PEER REVIEW

Why skepticism is essential

Donna Laframboise

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Foreword

By Christopher Essex and Matt Ridley

The Intergovernmental Panel on Climate Change has repeatedly and falsely claimed that it depends entirely on peer-reviewed papers. Donna Laframboise used volunteers to check this claim and found that a significant part of the references in the fourth assessment report of the IPCC were to 'grey literature' – that is, press releases, 'reports' from pressure groups and the like, which are not remotely the normal peer reviewed scientific literature.

Yet even if all the citations used by the IPCC were peer-reviewed, this would not mean they were infallible. Peer review is not, never was, and never can be a general protection against prejudice, error, or misconception about scientific matters. That it seems otherwise to some people is a misapprehension on their part, reflecting widespread myths about the reality of human investigations into the natural world.

It is startling for non-scientists who actually visit the sausage factory of science for the first time. There, peer review proves to be an often biased, prejudicial, and perfunctory process contrary in every respect to popular expectations about science. But scientists know that no increased regulation or standards can ever improve things, because there are no higher authorities to appeal to in a domain of human endeavour where no one knows or ever knew the answers – hence the name 'peer review' and not 'expert correction'.

Donna Laframboise observes that, 'There is a reason publishing insiders are among peer review's most derisive critics. They know it's mostly just a game. Everyone is pretending that all is well despite a mountain of evidence to the contrary.' Most scientists grudgingly tolerate peer review because they cannot think of anything better. Experienced ones do not expect much from it, even if they must play along to succeed given modern customs (until about the mid-20th century it hardly existed).

Most scientists cringe when they hear other scientists claim that because their work is peer-reviewed, they do not have to respond to criticisms, even those from qualified colleagues, whether peer-reviewed or not. Some surely do make such claims: '…many academics insist that the research they present to the world has been fully vetted. Indeed, they often behave as though it meets a standard unrivalled elsewhere', observes Laframboise.

Furthermore, those same scientists retreat to the truth about the state of human knowledge of Nature when facts go against their claims. She points out that: 'On the other hand, they take no responsibility when information they've produced turns out to be mistaken. In such cases everyone is then reminded that scholarly publishing is

really just an exchange of ideas.' Few competent scientists regard current scientific thinking as anything more than provisional. It is always fully open to challenge.

Peer-review is also abused as a form of gatekeeping to defend orthodox ideas from challenge, as Laframboise says: 'Alternative schools of thought are more likely to encounter scorn than a fair hearing, and the secretive nature of peer review provides ample cover for intolerance and tribalism…It places unconventional thinkers at the mercy of their more conventional colleagues. Indeed, this approach seems designed to extinguish – rather than nurture – the bold, original thinking that leads to scientific breakthroughs.' Many unorthodox ideas prove to be wrong, but they are the lifeblood of scientific advance. They challenge our orthodoxies, either sharpening them or overthrowing them. Thus the notion of challenging the orthodox is accepted in science by necessity, even if grudgingly.

Gatekeeping against the unorthodox is not remotely a new problem. Oracular mediocrities down the centuries have doggedly resisted human advances in knowledge from Galileo to Semmelweis to Einstein, and thousands of other cases that only the most learned science historians will ever know. Spectacular scandals come and go, but science is in the end a long game of the generations, not something played out in news cycles. So why then has the public debate about the perfunctory, crony, gate-keeping aspects of peer review grown in volume in the media now? Partly it is because science has become a 'bigger' and more centralized endeavor, with massive budgets invested in conventional wisdom, and more politicians involved in pushing certain conclusions. How else can one comprehend the term 'orchestrated' used by one founder of the IPCC to describe how scientific opinion was designed to be treated for the use of policymakers?

It is clear that people who have never studied the history of science, or have never been on the unfashionable side of a scientific debate are in for a shock upon encountering this messy and sordid reality for the first time. Not least in for a shock is the media, which has been busy identifying heartbreaking science scandals in medicine, social science, neuroscience, and economics. But curiously they offer none from the subject of climate, despite it being one of the most policy driven and lavishly funded branches of science today.

Is this because there are few examples of bad practice, irreproducibility, retraction, pal review and gatekeeping in climatology? Far from it. The Climategate emails of 2009 revealed gatekeeping at its most blatant. Who can forget Phil Jones writing to Michael Mann on 8 July 2004 'can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow – even if we have to redefine what the peer-review literature is!' Or Steve McIntyre and Ross McKitrick struggling to publish (leading to a US Congressional hearing, no less) their comprehensive demolition of the statistical errors and data-selection issues in the infamous 'hockey stick' paper? Or Richard Tol's exposure of the practices employed in the Cook et al '97%

consensus' paper? Again and again, peer-reviewed climate papers have fallen apart under post-publication scrutiny from the likes of McIntyre, Willis Eschenbach, Donna Laframboise, Judith Curry and Nic Lewis. And these do not even touch on the challenge of independently reproducing climate model output without the machinery and resources necessary to do so, as Laframboise rightly observes in the following paper.

Indeed, the field of climate science could supply a rich harvest of examples of this crisis of scientific credibility all on its own. Yet it is the scandal that dare not speak its name. The discussions of the crisis in peer review in *Nature*, *Science*, the *Economist* and elsewhere studiously ignore any examples from climate science. Why is this? It is an article of faith among certain scientists and science journalists that because climate scepticism is also a position supported by those on the right of politics, so nobody in science must give fodder to the sceptics.

This is nothing less than the modern manifestation of gatekeeping continuing its ancient legacy, driven by sheer ignorance and self-delusion, to keep the forces that actually advance science away from the door. Scientific research stretches human faculties to their limits, and it is at such limits where human frailties become most prominent.

