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What is science's crisis really about?

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ABSTRACT

Present day reasoning about difficulties in science reproducibility, science governance, and the use of science for policy could benefit from a philosophical and historical perspective. This would show that the present crisis was anticipated by some scholars of these disciplines, and that diagnoses were offered which are not yet mainstream among crisis-aware disciplines, from statistics to medicine, from bibliometrics to biology. Diagnoses in turn open the path to possible solutions. This discussion is urgent given the impact of the crises on public trust in institutions. We ask whether the present crisis may be seminal in terms of drawing attention to alternative visions for the role of Science in society, and its relevant institutional arrangements. We finish by offering a number of suggestions in this direction.

1. Crises recognized and unrecognized

Many articles have been written on the crisis in science's reproducibility, science's governance, and the use of science for policy. Still a clear identification of the root causes of the present situation seem to have eluded most commentators. A discussion of those causes is urgent given the public impact of the crisis on trust in institutions. We argue that:

– Science's crisis is real, encompassing roles and social functions of science.
– The mainstream interpretation of the root causes of the crisis is insufficient, and needs to be complemented with the insight offered by some scholars who anticipated the current predicament.
– The root causes of the crisis include profound transformations of society and science's role in society. In this situation, scientists cannot hope to resolve the problem alone as they have contributed to create it in the first place, and have high stakes in the preservation of the status quo.
– A resolution to the predicament is not in sight, but there are some remedies that can be implemented to improve the situation, including important changes in the behavior and societal activity of the scientific community.

2. An eye on the past history

The use of scientific evidence for governance has historically been a locus of conflict. The conception of science as a privileged way to reveal objective facts driving social progress and civilization has been the basis of an enduring tradition. It begun in early modern times with Bacon's *New Atlantis*, continuing with Descartes and Condorcet, and more recently exemplified by Vannevar Bush's post-World War II metaphor of science as the "endless frontier & (1945).

This intellectual tradition has been vigorously challenged by similarly tenacious schools of philosophical and political thought.

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Philosophers have variously addressed the role played by science in legitimizing a social order, and the problems arising when science becomes an instrument of profit and growth as opposed to an instrument of human betterment and emancipation.

This critique has questioned, for instance, the dichotomy facts-values, stressing the social construction of facts, and can be traced back to the works of philosophers such as Friedrich Nietzsche and Edmund Husserl, with their rejection of science as a substitute metaphysics. Stephen Toulmin has been quite articulated in his critique of the Cartesian idea of rationality (1990, 2001), while the essence of the scientific method has been the subject of the writings (and disputes) of authors such as Karl Popper, Thomas Kuhn, Paul Feyerabend and Imre Lakatos. The echo of these disputes leads to the present days with the debate in physics on the falsifiability of string theory.

A first important forewarning of a possible crisis was in 1963 work *Little Science, Big Science* by Derek de Solla Price. He hypothesized that science would reach saturation (and in the worst case, senility) as a result of its own exponential growth.

Science as a human and social enterprise is a central subject of the work of Jerome Ravetz and of Bruno Latour (1987). Ravetz's 1971 book entitled 'Scientific knowledge and its social problems', in particular, offers a critique of the myths of objectivity and of the unlimited power of science in relation to science's enrolment in solving practical (*i.e.* 'social') problems. A lifelong concern for Ravetz was that science is foremost a social activity, and that the transformation from little to big or incorporated science would take place with deep changes in the social fabric of the research enterprise. Ravetz argues that the transformation would endanger the informal quality control arrangements of little science, with a consequent degeneration and corruption of the practice. This would happen when the 'Gemeinschaft', the community of scientists whose personal acquaintance kept them committed to a high moral standard is replaced by a 'Gesellschaft', where the worth of each member is evaluated by 'objective' metrics. He argued recently (2016) that "applying a 'scientific' methodology to the tasks of governance of science leads directly to corruption, since any such system can be gamed". Elsewhere Ravetz (2011) defines the issue in terms of the "maturing of the structural contradictions of modern European science".

