

***Science and Society at the Crossroads: Skepticism vs. Denial and Elitism vs. Public Engagement***

*Report on an international meeting at Chicheley Hall, 15-16 June 2015*

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**Executive Summary: Exogenous and endogenous threats to the cultural authority of science**

Why do people reject the fact that HIV causes AIDS? Why do some parents withhold potentially life-saving MMR vaccinations for their children because YouTube videos accuse scientists of being corrupt? Why would a U.S. Senator write a book entitled *The Greatest Hoax: How the Global Warming Conspiracy Threatens Your Future*, when scientists are 95% certain that climate change is human induced and when only a handful out of 14000 relevant peer-reviewed articles oppose that consensus?

There is strong evidence that attitudes towards scientific findings are determined by variables such as people's worldview that have little to do with scientific evidence. For example, findings that entail regulatory implications for industry—such as the link between tobacco and lung cancer or between fossil fuels and climate change—are opposed by people to whom the preservation of unregulated free markets is crucial. If rejection of scientific findings becomes widespread, society runs the risk that policy decisions are not based on the best available scientific evidence. It is not clear how the scientific community and society in general should respond to those exogenous threats that originate outside the scientific community.

At the same time, there has been much recent concern about the integrity of science from within the scientific community. In some disciplines, such as psychology or genetics, there has been much concern about a replication crisis because a substantial proportion of phenomena does not appear to replicate. Likewise, concerns have been raised about questionable research practices and the misapplication of inferential statistics (e.g., techniques known as p-hacking). In response to those issues, there have been numerous initiatives, such as the Open Science Foundation, that seek to enhance transparency and ensure that data are freely available. Nonetheless, although much work has been done, it is not entirely clear how the scientific community should respond to those endogenous threats that arise within the scientific community.

Those exogenous and endogenous threats to science were discussed at an international meeting at Chicheley Hall in June of 2015. The meeting included:

- Academics whose research has evoked a backlash from vested interests or segments of the public in disciplines ranging from tobacco and alcohol research to medical research and research in climate science and psychological investigations of the rejection of science.
- University Executives who are managing the responses to inconvenient research.
- Academics whose research addresses the positioning of science in society and who take an interest in alternative means of quality control, such as post-publication peer review.
- Experts on public engagement and the communication of controversial scientific topics.

The enclosed testimonies report the personal experiences related by participants of the meeting. They do not offer a fixed solution, but they describe the landscape of the exogenous and endogenous threats to science under which many scientists are operating today, and they present some preliminary thoughts about their possible solutions.

## **Part I: Exogenous Threats**

## The topsy-turvy world

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In September 2010 I published an opinion piece on the website of the Australian Broadcasting Corporation (ABC) in which I made what I thought was an uncontroversial claim, namely that scientific opinions are filtered on the basis of evidence, and that some scientific opinions cease to have credibility when there is no evidence to support them. Somewhat to my surprise this article generated over 2,000 comments and some hateful email.

A further ABC article about the costs and benefits of climate mitigation compared to business as usual elicited more hate mail, but also, for the first time, formal complaints with the University of Western Australia, my host institution at the time. The complaint cited the fact that I opined about the decision trade-off in climate change, which as a cognitive scientist I was allegedly not entitled to do. (I published several papers on this issue in *Climatic Change* last year.)

During the five years hence, the number of complaints against me with universities in Australia and the UK, and several journals around the world, must by now run into the three digits. I do not believe that there has been a period longer than a few months since late 2010 in which I was not the target of some investigation by a university or journal. On Twitter, a single particularly obsessive individual has produced a defamatory or disparaging tweet about me every 72 hours for the last 3 years. Another individual has accused me repeatedly of “fraud” in public because his acquaintance with Excel pivot tables does not extend to the distinction between latent structure and measurement error in multivariate data sets.

To illustrate the nature of the complaints, I was once accused of “malice” by an internet blogger based on the fact that one of my co-authors let two people, whom I have never met and whose identity I still do not know, refer to that blogger disparagingly in a private password protected forum that was subsequently hacked.

As this is a somewhat baroque construction, an analogy may be helpful: Suppose you have a friend called Joe. Joe holds a party and invites some people (but not you) over for drinks. Two of those people, Jack and Jill (whom you have never met), have a conversation in a corner of the room and, unbeknownst to Joe, they make disparaging remarks about a blogger (let’s call him Frank). Sadly, some criminals have illegally wire-tapped the party, and so Jack and Jill’s comments become public. Frank thereupon accuses *you* of malice because even though you have not been to the party, you have co-authored with Joe. Such is the topsy-turvy world of climate denial, which some people—mistakenly and frivolously, in my opinion—refer to as “skepticism.”

This topsy-turvy world manifests itself in a variety of unconventional ways. For example, a few years ago I received an email from a person who called herself Arlene, which read as follows: “Dear Prof Lewandowsky, We have never met, although we do share a background in the field of psychology, so I feel emboldened to ask for your professional advice. ... I recently began blogging, especially about climate change, and after a month my site was

noticed. Noticed by the wrong people, sadly. Readers of Tim Blair and Andrew Bolt have swamped my site with genuinely abusive comments, many relating to my disability, which I find very hurtful. So my question to you is this: How do you deal with monsters like this?" (Tim Blair and Andrew Bolt are two nationally-known right-wing bloggers in Australia with a somewhat dubious history; Mr Bolt, for example, has been convicted of racial vilification, and I won an adjudication from the Australian Press Council against him on one occasion).

I replied to this email as follows: "... they [commenters] are like the school bullies whom no one really liked and who didn't really have close friends, only followers. I deal with those comments and actions largely by ignoring them. ... Unfortunately, there presently is not much else that can be done about those comments. ... Bear in mind that a proportion of those comments is orchestrated *and for all we know there are only a handful of people with multiple electronic 'personas' each*, who are paid to create disproportionate noise." [Emphasis added].

A few days later this conversation was posted on the internet together with logical accoutrements of the kind "Lewandowsky is gullible, therefore climate change is a hoax". It turned out that I had been entrapped by a "sock puppet"—a non-existent persona that was remote-controlled by persons unknown. In an ironic recursive twist, I had warned a sock puppet about sock puppets.

