-et-services/dossiers_web/stiglitz/doc-commission/RAPPORT_anglais.pdf

De Solla Price, D. J., 1963. *Little science, big science*. New York: Columbia University Press.

Deming, W. E., 1986. *Out of the Crisis*. Cambridge, MA: MIT Press.

Economist, 2013. "How science goes wrong", 19 October: 11.

Economist, 2014. "Combating bad science Metaphysicians. Sloppy researchers beware. A new institute has you in its sights", 15 March.

Feyerabend, P., 2010 (1975). *Against Method*. London: Verso.

Funtowicz, S., 2006. "What is Knowledge Assessment?", in Guimarães Pereira, Â., Guedes Vaz, S. and Tognetti, S. (eds.) *Interfaces between Science and Society*. Sheffield: Greenleaf Publishers.

Funtowicz, S. and Ravetz, J., 1990. *Uncertainty and Quality in Science for Policy*. Dordrecht: Kluwer Academic Publishers.

Funtowicz, S. O. and Ravetz, J. R., 1991. "A New Scientific Methodology for Global Environmental Issues", in Costanza, R. (ed.), *Ecological Economics: The Science and Management* *of Sustainability*: 137–152. New York: Columbia University Press.

Funtowicz, S. O. and Ravetz, J. R., 1992. "Three types of risk assessment and the emergence of postnormal science", in Krimsky, S. and Golding, D. (eds.), *Social Theories of Risk*: 251–273. Westport, CT: Greenwood.

Funtowicz, S. and Ravetz, J., 1993. "Science for the post-normal age", *Futures*, 31(7): 735-755.

Funtowicz, S. and Ravetz, J. R., 2015. "Peer Review and Quality Control", Wright, J. D., (ed.), *International Encyclopedia of the Social and Behavioral Sciences,* 2nd edition. Oxford: Elsevier.

Grundmann, R., 2009. "The role of expertise in governance processes", *Forest Policy and Economics*, 11: 398-403.

Guimarães Pereira, Â. and Funtowicz, S. (eds.), 2015. *Science, Philosophy and Sustainability: The end of the Cartesian dream*. Routledge series Explorations in Sustainability and Governance. New York: Routledge.

Horton, R., 2015. "Offline: What is medicine's 5 sigma?", *Lancet*, 385: 1380.

Hvistendahl, M., 2013. "China's Publication Bazaar", *Science*, 342: 1035-1039.

Ioannidis, J. P. A., 2005. "Why Most Published Research Findings Are False", *PLoS Medicine*, 2(8): 696-701.

Ioannidis, J. P., 2014. "How to Make More Published Research True", *PLoS medicine*, 11(10), e1001747.

Jasanoff, S., 2003. "Technologies of Humility: Citizen Participation in Governing Science", *Minerva*, 41(3): 223-244.

Jasanoff, S., 2007. "Science & Politics. Technologies of humility", *Nature*, 450: 33.

Kuhn, T. S., 1962, *The Structure of Scientific Revolutions* (1st ed.). University of Chicago Press.

Lancet, 2015. Editorial: "Rewarding true inquiry and diligence in research", 385: 2121.

Lane, S. N., Odoni, N., Landström, C., Whatmore, S. J., Ward, N. and Bradley, S., 2011. "Doing flood risk science differently: an experiment in radical scientific method." *Transactions of the Institute of British Geographers*, 36: 15-36.

Latour, B., 1993. *We Have Never Been Modern*. Cambridge, MA: Harvard University Press. Originally published 1991 as

Nous n'avons jamais été modernes. Paris: Editions La découverte.

Leek, J. T. and Peng, R. D., 2015. "P values are just the tip of the iceberg", *Nature*, 520: 612.

Lyotard, J.-F., 1979. *La Condition postmoderne. Rapport sur le savoir*: Chapter 10. Paris: Minuit.

Macilwain, C., 2015. "The future of science will soon be upon us", *Nature*, 524: 137.