Humans are fallible. That is one of the greatest lessons from the history of science. The message to be taken from these heartbreaking scientific scandals and absurdities is not one of chagrin and a temptation to adopt cynicism. The true authors of such scandals are the laymen, academics, journalists, and policymakers who do not give a fair hearing to the many highly trained scientists motivated by alternative views who would put such dubious claims to the test. A pervasive uneducated appeal to science as a monolithic incomprehensible authority, assessed only in terms of moral purity rather than factual accuracy, has made such a fair hearing nearly impossible, and done great harm to science and us all.

Christopher Essex
Matt Ridley
September 2016

About the author

Donna Laframboise is a Canadian investigative journalist. She is the author of a 2011 exposé of the Intergovernmental Panel on Climate Change, *The Delinquent Teenager Who Was Mistaken for the World's Top Climate Expert*. Her 2013 book, *Into the Dustbin: Rajendra Pachauri, the Climate Report & the Nobel Peace Prize*, examines the leadership of that organization. She is a former columnist and editorial board member of the *National Post*, and a former vice president of the Canadian Civil Liberties Association. She has never belonged to any political party, and has voted across the political spectrum. She currently blogs at BigPicNews.com.

Summary

Prior to the 2009 Copenhagen climate summit, Peter Doherty, winner of the Nobel Prize in medicine, defended the Intergovernmental Panel on Climate Change (IPCC) from its critics. The IPCC involves hundreds of scientists and 'draws its evidence exclusively from peer-reviewed, published scientific literature', he wrote.[1] Around the same time, the IPCC chairman was asked if an Indian environment ministry report might alter the IPCC's pessimistic view of Himalayan glaciers. The 'IPCC studies only peer-review science', Rajendra Pachauri replied dismissively. Until the report's data appears in 'a decent credible publication', he said, 'we can just throw it into the dustbin'.[2]

Peer-reviewed research is reliable, so the reasoning goes. Non-peer-reviewed research is not. The IPCC makes exclusive use of the former, therefore its conclusions can be trusted. This argument has long been used to deflect criticism and to repel contrary climate perspectives.

But behind it lies a dubious assumption: that academic publications are a sound foundation on which to base real-world decisions. In fact, science is currently in the grip of a 'reproducibility crisis' so severe that the editor of a prominent journal has declared that 'much of the scientific literature, perhaps half, may simply be untrue'.[3] Media coverage declaring that 'science is broken' has become commonplace.[4]

Part 1 of this report demonstrates that a journal's decision to publish a paper provides no assurance that its conclusions are sound. Large swathes of peer-reviewed work contain errors. Fraudulent research makes it past gatekeepers at even the most prestigious journals. And while science is supposed to be self-correcting, the process by which this occurs is haphazard and byzantine.

A policy cannot be considered evidence-based if the evidence on which it depends was never independently verified. Peer review does not perform that function. News from the worlds of astrobiology, ecology, economics, chemistry, computer science, management studies, medicine, neuroscience, psychology, and physics all tell the same tale: 'peer-reviewed' does not equal 'policy-ready.'

Part 2 of this report invites us to re-examine what we think we know about the climate. While good scientists have always understood that peer review doesn't certify accuracy, IPCC officials – supported by politicians, activists, and journalists – think global climate decision-making should rest on this shaky foundation.

If half of all peer-reviewed research 'may simply be untrue', half of all climate research may also be untrue. The policy implications of this idea are immense.

1 The reproducibility crisis

The US National Institutes of Health (NIH) is the world's largest funder of medical research. In early 2014 it issued a statement that began by citing a lengthy *Economist* news story titled 'Trouble at the lab'. According to the news story, tens of billions of dollars are spent on medical research each year, yet an unidentified NIH official 'reckons, despairingly, that researchers would find it hard to reproduce at least three-quarters' of published medical findings.[5]

By calling attention to the news story without disputing the above statistic, Francis Collins, the director of the NIH, all but shouted from the rooftops that 75% of medical research is unreliable – a shocking state of affairs. In his words: 'the checks and balances that once ensured scientific fidelity' have collapsed.[6]

UK officials have begun to acknowledge the existence of a crisis.[7] A US National Academy of Sciences workshop has addressed ways of combatting it[8] and that country's National Science Foundation has restated a fundamental principle: 'If a scientific finding cannot be independently verified, then it cannot be regarded as an empirical fact'.[9] The elite scientific journal, *Nature*, has announced new measures aimed at 'reducing our irreproducibility'.[10] The editor-in-chief of the equally prestigious *Lancet* declares that 'science has taken a turn toward darkness' and that 'much of the scientific literature, perhaps half, may simply be untrue'.[11]

Computer scientists, alarmed at how little of the research in their own field is reproducible, refer to a credibility crisis.[12] The field of psychology is similarly grappling with high rates of irreproducibility.[13] An entire 2012 issue of *Perspectives in Psychological Science* explored this problem, with one paper referring to 'the myth of self-correction in science' and another titled 'Why science is not necessarily self-correcting'.[14]

In 2007, Daniel Hamermesh remarked that economists 'treat replication the way teenagers treat chastity – as an ideal to be professed but not to be practised'.[15] Two years later, a report concluded that 'economics journals do not ensure replicability, even when their own policies require it'. Rather, 'the evidence strongly suggests that most results published in economics journals are not independently' verifiable.[16] Little improvement was observed by the authors of a 2015 paper prepared for the Board of Governors of the US Federal Reserve. Since 'less than half of the papers in our sample' can be reproduced even with the assistance of the original authors, they wrote, 'we assert that economics research is usually not replicable'.[17]

The idea that research published in a scholarly journal is as likely to be wrong as it is to be right is difficult to absorb. But this is old news to venture capital investors. Bruce Booth of Atlas Ventures holds a PhD in molecular immunology from Oxford University. The 'unspoken rule', he wrote in early 2011, 'is that at least 50% of the studies published even in top tier academic journals…can't be repeated with the same conclusions by an industrial lab'.[18]