Lest one think that this is an isolated case, I have also received fake Facebook invitations to befriend a (faked) climate scientist. (And of course my blog is under frequent attack by hackers, although there is nothing worth hacking there. I therefore look forward to the attacks being successful, so that information that is already public can be "revealed" on the internet. It appears that information has to be stolen or obtained by entrapment before those who deny the laws of physics will pay attention to it.)

The bottom line is that these brief vignettes are merely tips of an iceberg of concerted attempts by a small but vociferous group of individuals around the world to do professional harm to me and to undermine the reputation of my science. At the time of this writing (September, 2015), plans are being hatched very openly on Twitter and on blogs to "take down" me and several of my colleagues, and to harass journal editors into retraction of my inconvenient papers. To illustrate, this campaign also targets *Psychological Science*, whose Editorship recently changed hands, in the belief that the new Editor may be more receptive to a plethora of recycled allegations that had been dismissed by the previous Editor.

I am not the only person targeted by those same individuals in the climate arena, and the same playbook is being followed during attacks on scientists in other disciplines.

To me, the most pressing questions that arise out of those experiences are:

(a) how scientists can be protected against such harassment to guard against the (self-)censoring of science. I believe that this type of harassment has demonstrably affected the conduct and reporting of contentious scientific findings, including in particular climate science.

(b) how the scientific community can differentiate between harassment and bad-faith attacks on the one hand, and bona fide criticism and necessary self-correction on the other. I am deeply concerned about failures to replicate, failures to release data, and other harmful practices in science. Full disclosure: I always release my data (although that hasn't stopped

accusations of me “covering up” results) and my climate-related work has demonstrably replicated.

(c) how the scientific community, university executives, and journal editors can be sufficiently informed about the strategies and activities underlying such attacks on scientists so they do not (inadvertently or otherwise) provide a fertile ground for further attacks.

## **A climate of intimidation**

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There is a growing list of examples of researchers on climate change, particularly in the United States, who have been subjected to personalised attacks on their reputation and character by companies and lobby groups that seek to promote their causes or to protect their interests through the intimidation of university staff.

These researchers have included Michael Mann, Distinguished Professor of Meteorology at Pennsylvania State University, who has provided a stark account (Mann, 2012) of the abuse and harassment he has suffered from a variety of sources, including politicians, magazine columnists and bloggers. Oreskes and Conway (2010) describe how a number of climate scientists in the United States have been targeted, including by individuals who have received funding from fossil fuel companies. These attacks have been mounted in a number of ways, including direct verbal attacks, e-mail messages and letters, blogs, newspaper articles, and even formal political and legal processes.

Examples of such assaults on academic researchers in other countries, such as the United Kingdom, are less well-known but still noteworthy. For example, Phil Jones of the Climatic Research Unit at the University of East Anglia in Norwich, UK, told 'The Sunday Times' newspaper about receiving death threats and contemplating suicide after some of his e-mail messages were hacked and posted on websites by climate change 'sceptics' (Girling, 2010).

While most attacks have focused on academic staff, other individuals who are affiliated to the research community have also been targeted. In 2006, for instance, a UK Member of Parliament, Chris Huhne, told the House of Commons how a representative of ExxonMobil tried to mislead him into believing that I had been fired from my job at the Royal Society for publicly challenging inaccurate statements by the company about climate change (Ward, 2009). This false rumour has since been repeated in a pamphlet (Montford, 2012) produced by the Global Warming Policy Foundation, a lobby group for climate change 'sceptics' set up by Nigel Lawson, a Conservative peer. And since I moved to a post at London School of Economics and Political Science in 2008, I have been subjected to a variety of written and verbal attacks, some co-ordinated, particularly from bloggers (eg Delingpole, 2010).

In most cases, the targets of these attacks have attempted to cope with the help of their respective employers. In the United States, Professor Mann has taken the additional step of helping to establish the Climate Science Legal Defense Fund to provide financial support and advice to researchers seeking to use the law to protect themselves.

However, universities and scientific institutions, including scientific societies and national science academies, have not spoken out against the targeting of researchers in climate science in the same way as they have acted to back those threatened because they carry out research involving animals. More could and should be done by universities and scientific institutions in the UK, United States and other countries to prepare researchers on climate change and

other controversial subjects about how to deal with a range of responses to their work, including personal criticism, abuse and harassment.

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## **The cost of producing inconvenient data on our favourite recreational drug**

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For the past 25 years I have worked in the contentious area of alcohol policy and public health/safety. It is contentious both because the great majority of people in developed countries consume alcohol and because it is part of many cultural activities. However, evidence has been mounting that alcohol is not so dissimilar from tobacco in that, for example, risk of a range of cancers starts at even one drink per day. Furthermore, the idea that low volume consumption is cardio-protective is also being reconsidered. The study of harms from alcohol and also of population health and safety benefits from implementing policies such as raising prices or reducing physical availability can be profoundly unpopular, especially with regular drinkers. Furthermore, both small and large commercial vested interest groups and their front organisations increasingly work to minimise public perceptions of the risks of drinking, draw attention to evidence of health benefits, and discredit evidence supporting policies which would reduce their sales. In this context, scientists in this area often experience various forms of abuse, harassment and occasionally even physical threats.

In recent years I've been particularly involved with researching the impact of minimum pricing on alcohol consumption and related harms, a policy which my research has helped shape (e.g. Stockwell et al, 2012; Stockwell et al, 2013a), which in various forms is implemented in Canada and is being contested in both UK and European courts by public health versus corporate interests. I frequently experience "hate mail" and sometimes abusive phone calls after my work is featured in the media. Mostly I ignore them but I realise they do leave a toll. They make me wary about talking publicly about this work and, further, because of comments made on electronic media reports are read by my friends, family and colleagues they may frame how people regard me, positively or negatively. Sometimes very crude, insulting and personal attacks are made both to me and even to my family. Occasionally intimidation surfaces. On one occasion an individual emailed to say he was visiting the president of my university to explain that because of my activities he was withdrawing a large donation and, further, he indicated he would be visiting my office in a way which implied the possibility of violence. Campus security took the threat seriously.