For Philip Mirowski (2011) science degenerates as a result of its dependence to the logic and the needs of the marketplace; he shows how the adjudication of research to the lowest bidder progressively corrupts the quality of its results. Comparing Ravetz and Mirowski, it is fascinating to see how the insights of the former are confirmed by the detailed account of the latter.

For Collingridge and Reeve (1986), science's mechanism for convergence towards 'consensus' simply fails in the presence of political pressures. A thesis that the authors illustrate with a rich collection of examples, from the effects of lead in gasoline to the toxicity of tobacco. The critique targets two twin myths: of rationality (whereby policy action is predicated on the accumulation of facts and the suppression of uncertainty), and of the power of science (whereby science provides value neutral facts to inform positive policy action).

Finally, the narrative of the possibility of scientific quantification of natural and social processes, via risk or cost–benefit analyses, including of the impact of new technologies, has provoked an early critique from parts of the ecological movement, such as in the famous Small Is Beautiful by Schumacher (1973).

3. Is there a crisis?

In its present configuration, science finds itself under pressure from two major concurrent and intersecting crises (Benessia et al., 2016): one concerns public trust in the evidence produced by science and its institutions; the other, the governance of science and the reproducibility of its results.

Natural sciences offer many examples of imprudent inferences, from forensics to medicine, from chemistry to nutrition. A collection of instances involving the use of mathematical modelling can be found in a book by Orrin H. Pilkey and Linda Pilkey-Jarvis entitled Useless Arithmetic (2007); (see a discussion is Saltelli & Funtowicz 2014).

Nutrition provides a particularly telling example (Leslie, 2016). In the early post-World War II period an eminent nutritionist, Ancel Keys, published the results of a "seven nations" study in which he identified a correlation between a diet high in animal fats and heart disease. By the time the flaws in his study were exposed, its recommendations were already entrenched in the strategies and policies of major charities and the medical profession. About half a century elapsed before the situation was corrected, and the experts began to change their advice. As in almost any epidemiological issue, the case is complex, and there is confusion between 'the lipid hypothesis' of the cause of high-cholesterol, and the explanation of coronary heart disease with a diet high in animal fats. However, there is little doubt that the focus on the role of fat, while ignoring the relevance of sugar, was indeed responsible for serious medical and societal consequences. As noted by David Barash (2015) when discussing a broader range of foodstuff, "medical paradigms found, then lost, then regained, then placed in a kind of scientific limbo occur in the field of nutrition". The issue has now become the subject of media parodies (Hutto, 2016). The publication of a recent article on the journal of the American Medical Association (Kearns, Schmidt, & Glantz, 2016) revealing that "the industry sponsored a research program in the 1960s and 1970s that successfully cast doubt about the hazards of sucrose while promoting fat as the dietary culprit in CHD [Coronary Heart Disease]" brings the entire sad story to its end.

Economics offered a disturbing case of improper science advice. Harvard professors Kenneth Rogoff and Carmen Reinhart calculated a threshold of 90% for the ratio of public debt to gross domestic product, above which countries' economic growth would supposedly be impaired. A subsequent re-analysis by researchers from the University of Massachusetts at Amherst disproved this finding by tracing it to a coding error in the authors' work. However, this revelation did not lead to a reconsideration of the austerity economic policies that had already been implemented on the basis of the original advice (Cassidy, 2013), nor to a loss of prestige or adviser status for the same economists.

The second important dimension of the crisis relates to the internal system of governance and quality control in science. Evidence of this crisis can be found in the existence of a series of world conferences on research integrity,[1] in critical editorials of prestigious journals such as Lancet (2015) or Nature (2015) and in numerous reflective articles (Begley & Ioannidis 2015; Horton, 2015; Ioannidis, 2005, 2014). The crisis leaves no field untouched, from preclinical medical research (Begley & Ellis, 2012; Begley, 2013) to clinical research (Ioannidis, 2016). This may damage trust in medical science (Evans, 2015), also as a result of ever more frequent reversals in medical advice (Sukel, 2016).

