5: Innovation and peer review

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Editorial peer review has often been blamed for the stifling of truly innovative scientific ideas but we hear little complaint from the authors of those works of great innovative significance that were immediately welcomed by funders, reviewers, editors, and scientists at large. We have no useful data, but if peer review indeed suppresses the new, this may be due to the underlying tension between creative ideas and the need for journals and grant giving bodies to ensure some basic level of quality control. Editors and funders have an interest in innovation, and must constantly remind themselves of this. Electronic publication may promote dissemination of innovations, but this will still not guarantee acceptance of the new.

Are manuscripts containing innovative material rejected?

In June 1899, Dr Denslow Lewis, of Chicago, presented a paper at the 50th Annual Meeting of the American Medical Association, in Columbus, Ohio. The paper was called "The gynecological consideration of the sexual act". In it Lewis detailed the normal process of human sexual intercourse, and discussed sexual education of the bride (grooms being assumed to have had previous experience with prostitutes), marital incompatibility, sexual response in women, female homosexuality, and treatment of sexual disorders.[1]

Immediately afterwards, Dr Howard Kelly of Baltimore stood up and starting "With all due respect to Dr Lewis" went on to demonstrate this respect by asserting he was "strongly opposed" to Lewis reading the paper, saying "its discussion is attended with more or less filth and we besmirch ourselves by discussing it in public".

Though it was the custom for JAMA to publish the papers given at the annual meeting of the American Medical Association, it was 84 years before JAMA did so,[2] yet this article was so revolutionary that it was included in *One Hundred Years of JAMA Landmark Articles*, published in 1997.[3] Lewis had had it published as a pamphlet by a Chicago publisher in 1900, and its publication in JAMA would never have happened at all except for its chance discovery by Dr Marc Hollender in 1982.[4] Hollender described a vigorous correspondence in which Lewis strongly argued that his paper was the result of lengthy scientific observation, and that it was his duty to ameliorate the unhappy situation of young women.

Lewis appealed the decision of the then editor of *JAMA*, George H. Simmons, who believed it out of place in *JAMA*. Simmons suggested major changes, but Lewis felt these would wreck the paper. "An elimination of all references to the physiology of coitus, the psychic phenomena incident thereto, and the importance of a correct education of the young in sexual hygiene takes away my major premise and my deductions are without scientific merit."[4] The publications committee of JAMA backed the editor by two votes to one, one of the committee changing the discussion by saying that the AMA would be "open to the charge of sending obscene matter through the mail". This, despite the opinion of the eminent lawyer, Clarence Darrow that "any physician who did not have the courage to deliver such a paper before an association of scientific men, when he believed it was for the purpose of making people better and happier, and who hesitated for fear that some law might be construed to send him to jail, would not be worthy of the profession to which he belongs".[4] Attempts to distribute the pamphlet to AMA delegates, and appeals to the president of the AMA, the trustees and the general executive committee were similarly unsuccessful.

Here is a classic case of a revolutionary article being turned down after editorial peer review because it was unbearably new. The justification of those who thought it would corrupt young minds was cleverly twisted into a legal one that served only to cloud the issue and act as a smoke screen for those who opposed the article's publication. While it might be argued that the opposition was not "scientific", it is clear that the opposition was from medical and scientific men, and by and large they objected because to publish Lewis's observations and theories was to go against their clinical view of what should be published.

Sommer, in a long article, full of examples as well as allegories and neologisms, has discussed discoveries, serendipitous and otherwise, that have undergone what Sommer calls "cruel suppression".[5] *The Three Princes of Serendip* is a fairy tale based upon the life of Bahram V Gur, king of Persia, known for his eccentric, arbitrary, and despotic ways. Sommer calls the suppression "bahramdipity", and the studies cruelly suppressed by more powerful individuals "nulltiples". When "resistance to new ideas rises to abusive and destructive levels, it is bahramdipitous".[5] Sommer intends that bahramdipity be a term that applies to abuse that is hierarchical, personal, undisguised, *ad hominem*, private, and where the subordinate is relatively powerless, so he excludes peer review. Nevertheless, editors will readily recognise senior reviewers who use the power invested in them as reviewers in a hierarchical, personal, and unscientific fashion.