At another level, the impact and credibility of my work has been systematically attacked by corporate interests and their representatives. Because some of my research has been supportive of the policy of minimum pricing, my publications are often immediately condemned by a variety of commentators and, it would appear, hired academics whose one-sided and usually completely false criticisms are widely circulated (Stockwell et al, 2013b). As these criticisms appear in twitter, blogs, press releases, newspapers, high-profile magazines and news websites, they become more visible than my research and influence judgements made by both the public and decision-makers who serve them. Attempts to correct even the most blatantly false charges are hard to make as visible as the criticisms themselves—coming as they do from wealthy corporate interests. I have also had the experience of an academic from another institution gaining access to our data under false pretences and then providing questionable reanalyses which were published as a refutation. In one instance it was clear that the exercise was funded by a European spirits manufacturing organisation. I'm generally in favour of making data available for reanalysis—but this was

mischievously motivated. The suggestion of finding another institution or group willing to independently replicate results is a good one.

I support the recommendations for making the research process more transparent, open to criticism and to promoting a process of continuing critique/reanalysis. This should serve to protect the integrity of the science itself, the reputation of scientists and weaken misguided or corrupt efforts to discredit studies and bodies of research.

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## **Tobacco control, the tobacco industry and tobacco harm reduction: plus ça change, plus c'est la même chose?**

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There is now a fairly extensive literature on how corporate interests can undermine or distort science to influence policy and promote profit. In this literature, the tobacco industry is often held up as an exemplar. Cigarettes are a hugely successful, legal consumer product that is harmful to health. As the scientific evidence of harm has accumulated the industry has fought a determined battle to cast doubt on that evidence in order to challenge regulation and maintain sales. In the early 1960s, the US Surgeon General and the UK's Royal College of Physicians published reports<sup>1,2</sup> that outlined the harm from tobacco and set out the measures needed to protect public health. Yet due to the industry's success in funding the 'merchants of doubt'<sup>3</sup> it took more than 50 years for the recommended measures to be implemented in most developed countries.

Today, when smoking prevalence has significantly declined in many jurisdictions, it would be easy to assume that the industry had given up on its efforts to undermine health research. Yet this is not the case. In the UK, my own experience provides just one example. Between 2006 and 2010 I served as the UK government's scientific adviser on tobacco control, an appointment that followed a number of years of my own research on smoking cessation. While serving as scientific adviser I coordinated the evaluation of smokefree legislation in England. In 2011 the Department of Health published my review of these studies<sup>4</sup>, which set out reductions in exposure to second hand smoke, a fall in hospital admissions for heart attacks and improvements in child health that arose as a result of the legislation. Imperial Tobacco responded by publishing their own report 'The Bauld Truth' that described my review as 'lazy' and 'deliberately selective'<sup>5</sup>. What followed was a campaign of abuse—online, by post and over the telephone. Mocked up photographs of me appeared on websites and various bloggers outlined what they'd like to do to me given the opportunity. These kinds of misogynous comments were accompanied by more detailed critiques, epitomised by one blogger's 'Letter to Linda' that is still available online<sup>6</sup> (beginning 'Sit down Linda, and listen very carefully...'). My University alerted the police, removed my name from my office door and instructed mailroom staff to check post addressed to me before forwarding it.

The industry also used, and continues to use, more traditional means of taking up the time of academics in order to divert them from tobacco control research. Since 1999 Cancer Research UK has funded the UK Youth Tobacco Policy Survey, conducted by the team I now lead at the University of Stirling. For two years from 2009 Phillip Morris International (PMI) submitted a series of Freedom of Information requests to obtain 'all data, all original files and transcripts, all meeting notes' from this repeat cross sectional survey<sup>7</sup>. The YTPS includes data on 11-16 year old's attitudes to smoking, and their exposure to, and views on, tobacco marketing. Data the industry cannot obtain themselves, provided to us in confidence with the consent of parents and young people. Dealing with these FOI requests and trying a number of

options to protect these data took up an inordinate amount of time. This finally stopped in 2011 when a journalist, Steve Connor of The Independent, contacted me having been told by a Scottish colleague about this process. He covered the story on the front page of The Independent<sup>8</sup> and my colleagues and I were interviewed by journalists from all over the world. The public responded, writing to newspapers outlining why children should be protected from becoming the industry's new customers. After this coverage, no further FOI requests from PMI were received, although we do receive them from time to time from other interested parties when we publish findings from related studies. Steve Connor received an award from the European School of Oncology for his work on this issue.

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## **The perfect storm? Internal conflicts and normative tensions in climate change research**

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Climate change challenges so many of our dearly-held assumptions about how things are—and how they *should* be. You can see this reflected at every level of society, from the macro (does tackling climate change mean abandoning the idea of perpetual economic growth?), right down to the micro (everything from the way we eat and travel, to the way we heat our homes is shaken and stirred by climate change).

It's no wonder that even the best-intentioned people recoil from the brutal interrogation into our lives and livelihoods that climate change represents. The disruptive power of climate change is both exhilarating and terrifying: there is a perennial appeal in falling back on the status quo. And one of the less obvious—but nonetheless important—places in which climate change is challenging centuries of convention and established wisdom is within the scientific community itself.

Although there are no written 'rules of the game' for science and scientists, there are some widely accepted principles and standards—typically called 'Mertonian' norms, after the sociologist Robert Merton. These norms trace the boundaries of what constitutes acceptable scientific conduct. Merton identified four norms, of which two are especially pertinent for reflecting on the tensions experienced by climate change researchers: 'disinterestedness' and 'organised scepticism'.

At first glance, these two norms - the idea that scientists should not have a vested interest in particular research outcomes, and should submit any claim or theory to rigorous scepticism before accepting it—seem as applicable to climate change research as they to any other area of science. But a closer inspection reveals that these norms (constructed by scientists as a community over time, rather than handed down on a tablet of stone), are more complex and problematic than they first appear.

### ***Who are the sceptics?***

Despite seeming self-evidently virtuous for scientific practice, 'scepticism' and 'disinterest' are only understandable with reference to a particular context, and socio-political history. While sceptical testing of new (and old) knowledge claims is clearly the bedrock of scientific practice, this process eventually reaches a natural conclusion—an undefined point at which a hypothesis mutates into a 'fact'. There are hundreds of thousands of scientific 'facts' which scientists no longer interrogate. Even though strictly speaking it is always possible that even 'proven' hypotheses could be falsified by new data, we mercifully do not live our lives in this way, instead treating knowledge claims where there is a sufficiently high level of certainty and agreement as 'facts'.