That same year, employees of Germany's Bayer Healthcare reported that attempts to reproduce the findings of 67 studies involving promising drugs had resulted in a 75% failure rate. The investigators were surprised that research published in prestigious journals was no more reliable than research published in journals lower down the hierarchy.[19] An equally alarming report appeared in *Nature* in 2012. Amgen, an American pharmaceutical company, had attempted to verify the findings of 53 landmark papers connected to cancer research. It was unable to do so in 47 cases (89%).[20]

A world in which nine out of ten cancer studies aren't worth the paper they're printed on is a world that needs to look more closely at the way research currently gets vetted. The academic publishing industry makes use of a mechanism known as pre-publication peer review. This, we are told, is the watchdog that prevents poor-quality work from making its way into the scholarly record.

When a committee of the UK House of Commons examined peer review in 2011, Edmund Lamb, editor of the *Annals of Clinical Biochemistry*, declared that 'the peer review process protects the scientific community and the public from erroneous science'. The UK Academy of Social Sciences similarly asserted that peer review 'provides a guarantee for the public of the validity and scientific warrant of knowledge produced with public funds and placed in the public domain'.[21]

The mechanics of the process are straightforward. Because academic journals often have room for less than 10% of the research papers submitted to them, many thousands are summarily rejected.[22] Those that survive this first hurdle are typically sent to two or three individuals external to the journal who are thought to possess relevant scholarly expertise. These individuals, known as referees or reviewers, can recommend that a paper be published as is, rejected outright, or amended in order to make it publication-worthy. The decision rests with journal officials, and the identities of referees are usually kept secret from the authors whose research they evaluate, as well as from the public.

Context and history

It is difficult to exaggerate the central role peer review plays in modern academic life. Since at least the 1970s, educational administrators and funding bodies have placed inordinate emphasis on how many papers a researcher publishes, particularly in top journals.[23] Individual career advancement, university department funding, and academic prestige are all closely linked to this single metric.[24] The House of Commons committee was advised that a publishing 'arms race' is currently underway throughout the academy,[25] and that professional 'careers are built and destroyed on these numbers'.[26]

Peer review is, therefore, the mechanism by which otherwise invisible research gets transformed into academic gold. Sandra Goldbeck-Wood has observed that it 'confers legitimacy' on academics themselves as well as on their work, and is 'sur-

rounded by an almost religious mystique".[27] A *New York Times* headline has referred to 'the sacred rite of peer review'.[28] Richard Smith, a former editor-in-chief of the *British Medical Journal* (BMJ) and an outspoken critic, acknowledges that peer-reviewed research is considered 'in some sense blessed'.[29]

Given all of the above, it's disconcerting to discover that nothing more than a metaphorical sniff test actually takes place. A 2008 survey found that referees typically spend a total of five hours reading a paper, preparing written feedback, and exchanging e-mail with journal personnel.[30] Some reviews are a single paragraph in length.[31] Others appear to be little more than a posterior-covering formality.[32] There is no expectation on anyone's part that referees are conducting an audit. They typically don't examine raw data, computer codes, or even verify that cited sources say what the paper claims they do. If one receives what one pays for, it's worth noting that scholarly publishing's vaunted vetting process relies almost entirely on free, volunteer labour.

The Royal Society, the world's oldest scientific academy, told the Commons committee that peer review has 'stood the test of time' and that all of its own publishing decisions have been made this way since 1660.[33] But peer review didn't become widespread until centuries later. As Melinda Baldwin, the author of a book about *Nature*, has observed, 'many of the most influential texts in the history of science were never put through the peer review process, including Isaac Newton's 1687 *Principia Mathematica*, Albert Einstein's 1905 paper on relativity, and James Watson and Francis Crick's 1953 *Nature* paper on the structure of DNA'. In fact, she says, '*Nature* published some papers without peer review up until 1973'.[34]

The fragmentary historical record indicates that scholarly journals began making use of external referees at different times, for different reasons, and in a variety of configurations. The turning point appears to have been World War II, after which the number of people involved in academic life ballooned. As the supply of manuscripts rose and research became more specialized, most journals began using outside experts to help them evaluate submissions.[35] That said, as late as 1965 a survey of American humanities journals identified 16 publications in which editors continued to rely solely on their own judgment, and another 50 in which the editor normally sought input from only one additional person.[36] Institutionalized external peer review involving multiple referees is, therefore, a recent phenomenon.

Because of this disorderly history, peer review remains highly non-standardized. Michael Callaham compares the current situation to a bygone era in which no licence was required to practice medicine. 'Anyone can produce a journal and use any standards they see fit', he says.[37] After organizing four international peer review conferences between 1989 and 2001, Drummond Rennie concludes that 'the term ''peer review'' still seems to mean a great many things to different journal editors, and practices still seem to vary *widely* between journals' (italics added).[38]

The Commons committee was repeatedly told that peer review is a quality assurance mechanism.[39] But industry-wide definitions, standards, and best practices are wholly absent. No certification or monitoring by independent third parties occurs, as is common in other quality assurance contexts. Rather, the academy appears to believe that every ad hoc, idiosyncratic process to which the peer review label has been affixed produces equal benefits. But these benefits are difficult to document. An extensive body of research finds scant evidence that peer review accomplishes much at all, other than modestly improving the clarity of some manuscripts.[40] On the other hand, a great deal of duly peer-reviewed scholarship has identified numerous deficiencies.