This aspect of the present crisis has an economic chapter, with economist Paul Romer coining the term "mathiness & (2015) in a courageous debate against "freshwater economists" (targeting the neoclassical, minimum-government-intervention school of economic thought, and specifically economists such as Robert Lucas at the University of Chicago). For Romer, mathematics tends to be used like Latin, to intimidate adversaries, ward off debate and veil ideological stances. For Erik Reinert (2000) economics oscillates between states of mathematical scholasticism and a more useful 'science of practice'.

In social science, a milestone was the study of Brian Nosek, a psychologist at the University of Virginia, who attempted the replication of a 100 major works. Nosek's team could only replicate 35% of the research analyzed, and the size of the effects was systematically smaller in the replication than in the original (Baker, 2015; OSC, 2015). In turn, Nosek's own finding were questioned (Gilbert, King, Pettigrew, & Wilson, 2016) and the dispute continues as the involved parties battle on.

Manipulation of p-values (or 'p-hacking') is often identified as a problem. (Shanks et al., 2015). In the course of 2016 the American Statistical Association felt the need to intervene with a commentary (Wasserstein & Lazar, 2016) accompanied by as many as 20 'dissenting' opinions by leading statisticians.

For Gigerenzer and Marewski (2015) the issue is with statistics used as recipes, instead of tools needing judgment; an argument already made by philosophers Rudner (1953) and Ravetz (1971). Gigerenzer and Marewski recall Francis Bacon, noting that 'In Bacon's view, it is better to have no beliefs than to embrace falsehoods, because false idols block the way toward enlightenment.' Overall mathematical or statistical quantification are particularly prone to abuse and corruption, e.g. in the context of reductionist cost benefit analyses of complex phenomena (Saltelli & Funtowicz, 2014; Saltelli, Stark, Becker, & Stano, 2015).

In relation to the main mechanism of science's quality control system, the peer-reviewed journal arrangement is known to have serious difficulties. Extreme practices include paying for authors' slots on papers written by other scientists, and buying papers from online brokers (Hvistendahl, 2013). In 2015, the publisher Springer and the Université Joseph Fourier released SciDetect, a software to discover false scientific papers generated by an algorithm (Springer, 2015). Scientists are moreover chased by predatory publishers who charge authors for publishing without providing any control or peer review. Academic librarian Jeffrey Beall maintains lists[2] documenting corrupt practices in the publishing industry:

> The Misleading metrics list includes companies that "calculate" and publish counterfeit impact factors […]. The Hijacked journals list includes journals for which someone has created a counterfeit website, stealing the journal's identity and soliciting article submissions using the author-pays model (gold open-access).

Even without those extreme cases, peer review is presently perceived as threatened by serious dysfunctionalities (Schroter et al., 2008), and is the subject of reflections and pledges (Morey et al., 2016). The market side of publishing is also a source of concern due to the high cost of journals (see Wildschut's, this issue, discussion on open access), which has prompted Field medalist Timothy Gowers to launch a boycott of Elsevier in 2012 (Jha, 2012).

Even official metrics used to judge the quality of scientific work are in dispute. The issue is taken up in the San Francisco Declaration,[3] drafted in 2012 by a group of editors and publishers of scholarly journals, as well as in recent work by John Ioannidis (2014). This theme central to the review of the so-called Research Excellence Framework in the UK (Stern, 2016).

4. Impact of media and the public

The crisis is rapidly emerging from under the radar and has begun to be picked up by the media, e.g.:

- The well-known periodical The Economist devoted a cover page to "Bad Science & in 2013 with the rather peremptory title, "How science goes wrong";
- the American journal *The Atlantic* ran an article (Yong, 2015) on the Nosek study mentioned above on the same day the study was published in *Nature*;
- the finding that activists could easily change conservative voters' opinion on same-sex marriage quickly made it to the headlines, as did the news that the study was based on non-existent data (Nature, 2015; New York Times, 2015);
- the British daily *The Guardian*, which follows issues linked to the use of evidence for policy has run articles on the crisis (see for example, Frith & Frith, 2014; Ravetz, 2016);
- the high diffusion non-academic magazine *New Scientist* rings an alarm (New Scientist, 2016; Ryder, 2016; Saltelli, Ravetz, & Funtowicz, 2016b).