There are many such knowledge claims in climate science—hypotheses that have attracted such a high level of agreement and consensus that there is no longer much justification for continually questioning and challenging them. But outside of the scientific community, these very same ‘facts’ are highly contested. These voices often describe themselves (or are described) as ‘sceptics’, a confusing kind of double-think around the concept of organised scepticism in science. The problem is not so much that the label ‘sceptic’ has been used and abused in climate change debates, but that a fundamental scientific norm (organised scepticism) reveals itself to be an extremely value-laden and politicised concept in the context of climate research.

Modern-day ‘climate sceptics’ claim that the organised scepticism of scientists has been abandoned on climate change, with some evidence that their views have ‘seeped’ into the scientific discourse itself, and even the peer-reviewed literature via scientists becoming overly focused on what they *don’t* know at the expense of what they *do*. What seems clear is that ‘scepticism’ is at least partly in the eye of the beholder—one important way in which climate change challenges the notion of how science is (and should be) conducted.

### *Disinterest*

Last year, Chris Groves and I published a short comment piece in *Nature Climate Change*, titled ‘Breaking the Climate Change Communication Deadlock’. We asked why—despite 25 years of awareness raising and attempts to ‘educate’ the public on climate change—such a fragile and superficial level of engagement with climate risks prevails. We argued that climate change communication—the process of engaging the wider public on this issue—is trapped between two competing sets of norms: those that govern how scientists should act, and those that specify effective public engagement.

Scientists may operate using the scientific method, tentatively accepting claims until further evidence is forthcoming. But the instant that these claims leave the laboratory (as they must, if it is to have any bearing on the world beyond) a different set of norms kicks in. And this alternative set of normative guidelines pull in precisely the opposite direction.

We know from several decades of empirical social science and psychological research that deeply held values and views about the organization of society and political ideology are the primary determinants of engagement with climate change. Effective climate change communication means engaging with these values and political beliefs, not simply presenting the facts of climate science in a clearer or more concise way. The problem is not a lack of knowledge: the problem is that scientific findings on climate change pose deeply challenging questions about how we should live, and structure society. And unless climate change communication strategies engage directly with this (empirically demonstrated!) set of considerations, a communications deadlock will remain.

Our commentary concluded that:

*“If, as research funders increasingly insist, scientists ought to engage the public, they face a conundrum: embrace the norms that govern the social domain (where values, ideology and social identity shape the debating space) and effectively engage their audience, or stay within the boundaries that define professional scientific practice and commit to a strategy that is very likely to fail.”*

### *Climate change vs the status quo*

Climate change is not the first topic to raise difficult questions about the role of science in society, and the boundary between the objective scientific method and the unavoidably (to some extent) subjective process of funding, conducting and promoting science. Take any politically contentious issue with a scientific core—GM crops, nanotechnologies, vaccinations—and there will be challenges to the authority of scientists, disputes over the credibility of research, and values-based arguments around who the ‘real’ sceptics are.

But as is so often the case, climate change cranks the dial up to 11. Because it raises such fundamental questions about human affairs and our place in the world—everything from justice and inequality, to security and economic stability—the blowback into science is fierce and intense.

And something has to give.

The political tensions around climate change are not going to disappear, and they are not going to be quietened by polite, disinterested, neutral, information-based engagement. That doesn’t mean that scientists and science-communicators should be rude, biased, or unhelpfully partisan. But it does mean that we need to collectively consider whether the conventions which govern how scientists *ought* to behave are immutable. In the context of climate change, they do not seem entirely fit for purpose, and they may even be making it harder to conduct of high-quality science (and social science).

Returning to our *Nature Climate Change* commentary one final time, we argued that:

*“...the appropriate societal institutions do not yet exist to accommodate and ease the normative tensions within climate change research and communication. Scientists certainly have a role to play, but they cannot overcome the tensions on their own. Efforts should be concentrated on creating and supporting new institutions and societal infrastructure that provide a buffer between the science of climate change and the complex challenge of engaging the public. These institutions should be explicitly tasked with carving out new space between the normative tenets of scientific research and public engagement. The purpose of these new, hybrid institutions would be to catalyse new conversations about climate change.*

*These events would not be designed to make an economic case, communicate scientific facts or win an argument, but to allow people to express and discuss their concerns, fears, dreams and hopes for the future, providing answers to that troubling question ‘how shall we live?’ They would involve explicitly political voices and views, but not themselves pursue politicized ends.”*

So this is an argument for more—not less—politics in climate change communication. This does not mean aligning the goals of climate science with the advocacy strategies of a campaign group. But through a diversity of partnerships with actors from across the social and political spectrum, concerns about partisan influence can in effect be neutralized. The acceptance that everyone approaches climate change with their own ethical and political values—and that engaging with these is an appropriate aim for climate researchers—would be a major step towards easing the normative tensions at the heart of the field.

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## **A virus and a trial**

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In 2010 we started recruiting children with Chronic Fatigue Syndrome (or ME) into a trial investigating whether the Lightning Process (LP) (<http://lightningprocess.com/>) was effective and cost effective as an addition to Specialist Medical Care. We got funding for the trial as children and their parents kept asking me whether LP “worked”. I was interested in whether it might be harmful. At around the same time, I was asked to speak to a patient support group about new research in the field. I discussed lots of different research findings including what I thought about XMRV, a virus which was claimed to be the cause of CFS/ME. I was concerned because the authors of the initial paper had started to suggest that children were particularly vulnerable and should consider anti-retrovirals. The authors had a conflict of interest and other research groups had not been able to replicate the findings. In the end, XMRV was shown to have been created in the lab and had nothing to do with CFS/ME.

The fact that I was doing a trial investigating an alternative intervention and had criticised the XMRV story, made a few people very angry indeed. Over the next 12 months, I was sent aggressive and threatening emails, dealt with lots of Freedom of Information requests and was reported to: the General Medical Council (GMC; our regulator); the University of Bristol (my employer); the National Society of Protection of Children and the National Research Ethics Service. I was defamed in the accusations made to the regulatory authorities and in an article in the Church Times. A YouTube clip was made from the talk I gave to the patient support group dubbed with different words. The complaints were quickly dismissed and the University and patient support groups were supportive. The police got involved and helped me realise the complaints came from what they called “extremists”, that this was a co-ordinated campaign and that nothing had come from my patient group. They recommended that I and other CFS researchers went public and I told my story through newspapers, TV, radio and scientific journals. The abuse stopped. I have received one apology three years later.