Smith, the BMJ's former editor, describes peer review as a roulette wheel, a lottery, and a black box.[41] As early as 1982, researchers demonstrated its random, arbitrary nature by resubmitting papers to journals that had already published them. Cosmetic changes (including the use of fictitious author and institution names) were made to 12 papers that had appeared in a dozen highly regarded psychology journals during the previous 18–32 months. The duplication was noticed in three instances, but the remaining nine papers underwent review by two referees each. The 16 referees (89%) who recommended rejection didn't cite lack of originality as a concern. Instead, the manuscripts 'were rejected primarily for reasons of methodology and statistical treatment'. Only one of the nine papers was deemed worthy of seeing the light of day the second time it was examined by reviewers at the same journal.[42]

Bias and conflicting interests

Peer review is prone to all manner of bias. After an ecology journal began removing author names prior to forwarding manuscripts to reviewers, the number of published papers whose lead author was female rose.[43] Conversely, a 2013 study found that female referees were significantly less likely to recommend publication of male-authored papers than female-authored papers.[44] When a researcher's gender, nationality, prominence, or institutional affiliation influences publishing decisions, scientific integrity suffers.

In the publish-or-perish environment in which they spend their professional lives, modern scientists are hugely incentivised to pursue flashy, superficially tested theories rather than triple-checked, non-glamorous ones. Referees are routinely asked to pass judgment on a paper's novelty and originality, and journals display a well-documented bias against publishing underwhelming (in other words, negative), results.[45] Keen to secure publication in high-prestige journals, scientists are practically invited to grasp at straws and to exaggerate borderline findings. Eager to maintain their industry rankings and to attract mainstream media attention, scholarly journals frequently select papers based on criteria other than scientific virtue.

A 2010 *New Yorker* article explores the significant and distorting role that confirmatory bias can play. Titled 'The truth wears off', it describes how early, persuasive scientific results often dissipate over time. Energized by a new idea, researchers start finding proof of it everywhere. As the years pass, however, the strength of the evidence declines. This phenomenon applies to topics as diverse as the mating rituals of barn swallows and the effectiveness of antipsychotic drugs. Being mere mortals, scientists aren't impervious to 'fleeting fads'. When a community of researchers succumbs to groupthink (the *New Yorker* calls it 'a collective illusion nurtured by strong a-priori beliefs') referees – who are drawn from that same community – provide the public with little protection against erroneous scholarship.[46]

Outsiders naively assume that scientists are open-minded intellectual explorers, but back in 1977 Michael Mahoney demonstrated that identical manuscripts suffer 'very different fates' depending on whether their conclusions confirm or challenge a referee's own theoretical perspective.[47] Facts are supposed to prevail over dogma but scientists, alas, are only human. Alternative schools of thought are more likely to encounter scorn than a fair hearing, and the secretive nature of peer review provides ample cover for intolerance and tribalism.

Peer review's Achilles heel

Three decades ago Herbert Ayres,[48] then president of the Institute of Management Sciences, identified peer review's Achilles heel: it places unconventional thinkers at the mercy of their more conventional colleagues. Indeed, this approach seems designed to extinguish – rather than nurture – the bold, original thinking that leads to scientific breakthroughs. Most scientists, and therefore most referees, says Ayres, 'are not groundbreakers'. When asked to review research, they represent 'the establishment'. He eloquently invites us to consider matters from the perspective of a hypothetical 'Professor Zee', who has tenure, is a recognized authority in his field, and is juggling numerous professional responsibilities:

> In the 31 years since he was a freshman, he has invested 9 solid man-years of work mastering his specialty. If its structure is basically disrupted, he will need up to a solid man-year to rework it. Where is he going to get a man-year? Nowhere, unless he turns his life inside out. As a referee of a paper that threatens to disrupt his life, he is in a conflict-of-interest position, pure and simple. Unless we're convinced that he, we, and all our friends who referee have integrity in the upper fifth percentile of those who have so far qualified for sainthood, it is beyond naive to believe that censorship does not occur. It need not be entirely conscious…Zee is not a bad buy. Zee is a good guy…He never said he was a saint; it is we who are asking him to be.[49]

Many Nobel laureates received a thumbs-down verdict from journals when submitting the very work that would later win them accolades. Juan Miguel Campanario

reports[50] that both *Nature* and *Science* rejected Kary Mullis' description of a new approach to analyzing DNA. Eight years later, it won him a Nobel prize in chemistry. In another instance, a referee dismissed as 'not interesting enough' work that led directly to a physics Nobel.[51] A 1955 letter from the editor of *The Journal of Clinical Investigation* advised that its peer reviewers 'have been particularly emphatic in rejecting' discoveries that would earn Rosalind Yalow the 1977 Nobel prize in medicine.[52]

No defence against fraud

The same peer review process that fails to recognize Nobel-quality work when it sees it offers the public no protection against fraud. This is true even at top-tier journals where, according to the Royal Society, peer review is the most exacting.[53] Smith has observed that having one's paper published 'in a major journal like *Nature*' is equivalent to winning 'the jackpot'.[54] In early 2003, *Nature* formally retracted seven papers authored by Jan Hendrik Schön.[55] The condensed matter physicist said he'd discovered a new form of superconductor, amongst other things, but had actually faked his data. The similarly high-status *Science* retracted eight of Schön's papers.[56] Between them, three other journals withdrew an additional twelve. In the Schön case alone, therefore, fraudulent research was published on 27 occasions by five separate scientific journals, including *Nature* and Science.[57]

In 2009, the editor-in-chief of the highly-ranked *Anesthesia & Analgesia* journal released a list of 21 papers that a third-party investigation had determined were based on fabricated data. Scott Reuben, a prominent professor of anesthesiology, had foiled the peer review process of that journal ten times over a twelve-year period. Eleven more of his fraudulent papers had been published by eight additional journals.[58]