A good example occurred in the field of geomagnetics. The idea of plate tectonics – shifts in the earth's mantle as a result of thermal convection – was proposed by Arthur Holmes in 1929 but did not receive attention until the early 1960s.[6] In 1963, Lawrence Morley wrote a short paper that "locked three disparate and unproven theories together in a mutually supportive way: the theories of continental drift, sea floor spreading, and the periodic reversing of the geomagnetic field".[7] He sent the paper, written after what he described as his "Eureka moment", to *Nature* in February 1963. *Nature* rejected it after two months. So, in August 1963, did the *Journal of Geophysical Research*, the anonymous reviewer's comment, sent on by the editor, stating: "Found your note with Morley's paper on my return from the field. His idea is an interesting one – I suppose – but it seems most appropriate over martinis, say, [rather] than in the *Journal of Geophysical Research*". A month later Morley was mortified to find a paper in *Nature* by two other scientists independently describing essentially the same idea he had attempted to publish twice, and, moreover, in the same journal to which he had first sent it. "Obviously I could not publish elsewhere because I could have been accused of plagiarism". Discussing this 38 years later, in an essay that includes the full *Nature* manuscript, finally published, Morley writes about reviewers "the very expertise that makes them appropriate reviewers also generates a conflict of interest: they have a vested interest in the outcome of the debate. We could call this the 'not invented here syndrome". The effect was what Sommer might call bahramdipity making a nulltiple,[7] though I do not think we need new words for an old phenomenon we all recognise.

So it is easy to show that some peer reviewers have been biased against some innovative articles, but is this usual? The cases of Lewis and Holmes both reflect the lengthy period it may take the scientific community to accept a revolutionary idea. Though Morley's case may be thought to be typical of the reception of a highly innovative and important idea that completely overturns established theories, it is really more ambiguous. He certainly suffered harm at the hands of an abusive reviewer, and his manuscript was thoroughly suppressed, so he has legitimate claim that peer review is biased against innovation. But what of the scientists who scooped him in *Nature*? They might reasonably attest to the openness of peer review to new theory. Might not the difference be due to the selection of reviewers in the two cases?

Stanley Prusiner received the Nobel prize in physiology or medicine in 1997 for his work on prions. He began to set up a laboratory at University of California San Francisco (UCSF) in 1974 to work on scrapie, thought then to be due to a "slow virus". In his autobiography, Prusiner tells of writing dull, readily funded grant proposals to study choroid plexus glutamate metabolism, in order to fund his early work on scrapie, the implication being that this would finesse a grant peer review system stacked against controversial new ideas. Prusiner describes finding, to his surprise, that the "virus" had protein but no nucleic acid; and simultaneously losing his funding and being told, fortunately in a decision that was later rescinded, that he would not be promoted to tenure. His 1982 article, introducing the term "prion", "set off a firestorm", he wrote, scientists in the field reacting with incredulity and anger. Some of these vented their frustration by involving the media and "the personal attacks of the naysayers at times became very vicious",[8] showing that those who attempt to change the established model and who manage to publish can still be punished. Prions became generally accepted only in the late 1990s. Here editorial peer review did *not* delay a revolutionary discovery that yet caused enormous hostility when it was published. Such examples suggest that sometimes editors and reviewers are well ahead of their communities.

If delay by peer review is the norm, how common is it?