I think that as a research community, we need to be aware this happens and prepare researchers in fields where this is likely to happen. We should ensure that institutions provide training in dealing with FOI requests, defamation and accusatory emails and complaints to institutions. I believe institutions have a responsibility to protect researchers from harassment and abuse, and this requires some thought on how we should identify those at risk and prepare them for what might happen. Institutions can use the approach adopted by the GMC which explains why accusations lead to investigation but supports clinicians with clear time lines and what they can expect through the process.

## The case of the PACE trial

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The PACE (Pacing, graded Activity and Cognitive behaviour therapy: a randomised Evaluation) trial was a randomised controlled trial which tested four non-drug treatments advocated for an illness called chronic fatigue syndrome (CFS), sometimes called myalgic encephalomyelitis (ME).<sup>1</sup> Every participant received specialist medical care (SMC); one group received SMC alone, and the other three groups received a supplementary therapy of adaptive pacing therapy (APT), cognitive behaviour therapy (CBT), or graded exercise therapy (GET). The trial was designed and implemented with the help of both a patient charity and a patient representative.

Overall, we recruited 640 patients. Almost all participants (95%) provided outcome data at 12 months, with no significant differences between treatment arms in missing data. Only 8% of participants dropped out of treatment, again with no significant differences between treatment arms. Both CBT and GET led to significantly greater improvements in the two primary outcomes of fatigue and physical disability than did either SMC or APT. There was also a similar pattern of results in the two subgroups that met alternative case definitions for CFS and ME. There were no significant differences in any of the five measures of harm between treatment arms. These results were important in confirming to patients, healthcare professionals and commissioners that the promise of CBT and GET found in previous smaller trials was justified, and that these treatments were safe to receive, if delivered as designed by the appropriate therapists.

The PACE trial was criticised by patient organisations from the moment it was funded.<sup>2</sup> Our understanding is that this opposition reflected the fact that the trial did not focus on a biomedical approach to the illness, and that it tested a treatment (GET) that was thought by some patient organisations to be harmful. Strategies against the trial and those who led it included a public petition to the UK prime minister and formal complaints to the funders (including one of 443 pages), our employers, and journal publishers.<sup>3</sup> Not one of these complaints was upheld, but they did take considerable time to address.

As a result of concerns about confidentiality, one participant withdrew their consent for us to use their data, some two years after leaving the trial; this led to us having to restart the analysis, which caused a four month delay in publication of the main results. A large number of Freedom of Information Act requests seeking information on all aspects of the trial were received since the main results were published in 2011. Some requests were complied with, so long as they did not compromise medical confidentiality, future publications, or academic safe space to deliberate (our patient representatives asked us not to release minutes of meetings for fear of harassment). Declined requests were sometimes appealed all the way to the third stage of the appeal process; the Information Tribunal (a court of law).<sup>4</sup> All appeals were rejected, two being considered vexatious. These requests and appeals took up a considerable amount of time to address, which meant that some secondary papers were delayed. There was even a debate about the trial in the UK House of Lords.<sup>5</sup> Our deliberate policy, to help allay concerns about the trial, was to be as transparent as possible regarding what we did, while also protecting medical confidentiality and our staff and patient

supporters; this included publishing the protocol and the statistical analysis plan, and paying for open access to all publications.

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## Government Suppression, A Contributor to Public Skepticism

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In 1996, I published a commentary in *Nature* about problems with EPA regulations, which I attributed to the reorganization and politicization of EPA's Office of Research & Development (ORD).<sup>i</sup> EPA headquarters accused me of violating ethics rules and the Hatch Act, which carries criminal penalties.<sup>ii</sup> Fortunately, the U.S. Department of Labor (DOL) investigated and ruled that EPA's actions violated federal whistleblower statutes.<sup>iii</sup> After dead-ending my career, EPA offered to transfer me to the University of Georgia under an Intergovernmental Personnel Act (IPA) appointment if I agreed to resign my EPA position when eligible for retirement. UGA, in turn, promised to seek a tenured full professorship for me if I accepted EPA's offer.

At UGA, where I continued to draw my EPA salary, I published a research article in *Nature* in 1999 that raised concerns about EPA's policies on land application of sewage sludges, which often contain EPA-listed priority pollutants at millions of times higher concentrations than EPA permits in air or water.<sup>iv</sup> In retaliation, EPA headquarters ordered that my local laboratory director, Dr. Rosemarie Russo, be transferred for approving the *Nature* article.<sup>v</sup> EPA's retaliations prompted the Committee on Science in the U.S. House of Representatives to hold two hearings in 2000, which failed to deter EPA.<sup>vi</sup> The head of EPA's Office of Wastewater Management and one of his subordinates later met with wastewater industry executives to discuss my research. Assisted by the EPA subordinate, the company published false allegations of research misconduct against me in 2001, and provided them to a National Academy of Sciences (NAS) panel that EPA had funded to investigate the scientific basis behind EPA's (503) sewage sludge rule.

In 2002, the NAS issued a report concluding that there is no evidence sewage sludge applied under EPA's sludge rule has ever harmed public health or the environment.<sup>vii</sup> In its report, members of the NAS panel extensively appropriated materials from the discussions and recommendations in my unpublished manuscripts being cleared by EPA, but omitted all of my evidence documenting adverse health effects. Before publishing their report, they removed citations indicating the sources of the information they found useful. Only one citation to my work remained in the advance electronic version of the NAS report; and it was removed before releasing the final version.<sup>viii</sup> *Environmental Health Perspectives* rejected my work upon submission, claiming that virtually all of it was drawn from the NAS report. Editors reversed their decision once I provided copies of my EPA-cleared papers proving that the reverse was true.