In 2011, *Science* retracted highly-publicized research by the social psychologist Diederik Stapel that claimed to have found a link between litter and racism but was actually fabricated whole cloth.[59] A lengthy *New York Times Magazine* profile, entitled 'The mind of a con man', describes Stapel as 'an academic star in the Netherlands and abroad'.[60] A 2012 report written by investigators at three Dutch universities concluded that Stapel had published a total of 55 fraudulent papers.[61]

His work has appeared 'in nearly all the respected international journals in his field', yet the report said it was 'extremely rare' for his findings 'to be subjected to serious doubt, even…where the fraud was blatant'.[62] These investigators consider it 'almost inconceivable' that referees at these journals failed to notice 'all the impossibilities, peculiarities and sloppiness' they themselves unearthed. 'Time and again', they say, the 'journals accepted that Mr Stapel's hypotheses had been confirmed in a single experiment, with extremely large effect sizes'.[63] Overall, Stapel's fraudulent research got past the peer reviewers of almost two dozen journals.

The Dutch investigators assert that the Faculty of Psychology at the University of Amsterdam failed to enforce scientific standards:

> It was easy for researchers to go their own way. Nobody looked over their shoulder…there was no culture in which scientific integrity was given a high priority through explicit attention, detailed monitoring, an implicit exemplary function of senior staff and a collective responsibility within research groups, where the members correct each other and keep each other alert.[64]

Instead, say these investigators, the academic culture in which Stapel rose to prominence was one 'of careless, selective and uncritical handling of research and data. The observed flaws were not minor…but violations of fundamental rules of proper scientific research'.[65]

These investigators also say that journal referees sometimes 'encouraged irregular practices' by urging the omission of untidy details in Stapel's manuscripts in the interests 'of telling an interesting, elegant, concise and compelling story'.[66] The authors who reported that 89% of cancer studies couldn't be reproduced noticed the same thing. Data originally included in manuscripts is 'removed during the peer review and publication process' they say, because editors and referees 'often look for a scientific finding that is simple, clear and complete'.[67]

That peer review may, in fact, be undermining reproducibility is further supported by a report published in 2003. Drawing on 30 years' experience as a journal editor, Arthur Bedeian invited nearly 200 lead authors who'd recently had papers published in premier management journals to complete a survey about the peer review process those papers had undergone. A majority (74%) felt 'the revisions they were required to make in their manuscripts were beneficial enough to justify the additional labor and delay in publication'. Nevertheless, 24% reported that, during the process of making their work acceptable to the referees and editors involved, 'they had actually made changes they felt were wrong'.[68] Academic desperation to be published may be introducing error into a quarter of the research in top-tier journals.

Flawed analyses

Another deficiency that peer review provides no shield against is the improper use of statistics. In 2011, an examination of neuroscience research papers published in 'five top-ranking journals' found that half had applied a statistical procedure incorrectly.[69] Why, wondered one news account, are journals 'not properly vetting such papers before publishing them?'[70]

In 2014, third-party academics alerted the *Obesity Facts* journal to significant flaws in a paper it had published. While offering statistical guidance to other researchers, the authors had misstated key concepts. A statistician was consulted and he confirmed these shortcomings, but this flawed statistical advice remains part of the official scientific record. The journal subsequently published an editorial that declares 'it is each author's responsibility to make sure that statistical procedures are correctly used' in the work they submit since 'it cannot be expected that all of our reviewers,

in addition to their expertise in various aspects of obesity research, are designated experts for advanced statistical procedures'.[71] In other words, *caveat emptor*. Just because a paper passed peer review doesn't mean the referees were intellectually equipped to evaluate it.

The Economist news story mentioned at the beginning of this report sums up the current statistical quagmire:

> 'Some scientists use inappropriate techniques because those are the ones they feel comfortable with; others latch on to new ones without understanding their subtleties. Some just rely on the methods built into their software, even if they don't understand them.[72]

In 2014, *Science* announced measures to provide deeper scrutiny of statistical claims in the research it publishes.[73] John Ioannidis, the author of a seminal 2005 paper asserting that most published research findings are false,[74] called this announcement 'long overdue'. In his opinion, statistical review has become more important than traditional peer review for a 'majority of scientific papers'.[75]

In February 2016, the American Statistical Association issued a formal statement clarifying 'the proper use and interpretation' of a statistical measure 'commonly misused' in published scientific research.[76] The degree to which efforts such as these will improve the quality of the peer review that is applied to the 1.5 million scholarly articles published each year[77] remains unclear.[78]

Wobbly standards

As mentioned earlier, educational administrators and funding agencies assess academic performance according to certain metrics. Despite the extravagant shortcomings associated with the peer-review process, they continue to equate publication in a peer-reviewed venue with scientific quality. The folly of this position is highlighted by a headline that appeared in *Nature* in 2014. 'Publishers withdraw more than 120 gibberish papers', it announced.[79] In contrast to other fields, computer scientists publish their work primarily in conference proceedings rather than journals.[80] These are normally fully refereed publications but here, too, peer review is a poor watchdog. Between 2008 and 2013, papers containing computer-generated nonsense made their way into 30 conference proceedings issued by reputable scholarly publishers. When contacted, one publisher was adamant that the papers had, indeed, been peer reviewed.