[1] http://www.wcri2015.org/

[2] By the time the present article was published the list went offline following pressure on its author; see http://retractionwatch.com/2017/01/17/bealls-list-potential-pred

[3] http://www.ascb.org/dora/

The impact of the crisis in science on public trust may be smaller than expected according to surveys (Nature, 2015), but a deterioration is possible in the current political context (Ravetz & Saltelli, 2015). This crisis of 'trustworthiness' occurs in the context of an increasing number of major scandals in other fields, starting with finance (credit crunch and after), sport (Olympics, football, athletics) and environmental regulation (Volkswagen's illicit software to circumvent emission control standards). The balance of public plausibility is now shifting towards an assumption of corruption rather than of innocence, and it is now often assumed that 'evidence-based policy' (Saltelli and Giampietro, this issue) is in practice very frequently 'policy-based evidence' (Boden & Epstein, 2006).

Statements to the effect that: "Currently, many published research findings are false or exaggerated, and an estimated 85% of research resources are wasted" (Ioannidis, 2014), or that an estimated US$200 billion were wasted in the USA in 2010 (Lancet, 2015) or that most clinical research is not useful (Ioannidis, 2016) raise concerns across a wide spectrum of constituencies: scientific editors, funders of research, university administrators, scientists, major corporations (*e.g.* pharmaceutical), NGOs, taxpayers, and citizens.

It appears that any use of science to settle a policy dispute may be controversial, and this is true not only of historically highly contentious issues such as climate change and genetically modified foods. Other subjects that have provoked dispute over scientific evidence include the culling of badgers in the UK, the relation between bees and pesticides, the fate of children born to gay couples, and the inflammability and greenhouse potential of cooling liquids used by carmakers (Saltelli & Giampietro, 2016). The level of conflict over scientific evidence in such cases justifies the reservations of scholars (Collingridge and Reeve, 1986Collingridge & Reeve, 1986) about the rightful role of science in determining policy decisions. The present debate about 'post-factual' democracy following Brexit also targets the use of evidence and expertise (Barrett, 2016), showing a constantly evolving relationship between science, policy and society.

The severity of the crisis in the governance of science has not fully registered in the discussions on science advice in the policy sphere. A substantial report issued by the OECD in 2015 on "Scientific Advice for Policy Making" does not mention a crisis in science. It refers to "crisis situations" (in which science intervenes), but the authors of the report seem unconcerned by the fact that science itself is in crisis. A similar neglect occurs in the 2016 report of the United Nations Educational, Scientific and Cultural Organization (UNESCO) on scientific advice.

For other observers instead it is important to register the situation of crisis, scientists' own responsibility in the present predicaments and the implication of this for the social uses of science. In an editorial entitled "Misplaced faith", the journal Nature asked pertly (2015): "The public trusts scientists much more than scientists think. But should it?"

More bluntly, Belinda Phipps, head of the UK Science Council, noted in an interview to *The Times* (Whipple, 2016) that the public trusted scientists only because they did not understand their work, and that scientists 'should take ethics oath, like doctors' (see König et al.'s contribution in this issue on the ethos of post-normal science). Also for Jenkins (2016) it is important to remember that experts offering advice on healthy eating or cancer cures are not immune to worlds of politics and commerce, while Colin Macilwain (2016) warns against "experts" who "crawl around after [the] elites, massaging their egos and defending their interests." Among political scientists, it is now a known secret that experts and expertise are most usefully used by lobbyists, for whom 'evidence' is a valuable currency to get the politicians' ear, in both the US (Drutman, 2016) and the EU (Laurens, 2015). Public concerns for the use of technology − already high in the field of nutrition − now include the algorithms used to take decisions affecting people's (and researchers') life, from recruiting of personnel to prison terms sentencing of defendants to adjudicating research grants (Abraham, 2016; Alexander, 2016)