That same year, Congress passed the *No Fear Act* to better protect federal employees; however, lobbyists from the Senior Executives Association neutralized the legislation by having it immunize managers.<sup>ix</sup> Then, in 2003, EPA bypassed government IPA regulations requiring that I continue working at EPA until 2007, and unilaterally processed my retirement. UGA then reneged on its promise to seek a permanent position for me. My Department Chairman testified to the U.S. Department of Labor that UGA's Provost advised

him not to hire me, saying: "We're dependent on this money...grant and contract money... money either from possible future EPA grants or [from] connections there might be between the waste-disposal community [and] members of faculty at the university."<sup>x</sup>

In a news article and editorial published in 2008, *Nature* covered a decision by a federal Judge who ordered the USDA to pay damages related to sewage sludge, and noted that a multi-university study had independently confirmed my research findings. The judge found that data EPA and the University of Georgia published to discredit my research and a lawsuit filed by dairy farmers were fabricated.<sup>xi</sup> In the wake of the bad press, UGA terminated my temporary appointment as a Visiting Scientist. In 2011, *Nature* described attempts by EPA policymakers to discredit my research as a prime example of political interference.<sup>xii</sup>

## **Part II: Endogenous Threats**

## Post-publication peer review

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Most authors of scientific papers heave a sigh of relief when a paper is published. They will have laboured over data collection and analysis, spent hours writing and polishing the manuscript, and responded to idiosyncratic reviewers and editors. What they do not want at that point is to have people raising a whole new set of queries about the paper. Yet science is a process, and there are some clear examples where criticisms raised after publication have highlighted important issues.

A good example is the so-called 'arsenic life' story. In December 2010 NASA held a press conference to highlight a paper in *Science* (Wolfe-Simon et al 2010) describing a micro-organism that used arsenic rather than phosphorus in its DNA.

([http://science.nasa.gov/science-news/science-at-nasa/2010/02dec\\_monolake/](http://science.nasa.gov/science-news/science-at-nasa/2010/02dec_monolake/)) This was radically different from any life-form known on earth and generated speculation that it might provide a mechanism for life to develop on other planets. However, in a matter of days after the announcement, science writer Carl Zimmer wrote a piece for *Slate*, a popular online magazine, based on interviews with other scientists entitled "This paper should not have been published"

([http://www.slate.com/articles/health\\_and\\_science/science/2010/12/this\\_paper\\_should\\_not\\_have\\_been\\_published.html](http://www.slate.com/articles/health_and_science/science/2010/12/this_paper_should_not_have_been_published.html)). As explained by Zimmer on his blog,

(<http://blogs.discovermagazine.com/loom/2012/07/08/live-blogging-arsenic-life/#.VX11R0bznm4>), the critics were ultimately proven true: the bacteria could grow with very low levels of phosphorus, but they were not substituting arsenic. An interesting feature of this story, though, was the response of the original authors, who refused to engage with Zimmer, writing: "The items you are presenting do not represent the proper way to engage in a scientific discourse and we will not respond in this manner." With hindsight, this seems remarkably short-sighted. The only way usually available to critique a published paper is via the slow and uncertain process of submitting a Letter to the Editor. In contrast, the debate about this paper on social media led to scientists collaborating to perform a very public and speedy replication study, which benefited from the views of many experts in the field.

But should we then have a free-for-all, whereby authors are obliged to engage with anyone who wants to critique their work? Scientists who work in controversial areas could then be targeted by individuals who are motivated to discredit the work and/or take up valuable time by raising spurious criticisms. For example, when evolutionary biologist, Richard Lenski, showed evolution in experimental colonies of bacteria, he was beset by creationists who accused him of not making data available ([http://rationalwiki.org/wiki/Lenski\\_affair](http://rationalwiki.org/wiki/Lenski_affair)). Eventually, Lenski wrote a critique of their demands that was so devastating that it has gone down as a major event in the annals of creationist debunking. However, the campaign against him took up his time and energies and may well have influenced the general public to be sceptical about the work.

Is there a way of resolving the need for timely debate with the need to protect scientists from mischievous and hostile attacks by those with an unscientific agenda? One solution is to have a forum for such debate which restricts commentary to those with scientific credentials.

PubMed Commons was set up by NIH to serve this purpose for biomedical papers (<http://www.ncbi.nlm.nih.gov/pubmedcommons/>). Commentaries can be posted alongside the abstract on PubMed, but there are restrictions on who can post. Commentators have to have themselves published a paper that is abstracted on PubMed, they cannot be anonymous, and their comments can be rated by readers, so that anyone who tries to use the Commons as a platform for promoting a particular agenda can be identified and potentially debarred. Evaluation of PubMed Commons is ongoing; to date it seems that the take-up has been limited, but the comments that do appear are of high quality. There have also been instances where original authors have responded positively, by engaging in debate, or even by doing suggested analyses or posting data online.

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## Windows on the World and the Cultural Standing of Science

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Returning from the workshop I spoke to a friend who works in a large corporation. Upon hearing the examples of corporate influence on science, she said that, prior to signing a billion-pound contract, the CEO of her company flew the CEO of the potential client to Monaco for a weekend of Formula One racing and champagne. She saw that as “relationship building” appropriate to that corporate context. For two CEOs, both with a pay-check around the £10 million mark, a £10,000 weekend in Monaco may indeed have the significance of meeting up for tea and cake.

Juxtaposing this with the different perspectives we heard at the workshop reminded me of several things. We probably all share certain individual psychological drives: the need to feel right—not wrong—in our view of the world and our place within it; our need for social connection and abhorrence of rejection; and the ways by which we try to make sense of the motivations of others. However, we express these in ways and using mental tools that are peculiar to us individually. Scientists meet at conferences to connect; they conduct experiments and write papers to prove that they are right. Disaffected publics nowadays connect and hone their argumentation on blogs. For CEOs, “truth” lies in growth numbers and “connection” is found in sumptuous business trips and parties.

Problems arise where these worlds meet. When corporates want to fly a health official out for a weekend trip, this is an attempt to build relations as corporates normally would do. But it is inappropriate if interests should remain entirely perpendicular (as expressed in Article 5.3 in the case of tobacco). When a member of the public sees their sense of being right contradicted by scientific findings, writing a rude email or filing an FOI request are expressions of the discomfort felt at the challenge. Unfortunately, this may completely underestimate the impact (in terms of additional workload and emotional distress) this can have at the receiving end. Conversely, an academic who becomes frustrated with the rejection of scientific findings may turn to science to deal with the challenge. Yet, he/she may also completely misunderstand the effects this will have. I recently encountered the lament of a climate scientist at being targeted by an abusive tweet of a sceptic. “All I had done was point out his conspiracy ideation”, he said. Thereby failing to understand that at the receiving end his academic language translated as “You are crazy”, with all the might that the cultural authority of science bestows on the statement.