There is a reason publishing insiders are among peer review's most derisive critics. They know it's mostly just a game. Everyone is pretending that all is well despite a mountain of evidence to the contrary. Smith tells a story about Robbie Fox, 'the great 20th century editor of *The Lancet*', who joked that he 'had a system of throwing a pile of papers down the stairs and publishing those that reached the bottom'. Adds Smith,

> When I was editor of the BMJ I was challenged by two of the cleverest researchers in Britain to publish an issue of the journal comprised only of papers that had failed peer review and see if anybody noticed. I wrote back 'How do you know I haven't already done it?'[81]

Smith's book, *The Trouble With Medical Journals*, devotes an entire chapter to the subject of editorial misconduct.[82] A BMJ blog post he wrote in 2008 begins with this sentence: 'In what has been called the age of accountability, editors have continued to be as unaccountable as kings'. The upshot of the melodrama he then relates is that two academics who attempted to challenge a 2003 paper published in the *Journal of Health Economics*, felt that the manner in which their own paper was handled violated 'almost every ethical standard established for editors'. Nevertheless, they were advised that no mechanism existed within academic publishing to complain about such behaviour. In the wake of this incident, writes Smith, the owner of the journal – scholarly publishing giant Elsevier – 'has now signed up all of its journals to be members of the Committee on Publication Ethics (COPE)'.[83] A spokesperson from COPE advised the Commons committee that it provides 'advice to the editors, which they are free to ignore. We don't have any particular powers, except that we also provide a code of conduct and we ask all our members to adhere to that code'. Authors, editors, and members of the public can now file a complaint with COPE should a misbehaving editor work for an affiliated journal.[84]

In 2013, COPE issued a five-page ethical conduct guideline for peer reviewers. Tucked into the final page of that document is a bullet point declaring that, should an editor decide to 'provide a review' of a manuscript they themselves are handling, this fact must be revealed. According to COPE coordinator Irene Hames, editors struggling to find referees sometimes write up fake reviews under the cover of anonymity: 'I have come across editors who have almost boasted about it and said: 'I never have a worry about finding reviewers because I just do it myself'.[85]

Many scientific insiders are fully aware that the watchdog is feeble. For years, these critics have reminded anyone who'd listen that peer review doesn't certify accuracy and therefore can't possibly signal that a research paper is reliable in a public-policy context. But there's yet another problem. Some academics have seized hold of the leash and have turned the watchdog against those who pay the vet bills. In that context, a mechanism that helps publishers decide what to print has metamorphosed into an aggressive, snarling beast patrolling the perimeter between the Ivory Tower and the rest of us.

NASA's arsenic debacle

On a Monday in late November 2010, NASA issued a press release about a news conference scheduled for Thursday that would 'impact the search for evidence of extraterrestrial life'.[86] On the day of the news conference, the public was told that bi-

ology textbooks would need to be rewritten due to research that had just been published on *Science*'s website. NASA-funded scientists had discovered a microbe with the unique ability to build its DNA out of arsenic rather than phosphorus, altering our 'fundamental knowledge about what comprises all known life on Earth'.[87]

The paper's lead author, geobiochemist Felisa Wolfe-Simon, became an instant celebrity. Three months to the day later, she spoke at a TED event.[88] A month after that, *Time* magazine designated her one of the 100 most influential people in the world.[89] But on the Saturday following the news conference, Rosie Redfield, a microbiology professor at the University of British Columbia, posted a detailed and scathing critique of this research on her own blog. If the data in the *Science* paper had been presented to her by a PhD student, she said, she'd have sent him or her 'back to the bench to do more cleanup and controls'.[90]

Journalists began eliciting second opinions about what had been a celebratory, front page story. Carl Zimmer, who writes a science column for the *New York Times*, contacted a dozen experts who almost unanimously agreed that the NASA scientists had 'failed to make their case'. One said the paper shouldn't have been published. Another said it was 'pretty trivial to do a much better job'.[91] Wolfe-Simon was invited to respond. But although she'd had a great deal to say at the press conference, she suddenly didn't think the public square was where these matters should be discussed. Her e-mail response to Zimmer urged him to

> …honor the way scientific work must be conducted. Any discourse will have to be peer-reviewed in the same manner as our paper was…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.

One of Wolfe-Simon's co-authors similarly advised Zimmer that researchers who 'debate the questions or comments of others' in the media 'have crossed a sacred boundary'.[92] The publicly-funded NASA also demurred. A Canadian news outlet reported that,

> …when asked about public criticisms of the paper in the blogosphere, [NASA spokesperson Dwayne Brown] noted that the article was peer-reviewed and published in one of the most prestigious scientific journals. He added that Wolfe-Simon will not be responding to individual criticisms, as the agency doesn't feel it is appropriate to debate the science using the media and bloggers. Instead, it believes that should be done in scientific publications.[93]

Zimmer was stunned. In his words, 'I've been doing this kind of thing for a long time, and I have *never* encountered a response like this one from the hundreds of scientists I've interviewed'.[94] David Dobbs, a science writer at *Wired*, was caustic. Employing adjectives such as 'anti-empirical' and 'pre-Enlightenment,' he insisted that microbiology professor Redfield 'is a peer, and her blog is peer review'. NASA was dismissing criticism not on its merits, he said, but because Redfield hadn't delivered it

'standing on the proper altar' in the 'Church of the Peer-reviewed Journal'.[95] For his part, Jonathan Eisen, an evolutionary biologist at the University of California-Davis, described the situation as absurd. These researchers, he said, 'carried out science by press release and press conference…they are now hypocritical if they say that the only response should be in the scientific literature'.[96]

Hiding behind peer review

Zimmer's remarks notwithstanding, this behaviour isn't unique. The summer prior to NASA's press conference, in a column published in the *Independent*, British journalist Nigel Hawkes addressed what he called a 'sinister development'. Academics who've authored 'provocative or implausible claims in peer-reviewed journals have started arguing that they won't listen to criticism unless it has undergone the same laying-on of hands', he wrote.[97]

Hawkes points to professors Kate Pickett and Richard Wilkinson, the authors of the 2010 book, *The Spirit Level: Why Equality is Better for Everyone*. In fairness, they did issue a 22-page response to their detractors. But at the top of that document this statement appears in bold, italicized type:

> NOTE: Almost all of the research we present and synthesise in *The Spirit Level* had previously been peer-reviewed, and is fully referenced therein. In order to distinguish between well founded criticism and unsubstantiated claims made for political purposes, all future debate should take place in peer-reviewed publications.