The way science is perceived and defended, not last by scientists themselves, has some of the characteristics of a Baconian idol:

– In the still dominant worldview scientists are upholders of the truth, and constitute a 'body politic' (Polanyi, 1962) capable of speaking with one voice to defend humanity and the planet. The recent case of more than one hundred Nobel Prize laureates speaking in the name of "science" in relation to a variety of genetically modified rice (Achenbach, 2016) is a case in point where more care should have been taken to appraise all aspects of the matter (Hartley, 2016).
– Scientists tend to exalt science's capacity to correct itself, thus absolving their craft, and to blame the irrationality of the system on politics and economics. In this respect it is instructive to read the questions[4] addressed to the 2016 US presidential candidates by a pool of scientific institutions. Here integrity is achieved by "protecting scientists and federal agencies from political interference in their work", which is laudable of course, but forgets that the pressure is often based on scientific evidence also produced by scientists (Kearns et al., 2016).
– Scientists mostly subscribe the deficit model, whereby a lack of progress has to be attributed to the lay public's incomprehension of science and technology. The problem with citizens − it is also suggested − is not that they do not understand science, but that they are unable to sort out who the true experts are (Kahan, 2016; Widschut, this issue).

Topics which a reflexive science could usefully consider are for example the role of present day technologies to redefine the meaning of 'knowing' as discussed by philosopher Michael Lynch (2016). Another concern could be the end of the enlightenment man, and the beginning of a new endarkenment announced by Elijah Millgram (2015). Science could also confront innovation's drive to increasing inequality (Lanier, 2013) and making humans redundant (Jasanoff, 2016).

[4] http://sciencedebate.org/20questionshttp://sciencedebate.org/20questions

5. Reading the crisis

Different readings of the causes of science's crisis have been put forward. Schematically a non-mutually exclusive set could include:

a Poor statistical training and design, hubris of data mining, perverse incentives (such as publish or perish), counterproductive metrics. This is the dominant narrative, seen in the San Francisco Declaration, and in many commentators, see e.g. Fanelli (2010a, 2010b), and Begley and Ioannidis (2015).
b Science victim of its own success, senility by exponential growth and hyper-specialization (De Solla Price, 1963).
c Science as a victim of the neoliberal ideology when the 'market' takes care of allocating resources and driving priorities (Mirowski, 2011).
d Science as a social enterprise whose quality control apparatus suffers under the mutated conditions of techno-science and mega-science (Ravetz 1971).

Fanelli's analysis of scientists' behaviour as a function of different levels of competitiveness in different states or disciplines (2010a, 2010b) does not exclude Ravetz's (1971) lost ethos of little science and of its quality control arrangements. Fanelli's confirmation of Auguste Comte's theory of the Hierarchy of the Sciences, with physics at the top and social sciences at the bottom, reinforces the impression that there are in the practice and the craft of science powerful cultural and normative elements.

It is important to note that while (a) is the dominant narrative, the others three also have a considerable strength and plausibility, as discussed in Saltelli, Ravetz, and Funtowicz (2016). The more we move down the list (a-d), the more difficult it appears for a solution to be found by the scientists themselves, or from within science-centered institutions. Even relatively simple causes, e.g. the perverse influence of metrics, are hard to undo as shown by the ongoing discussion in the UK about following or resisting the 'Metric tide' (Wilsdon, 2015). Extricating oneself from 'metrics' is inconvenient for a mature established researcher and suicidal for one who is young and on a precarious career. One third of scientists in a recent poll admitted having recurred to 'questionable research practices', and in a 2% of cases to have falsified research (Fanelli, 2009). However difficult the implementation of remedies from within the house of science, new arrangements of the relations between science and society might be in order if the root causes of the crisis are to be identified in the range (b) to (d).