As scientists, we tend to believe that science is superior as an epistemic process to markets and blogs (and I do of course agree with that). On an abstract level many people *do* accept the cultural authority of science. Everyone wants a slice of the science pie to prove that they’re right, and in my opinion that is the main reason why difficult debates are often being fought on the science front, and not the policy front. It is the perceived cultural authority of science, not the lack of it, which leads to situations in which certain scientific findings become controversial.

In practice this does not mean that individuals roll over and accept that their own feeling of being right comes second to what science tells them. To expect this would be to fall prone to a false consensus effect: “I accept this, therefore so should everyone else”. In fact, scientists themselves do not do this easily. If they did, there would be no long-running disagreements and controversies within the science community itself. As physicist Max Planck allegedly wrote: “Science advances one funeral at a time”.

I second Warren Pearce’s final quote that science gives us the best we can do at the time. This seems for me to include dropping the idea of cultural “authority” of science and replacing it with ideas of “good standing” or “trust”. Perhaps it might also be fruitful to change the language of controversial science debates from “truth wars” to “games of risk”.<sup>1</sup> This can open up possibilities to move beyond the accept/deny gridlocks we find ourselves in today.

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<sup>1</sup> Paraphrasing Dr. Joe Smith (2014): <http://www.open.ac.uk/researchcentres/osrc/files/osrc/NARRATIVES.pdf>

## Public skepticism of science

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Having only participated in the second day of the meeting, and not coming from a background in one of the main fields under discussion, these observations are perhaps somewhat random, but I hope that they may contribute to the discussion.

When engaging with people about scientific claims, it helps if we can provide evidence that speaks to their personal experience and is hard for them to deny through their lack of familiarity with it. For example, the length of the English first-class cricket season—which until as recently as the early 1990s used to start around 20 April and end with matches dominated by dew in the first week of September, and now extends two weeks longer either side—is both solid and entertaining evidence for climate change, and also happens to intersect with the interests of a certain percentage of the “climate skeptic” demographic in the UK (see also below). Similarly, there is evidence that farmers—a group not traditionally known for their readiness to embrace “trendy” political causes—are planting their crops earlier<sup>2</sup> and growing a wider range of crops. Emphasising the positive sides of climate change, rather than presenting it as yet another way for left-of-centre “political types” to tell ordinary people that society is going to hell unless taxes go up, may have better chances of gaining acceptance for the overall proposition that the climate is in fact changing.

On a related note, I think that we (science in general, and social scientists in particular) need to engage more with the lived experiences of the members of the groups who are attacking any particular scientific conclusion, at least to the extent that these are not directly in the pay of cynical interests from industry. In many cases, these groups are untypical of the general population; for example, people who get angry about what they see as fake claims of climate change tend (from my entirely anecdotal observation, true) to skew heavily towards being white, middle-aged, middle-class, and male, and analogous—although demographically different—patterns might well be found among anti-vaccine or anti-GMO activists. We have a tendency to neglect or reject the concerns of these people, especially when these groups do not look like us, or remind us of people we don't want to be like. But we need to acknowledge the legitimacy *to them* of their version of reality, firstly because it's the human/humane thing to do even for those who are not demonstrably suffering from a mental health issue, and secondly because we're not going to win by telling them that they're idiots.

It was suggested at one point that we should stop talking about “evidence” and “hypotheses”, and instead talk about “facts”. Much as I regret the way in which we can be hamstrung by having to say things like “there's no evidence for that” rather than “we know that's crap”, I'm wary of maybe handing a rhetorical gift to the opposition in this regard. If I can go into “devil's advocate mode”: Who gets to define “facts”? Are you some kind of Stalinist? So I think this needs to be considered carefully.

During the discussion, I mentioned the idea of having a “science police” force (not as an immediately ready-to-go policy proposal; more to highlight the consequences of the absence of a first-line investigation “squad” compared to the situation in wider society). During

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<sup>2</sup> <http://www.theguardian.com/lifeandstyle/2009/oct/11/climate-change-britain-crops>

David Lewis's description of Institutional Scientific Misconduct, the phrase "science secret police" sprung to mind, and clearly that's not what we want. But I do have the feeling that some people behave the way they do because they know it's very unlikely that anyone is watching, or ever going to scrutinise what they did. In the absence of systematic checks of materials by journals, we need some way to inquire into published research without this being considered an implicit accusation of misconduct towards the author(s) on the part of the person raising the doubts. This isn't how science has worked up till now, but I think it's worthwhile to question whether we can continue on the basis of past assumptions of "gentlemanly" [sic] behaviour when the stakes are so high.

Finally, on the subject of perverse incentives to do bad science, I didn't see "personal financial enrichment" in the list (alongside professional status and bigger grants). Yet, there are a number of people making sizeable personal gains from their superstar status, at least in psychology. When you charge 5-figure sums to give a keynote about how people can live longer if they follow your discoveries, how hard are you going to be trying, Feynman-style, to prove yourself wrong? I'm not sure what can be done here, since our society loves celebrities so much, but I think it's worth mentioning as a separate dimension to the general conflict of interest problem.

## My Story

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Fredrickson and Losada (2005) claimed they discovered a precise mathematical tipping point (i.e., a ratio of positivity to negativity of 2.9013) separating flourishing from languishing in various human endeavors. This tipping point, called the *Critical Positivity Ratio* (CPR), was derived from the Lorenz equations used in fluid dynamics, and was claimed to be a universal (i.e., across all cultures and epochs) constant. This was seen by many in psychology as a major discovery and spawned hundreds of scholarly citations, nearly a million google hits, and untold numbers of applications (e.g., as one of the bases for the largest social science research project ever [Comprehensive Soldier Fitness in the United States Army], and widely used within schools, mental health systems, organizations, etc.).

A graduate student, Nick Brown, sought my help in debunking this claim, as he was taught the CPR as gospel but suspected it was flawed. As the last in a long list of psychologists he had contacted, I agreed to help him, as I had read the Fredrickson and Losada (2005) paper when it first came out and, although I thought it badly flawed, I did not know how to challenge its underlying mathematics. Brown and I invited Alan Sokal, renowned mathematician and notorious debunker of other bad science, and the three of us demonstrated that the mathematics behind the CPR was indeed flawed (Brown, Sokal, & Friedman, 2013). In psychology, claims and counter claims can result in endless debate, but fortunately in math things can be shown to be wrong.