Matthews, D., 2015. "Secret dossier on research fraud suggests government concern over science", *Times Higher Education*, 3 December.

McQuillan, D., 2014. "The Countercultural Potential of Citizen Science", *Media and Communication Journal*, 17(6).

Millgram, E., 2015. *The Great Endarkenment: Philosophy for an Age of Hyperspecialization*. Oxford University Press.

Mirowski, P., 2011. *Science-Mart: Privatizing American Science*. Harvard University Press.

Nature Biotechnology, 2012. "Further Confirmation Needed", Editorial, *Nature Biotechnology*, 30: 806.

Nature, 2015. "Misplaced faith. The public trusts scientists much more than scientists think. But should it?" Editorial, 2 June, *Nature*, 522: 6. http://www.nature.com/news/misplaced-faith-1.17684

Newman, G., 2015. "9 things to be grateful for about Citizen Science", Monthly Letter from Greg Newman (the CSA board chair) to all CSA members, 12 November. http://citizenscienceassociation.org/2015/11/12/9-things-to-be-grateful-for-about-citizen-science/

New York Times, 2015. "Scientists Who Cheat". Editorial, 1 June.

Ravetz, J. R., 1971. *Scientific Knowledge and its Social Problems*. Oxford University Press.

Ravetz, J. R., 2011. "Postnormal Science and the maturing of the structural contradictions of modern European science", *Futures* 43: 142–148.

Ravetz, J. R. and Funtowicz, S. O., 2015. "Science, New Forms of", in Wright, J. D., (ed.), *International Encyclopedia of the Social and Behavioral Sciences,* 2nd edition, Vol. 21: 248–254. Oxford: Elsevier.

Ravetz, R. and Saltelli, A., 2015. "Policy: The future of public trust in science", *Nature*, 524: 161.

Saltelli, A. and Funtowicz, S., 2014. "When all models are wrong: More stringent quality criteria are needed for models used at the science-policy interface", *Issues in Science and Technology*, 30(2): 79-85.

Sanderson, K., 2013. "Bloggers put chemical reactions through the replication mill", *Nature*, 21 January, doi:10.1038/nature.2013.12262.

San Francisco Declaration on Research Assessment (DORA). http://www.ascb.org/dora/.

Sarewitz, D., 2015. "Science can't solve it", *Nature*, 522: 413-414.

Springer, 2015. "Springer and Université Joseph Fourier release SciDetect to discover fake scientific papers". https://www.springer.com/gp/about-springer/media/press-releases/corporate/springer-and-universit%C3%A9-joseph-fourier-release-scidetect-to-discover-fake-scientific-papers--/54166

Stiglitz, J., 2010. *Freefall, Free Markets and the Sinking of the Global Economy*. London: Penguin.

Stilgoe, J., 2009. *Citizen Scientists: reconnecting science with civil society*. London: Demos.

Toulmin, S., 1990. *Cosmopolis. The Hidden Agenda of Modernity*. Chicago: University of Chicago Press.

Toulmin, S., 2003 (2001). *Return to Reason*. Harvard University Press.

Van der Sluijs, J. P., 2012. "Uncertainty and Dissent in Climate Risk Assessment: A Post-Normal Perspective", *Nature and Culture*, 7(2): 174-195.

Van Noorden, R., 2015. "Sluggish data sharing hampers reproducibility effort", *Nature News*, 3 June.

Weinberg, A. M., 1961. "Impact of Large-Scale Science on the United States", *Science*, 134 (3473): 161-164.

Whitfield, J., 2012. "Elsevier boycott gathers pace: Rebel academics ponder how to break free of commercial publishers", *Nature*, doi:10.1038/nature.2012.10010.

Wilsdon, J., 2015. "We need a measured approach to metrics", *Nature*, 523: 129, 9 July.

Wynne, B., 1993. "Public uptake of science: a case for institutional reflexivity", *Public Understanding of Science*, 2: 321-337.

Yong, E., 2012. "Nobel laureate challenges psychologists to clean up their act", *Nature News*, 3 October.