Feedback from the academic community is 'well founded'. Feedback from the rest of humanity is unsubstantiated and politically motivated. This is an odd argument for champions of equality to be making. A few paragraphs later, Pickett and Wilkinson declare that their overall analysis has been subjected to peer review and that their research has been funded by four separate agencies, 'all of whom subject research proposals to rigorous reviews'. On page 18, part of their response to a question about life expectancy includes this assertion: 'if you have good evidence [to support an alternate perspective] it should be presented in a peer-reviewed journal'.[98]

The Amazon UK website currently describes *The Spirit Level* as 'the most influential and talked-about book on society in the last decade'.[99] Journalist Hawkes says that while the authors' thesis may be correct, their views about public debate are not. Surely, he says, when you write a book for a lay audience 'about a hugely political subject such as inequality, you've surrendered any right to hide behind the flak-jacket of peer review'. Attempting to do so 'is a betrayal of all the principles of academic life, of open dialogue, of freedom of expression and…of equality as well. It's a disgrace'.

Hawkes points to another incident, involving World Health Organization research about Caesarean births published in *The Lancet*. As a journalist with a special interest in the misuse of statistics, Hawkes had challenged the study's analysis in a January

2010 column. 'Did no one check the arithmetic in the tables, which are full of errors?' he'd asked. '*The Lancet* is a distinguished journal – were its referees asleep?'[100]

Hawkes reports that after a copy of his column was forwarded to the journal by a childbirth advocacy group, an editor snootily replied: 'We are a scientific journal, and as such prefer to see the scientific debate continued by reference to other academic articles that have been peer-reviewed'.[101] The only knowledge we scholars recognize is the knowledge we ourselves generate. Challenges to this knowledge can only take place on territory we control.

Errors easy to find, hard to fix

Power dynamics aside, the above (admittedly truncated) response displays no zeal for ensuring an accurate scientific record. Although the Commons committee was repeatedly told that peer-reviewed journals perform this important service, once again we find a gap between rhetoric and reality.[102] A 2016 *Nature* article provocatively titled 'A tragedy of errors: mistakes in peer-reviewed papers are easy to find but hard to fix' provides a disturbing demonstration.[103] Over a period of 18 months, a team of four American academics attempted to correct the scholarly record after noticing dozens of substantial errors in published medical papers. Some of these papers 'described mathematically or physiologically impossible results' such as an 'average height change of about 7 centimetres in adults over 8 weeks'. While one study they targeted was retracted in response, that case isn't representative. The majority of their experiences suggest that the academic publishing system was never designed to be self-correcting.

The American academics observe that journals rarely indicate who should be contacted about errors, and don't deal with these matters in a systematic fashion. Editors 'seemed unprepared or ill-equipped to investigate, take action or even respond'. Discussions involving editors, authors, and 'unidentified journal representatives' would often drag on for months 'without any public statement added to the original article' to indicate that concern had been expressed about its soundness. Even when gaffes were eventually established or admitted, the process was Kafkaesque. 'Some journals that acknowledged mistakes required a substantial fee to publish our letters', they report, citing a US$1,716 charge in one instance and a US$2,100 charge in another. While trying to do the right thing, they say, 'we were asked to spend our research dollars on correcting other people's errors'. One publisher, they assert, 'states it will charge the author who initiates withdrawal of a published paper US$10,000'.[104] An industry that prizes accuracy does not look like this.

Now let us return to NASA's arsenic-based life paper published online in December 2010.[105] In May 2011, *Science* published eight short Technical Comments by third parties expressing doubt about the paper.[106] Then, in July 2012, the story took a sharp turn. *Science* published two studies by two teams of researchers (one of which in-

cluded microbiology professor Redfield).[107] It also issued an editorial statement to the media that declared unequivocally: 'Contrary to an original report, the new research clearly shows that the bacterium…cannot substitute arsenic for phosphorus to survive'. This means that the NASA paper 'does not break the long-held rules of life, contrary to how Wolfe-Simon had interpreted her group's data'.[108]

Science has not, however, formally retracted the NASA study – a step common enough that an entire blog, RetractionWatch.com, is devoted to tracking such announcements. Nearly four years after this study was refuted, the web page on which it is published makes no mention of this fact. Readers aren't warned that they're looking at discredited research.[109] Ambiguous corrections to the scientific record are of limited value. The public isn't clairvoyant. Nor are journalists who weren't watching closely when these events unfolded.

When *Science* published the eight technical comments in 2011, it told the media:

> These post-publication responses are an essential part of the process by which science moves forward, correcting itself when necessary. We hope that the study and the subsequent exchange being published today will stimulate further experiments – whether they support or overturn this conclusion. In either case, the overall result will advance our knowledge about conditions that support life.[110]

This is called playing the peer review 'Get out of jail, free' card. On the one hand, many academics insist that the research they present to the world has been fully vetted. Indeed, they often behave as though it meets a standard unrivalled elsewhere. On the other hand, they take no responsibility when information they've produced turns out to be mistaken. In such cases everyone is then reminded that scholarly publishing is really just an exchange of ideas. The unwashed masses may be foolish enough to confuse peer-reviewed studies with the gospel truth, but sophisticated academics know that all knowledge is just provisional.