But then came the politics, as the journal in which Fredrickson and Losada (2005) initially published their paper refused to peer-review our refutation, as it was dismissed as an overly long and very late comment (i.e., the journal had firm rules that comments could come no later than 60 days after a paper had been published and be no more than 1000 words). This resulted in a series of appeals to the journal, finally leading to the publication of our paper (Brown, Sokal, & Friedman, 2013) debunking the CPR. Consequently, Fredrickson and Losada (2013) withdrew the math in their 2005 paper, but Fredrickson (2013) continued to defend the claim that a tipping point was empirically evident. The journal initially did not allow for this claim to be answered but, after further arduous appeals, finally Brown, Sokal, and Friedman (2014a) published their refutation showing that there is no empirical evidence whatsoever for that claimed tipping point. Despite this, Losada continues to defend the math behind the CPR—even though he withdrew it, while Fredrickson continues to insist there is a "tipping point" somewhere.

Two other highly prominent recent papers by Fredrickson were subsequently examined and found to also be flawed (Heathers, Brown, Coyne, & Friedman, 2015; Brown, MacDonald, Samanta, Friedman, & Coyne, 2014), but publishing these refutation papers again required similarly arduous appeals. Overall, despite that there is a widely acknowledged replication crisis within psychology, this saga demonstrates considerable resistance involving scientific self-correction (Brown, Sokal, & Friedman, 2014b).

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## Public inclusion in science: blurring the exogenous and endogenous

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Harassment is unacceptable in all aspects of public life, including science. At the meeting, we heard a range of stories from working scientists about unacceptable harassment they had suffered, either from private individuals or corporate lobbies. These ‘exogenous threats’ present a significant problem. However, they also provide a puzzle. If science is to be integral to, not separate from, society, how can we ensure that the public are properly included?

Public inclusion in science is essential, both for science and democracy. Public discontent with science is often (but not always) a result of exclusion from debate about *the direction of scientific research* (GM crops being the most high profile example). In this way, greater public inclusion is a means of moderation between diverging views in society, and can help to reduce (but probably not remove) sources of friction. These diverging views are likely to result in discomfort, at the least, for scientists, public, or both. In very serious cases, such as the one Esther Crawley presented, a ‘code of conduct’ appears to be very helpful in establishing common boundaries within which actors agree to operate.

However, public inclusion is not only about the direction of research, but also *the opportunity to scrutinise and replicate scientific research*. This is where the boundary between exogenous and endogenous threats becomes blurred. Requests from the public may not be couched in the same kind of language as those from academics. They may even be rude to a point that one could categorise them as an exogenous threat. Yet such requests may also be entirely justified. (Even between academics who should be ‘speaking the same language’, there are overheated accusations of [‘cyber bullying’](#) in response to critical online commentary.) The replication crisis in psychology suggests that the presumption regarding data requests should be for full, unconditional disclosure, accompanied by appropriate caveats re anonymity.

Crucially, it is very hard for members of the public to know their points of entry into the academic process. Data requests are sometimes subjected to ‘pass bucking’ between academics, journals and universities. The rise of open science and the forthcoming Agenda for Open Research will hopefully make such behaviour a thing of the past, but we are a long way off that point at the moment.

Finally, a quote from [David Demeritt](#) from 2001 that neatly encapsulates the endogenous/exogenous problem in climate change, but could apply to other areas of science:

“The proper response to public doubts is not to increase the public’s technical knowledge about and therefore belief in the scientific facts of global warming. Rather, it should be to increase public understanding of and therefore trust in the social process through which those facts are scientifically determined. Science does not offer the final word, and its public authority should not be based on the myth that it does, because such an understanding of science ignores the ongoing process of organized skepticism that is, in fact, the secret of its epistemic success. Instead scientific knowledge should be presented more conditionally as the best that we can do for the moment. Though perhaps less authoritative, such a reflexive

understanding of science in the making provides an answer to the climate skeptics and their attempts to refute global warming as merely a social construction.”

## Rebuilding the Behavioural Sciences: Research Methods for the Next Decade

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It has been well-documented in recent years (e.g., Ioannidis et al., 2014) that imperfect practices across the science spectrum are undermining the robustness and credibility of published research findings. Studies with positive results are more likely to be published than negative ones (publication bias), research is often underpowered for the effects it is studying, and a range of questionable research practices ('*p*-hacking') can bias reported outcomes. Large-scale replication projects in biomedicine, psychology, and elsewhere are painting a depressing picture about the reproducibility of research findings. For example, fewer than 40% of a quasi-random sample of studies in psychology were successfully replicated (Nosek et al., 2015).

These factors, if ignored, could significantly undermine public trust in science. Scientists who dedicate considerable amounts of time and effort to collecting data tend to feel a sense of ownership of their research. Yet as recipients of public funding, scientists are under obligation to ensure that their data are exploited and evaluated thoroughly (OECD *Principles and Guidelines for Access to Research Data from Public Funding*, 2007). The transformation of the publishing model for scientific research towards Open Access recognizes this obligation. But as yet there has been little recognition that the same principle applies to the underlying research itself. Even amongst scientists, failure to comply with data sharing policies is substantial. Some estimates put compliance rates at only around 25%, and suggest that data-sharing is lowest for weak research findings (Wicherts, Bakker, & Molenaar, 2011).

Solutions will need to be multi-faceted, including tightening rules on reporting conflicts of interest, greater openness regarding data and methods, and changes in the incentive structures under which scientists operate. Amongst the remedies for these problems are several mechanisms which are already gaining ground. In medical research, pre-registration of randomised control trials has been the norm for some time and this is now gaining ground elsewhere in the social and behavioural sciences. Pre-registration ensures that the reported results have not been 'cherry-picked' and represent a true confirmatory test of the empirical hypothesis. It does not preclude exploratory analysis, so long as these are clearly indicated.

Meta-analysis represents another potent tool for countering some of the biasing factors known to be present in scientific research. Important findings which are supported meta-analytically are unlikely to be false positives resulting from *p*-hacking. Meta-analysis is also a powerful tool for documenting weaknesses in research fields. For instance, it is estimated that amongst meta-analyses that tested for publication via funnel plot asymmetry, it was present in around 20-40% (Ferguson & Brannick, 2012).

Universities and other research institutions employing scientists need to modify the incentives under which they operate to ensure that careful and trustworthy research is suitably rewarded, especially when it meets the gold standard for open data.

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