6. Remedies

Science's present reaction to the crisis is still in line with the mainstream reading (a), see e.g. the Peer Reviewers' Openness Initiative (Morey et al., 2016) intended to discipline authors into providing complete access to the materials used in the preparation of articles, or the San Francisco declaration, as well as Ioannidis's paper on 'How to Make More Published Research True' (2014). Both propose a list of remedies for the predicaments in science, and stress that journal-based metrics, such as journal impact factors, should not be taken as a surrogate measure for the quality of research, and, above all, should not be used in hiring, promotion, or funding decisions. James Wilsdon (2015), writing in *Nature*, calls for a "more sophisticated and nuanced approach to metrics", a theme central to the review (Stern, 2016) of the Research Excellence Framework in the UK.

These measures are sensible and justified. For example, Nature recently announced the decision to complement the journal's impact factor, which is constructed as a mean, with a two-year median in the calculation of (2016). The choice of a median is indeed less prone than the mean to give disproportionate significance to a few very highly cited papers. Yet all these approaches coming from Science's institutions only target rules and procedures. As Ravetz anticipated no formal system of imposed penalties and rewards will guarantee the maintenance of quality, in the absence of a community of scholars willing to respect and enforce collectively a set of generally unwritten quality standards (, p. 22–23; 407).

Scientists could perhaps refrain from speaking in the name of "science" against real and imaginary foes, recalling their own contribution to the current state of affairs. For instance the role of mathematics in the construction of financial products at the core of the ongoing recession (Ravetz, 2008) or the key role of science to both inform and to legitimize policy, as in the case of the negotiations for the trade agreement between the US and the EU (Cressey, 2015).

In tackling urgent social and ecological problems, scientists and science institutions could usefully adopt the 'model of extended participation' proposed by Post-normal Science (Funtowicz & Ravetz, 1993; see Editorial, this issue), where deliberation (on how and what to do) is extended beyond established disciplines to all relevant perspectives, including investigative journalists, whistle blowers and citizens with stakes in the matter. For instance, in medicine extended participation would mean inter alia that patients have a say on the framing of research questions (Miedema, 2016).

This prescription moves science from "speaking truth to power" to "working deliberatively within imperfections" (van der Sluijs, 2012) and is seen by scholars as an important step toward the 'democratization of expertise' (Carrozza, 2015). Today we see Post-normal science (see editorial, this issue) suggested in relation to science advice (Gluckman, 2014) and risk regulation (Grinnell, 2015).

Our personal and ongoing reading[5] of the crisis would suggest a more careful appreciation of uncertainty and ignorance, following the teaching of Knight, Keynes, Ravetz, and others, against the hubris of quantification often seen in science for policy. Also

[5] https://theconversation.com/profiles/andrea-saltelli-299347/articles

worth considering would be the reintroduction of different kinds of either physical or virtual Gemeinschafts, for example those of citizen science (see Wildschut, this issue) and citizen-scientists (Stilgoe, 2009) engaged in new forms of science (Ravetz & Funtowicz, 2015), and new forms of peer review and quality control (Funtowicz & Ravetz, 2015). Movement such as Science in Transition in the Netherlands (Dijstelbloem, Huisman, Miedema, & Mijnhardt, 2014) appear to have a full appreciation of the severity of the crisis, and of the need to find solutions in the direction of a democratization of expertise, and of a renewed focus on quality.

A key recommendation for citizens would be that their respect for the authority of science should be a circumspect one, mindful of the powerful interests and stakes involved in big and incorporated science. The existence of a Cult of Science (Wilson, 2016) may indeed delay the quest for a resolution, while some skepticism toward 'expertise' may be a sign of maturity rather than barbarism.

There might be partial resolution to some of the important issues discussed in the present work. But the problem and the solution are not in the same world, and it is hard to foresee a path from the problem-world to the solution-world. Even the existence of such a path is questionable. This is consistent with Stephen Toulmin's argument (1990) that modernity's blueprints for the future are exhausted. In short: a new storyline is needed, requiring acknowledgment of ignorance and failure.

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