No quality assurance

When the House of Commons committee examined peer review in early 2011, one witness after another insisted it was a 'robust' and 'rigorous' quality-control mechanism.[111] But it is not. The remarks of Martin Hill, a systems engineer who submitted written evidence, are highly relevant. Peer review, he says, can't provide bona fide quality assurance 'because it occurs far too late' in the research process. 'Quality has to be assured from the beginning of the work, not in retrospect', he says. 'You cannot check that equipment has been operated correctly after the event'. Unless an inspection was conducted at the time, there is no way of knowing 'if equipment was properly clean'. Nor are referees in a position to know if some data was discarded and other data fabricated.[112]

Examination of a manuscript by a few unpaid volunteers is not a serious quality control mechanism.

Conclusion to Part 1

When Nobel laureates, scientific lobby groups, academic publishers, or climate officials imply that peer-reviewed research is self-evidently sound and accurate, they are promoting a myth. Rigorous skepticism is a sensible response to myths – the same skepticism that animates genuine scientific inquiry. Back in 1974, the late physicist Richard Feynman delivered a commencement address in which he discussed South Sea islanders who had constructed an airstrip, a hut for an air traffic controller, and equipment resembling headphones and antennas. After taking these steps, the islanders were disappointed that planes laden with goods didn't automatically begin landing on the island as they had during wartime. The islanders had, after all, followed the recipe conscientiously.[113]

Feynman coined the term 'cargo cult science' because it's possible to go through what appear to be all the right motions and yet not end up with a valid result. Peer review is an example of cargo cult thinking. Despite the overwhelming evidence to the contrary, the academic establishment continues to believe that if a series of pre-publication procedures are followed, sound science emerges at the other end. In the words of former BMJ editor Richard Smith, 'How odd' that a tool so closely linked to science should, in fact, 'be rooted in belief'.[114]

In reality, peer review is an often perfunctory process. It takes place behind closed doors, with no enforcement of even minimum standards. The fact that a single scholarly journal – among an estimated 25,000 – has performed a peer review ritual tells us little.[115] The paper's data and computer codes have not been thoroughly examined. Its arithmetic hasn't necessarily been checked. Its statistical analysis may or may not have received informed scrutiny.

As the US National Science Foundation has recently reminded us, a scientific finding 'cannot be regarded as an empirical fact' unless it has been 'independently verified'.[116] Peer review does not perform that function.

Policymakers, journalists, and members of the public need to abandon the idea that peer-reviewed research is a sound foundation on which to base public policy. The reproducibility crisis currently gripping the scientific world indicates that, despite the widespread use of peer review, much of published academic research 'may simply be untrue'.

2 Climate implications

In 2008, Rajendra Pachauri, the chairman of the Intergovernmental Panel on Climate Change (IPCC), told a committee of the North Carolina legislature that the global warming that had occurred since the middle of the 20th century was 'most probably' the fault of humanity. A recent IPCC report had reached this conclusion, he said,

and his audience could be assured the report was authoritative because:

> we carry out an assessment of climate change based on peer-reviewed literature, so everything that we look at and take into account in our assessments has to carry [the] credibility of peer-reviewed publications, we don't settle for anything less than that.[117]

Pachauri's chain of logic went like this: scientific literature is reliable because it has been peer-reviewed. IPCC conclusions are reliable because they rest on that firm foundation.[118] But peer review is a castle built on sand. Pachauri's linking of the terms 'credibility' and 'peer-reviewed' highlights the disturbing fact that unverified academic research frequently becomes the basis of real-world policy decisions.[119]

This report has shown that academics across different fields are prone to 'collective illusions' that may lead them to exaggerate the strength of certain evidence. It has explained that scientists are typically neither saints nor groundbreakers. Rather, they are professionally committed to the theoretical perspective on which their own careers have been built, and are frequently hostile to alternative viewpoints. We have also seen that one quarter of academics may be so desperate to be published in prestigious venues they will permit opinionated referees and editors to introduce errors into their work.

There is no reason to believe that the politically charged arena of climate science is exempt from these problems, or that it doesn't share the alarming rates of irreproducibility observed in medicine, economics, and psychology. Indeed, non-transparency is an acute problem in climate science due to the use of climate modelling via supercomputers that cost tens of millions of dollars and employ millions of lines of code.[120] Outsiders – whether they be other scientists, peer reviewers, or journalists – have no access to the specialized software and hardware involved, and it is difficult to imagine how such access might be arranged, never mind the person-years that would be required to fully explore the subtle computational and mathematical issues arising from a careful audit of such a model.

Reproducibility is the backbone of sound science. If it is infeasible to independently evaluate the numerous assumptions embedded within climate model software, and if third parties lack comparable computing power, a great deal of climate science would appear to be inherently non-reproducible.

The world is currently spending billions on measures intended to combat humanity's allegedly significant role in climate change. The IPCC tells us this is prudent and necessary. But IPCC reports are based on thousands of research papers whose conclusions have never been independently verified. If half of published, peer-reviewed papers 'may simply be untrue', half of the papers cited by the IPCC may also be untrue.

We need to re-examine what we think we know about the climate. In response to the reproducibility crisis in medicine, the beginnings of a system of accountability are now being established. Researchers can confidentially submit their completed work

to an independent, fee-for-service lab prior to publication. Findings successfully reproduced in this manner earn an 'independently validated' designation.[121] The Laura and John Arnold Foundation is currently funding replication of 50 important cancer studies. The Prostate Cancer Foundation has arranged for select research to be assessed. And the Antibody Validation Project is attempting to 'validate thousands of commercial antibodies' so that healthcare dollars and expertise aren't wasted by a series of researchers stumbling down the same blind alleys.[122]

Currently, climate research is not subjected to meaningful due diligence prior to the IPCC presenting it as sound in its reports. Wealthy green charitable foundations and government funding agencies have the power to change that by pursuing measures similar to those now occurring in medicine.

Until key climate findings meet a higher standard than mere peer review, we cannot claim that our climate policies are evidence-based.

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