In 1989, Horrobin, in an important paper given at the First Peer Review Congress, provided cases of defective peer review, sometimes due to highly pathological behaviour on the part of reviewers, and alleged that many reviewers were against innovation unless it was their own innovation.[9] Campanario has written extensively on influential books and papers that have had difficulties with editors and reviewers.[10–12] In an attempt, admittedly rough, to go beyond mere listing of anecdotes, of which he provides many, Campanario took advantage of yet another initiative taken by the inventive Eugene Garfield.[13] ISI (Institute for Scientific Information) has published *Citation Classics*, as a part of *Current Contents*, from 1977 onwards.[13] The authors of 205 of the 400 most cited articles of all time wrote commentaries on their articles for *Citation Classics*, and were encouraged to describe difficulties in the revision phase and outright rejection by a journal.[12] Twenty-two, or 10.7%, reported such difficulties, 11 of them rejections. While those who were rejected may have been more likely to respond to an invitation to write the commentary, this confirms what we already know about peer review: it is at best an exceedingly crude filter.

In short, we have no reliable figures as to the incidence of rejection by peer review of truly original research. Given that we cannot know about those manuscripts that are never published, it is unlikely that we will ever get a reliable incidence.

Another reason is the difficulty in establishing the exact definition of innovation required to perform the necessary studies. Innovation here means what is established by the introduction of novel methods, new practices, and original ideas. Authors almost always believe that their manuscripts describe something new, but whether something is truly innovative is very much up to the eye of the beholder, whether reader, editor, or reviewer. Victor Fuchs and Harold Sox attempted to measure the importance of medical innovations according to 225 general internists. They found mean scores for innovation were rated higher for procedures than for medications, and that cardiovascular treatments rated higher than others. However, innovation is an inexact quality, the physicians' ages and their patient mix being important in the physicians' evaluations of innovations.[14] My guess is that panels of pharmacologists or of venture capitalists would have drawn up very different lists.

Why might innovations be rejected?

Peer review is part of the "organized skepticism" that Merton described as being one of the four norms defining the scientific culture[15] and is often regarded as a quality control mechanism.

Kuhn,[16] in his discussion of changing generally accepted models, paradigms, in science, notes that new paradigms are inevitably limited in scope and precision at the time of their appearance. "No part of the aim of normal science is to call forth new sorts of phenomena; indeed, those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others". Kuhn points out that as change requires scientists to see nature in a different way, it is a mark of those who make revolutionary discoveries that their minds are already convinced of anomaly in the prevailing paradigm – that something is awry; and that they are young or new to the field so have little invested in old theories. "… these are the men who, being little committed by prior practice to the traditional rules of normal science, are particularly likely to see that those rules no longer define a playable game and to conceive another set that can replace them".[16] This applies to the situation in the field of slow virus research when Prusiner first announced his findings. Kuhn, noting that both Darwin and Max Planck did not expect acceptance of their work until those in opposition died, continues: "The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced".

Editors will recognise the truth of much of this from their everyday experience. Just as those who introduce new ideas are somehow already convinced of a new view that enables them to reinterpret data ("If I hadn't believed it, I'd never have seen it") so the strength with which we hold on to outdated theories is impressive. Indeed, this might be one reason why young reviewers, who may be less invested in particular theories, tend to get higher marks from authors and editors than do older reviewers.

Truly innovative manuscripts will go against accepted teaching and may threaten reviewers whose whole career in research and perhaps whose income from clinical practice may both be invested in an older model. For example, psychiatrists who have been making a living from treating business people whose ulcers they blame on stress, when those ulcers may be cured by antibiotics, are unlikely to welcome the new model. Nor are pharmaceutical companies with a vested interest in therapies to suppress acid. As Kuhn and others have pointed out, at first the new ideas will be based on incomplete evidence so they are easy to criticise. And, though in science the facts are supposed to speak for themselves, editors often see reviewers consciously or unconsciously raising the bar for papers presenting unfamiliar material.

Reviewers are in a bind, stuck somewhere between trying to wrap their minds round the new notion, and a strong feeling that the old notions are not broken so do not need to be fixed. Meanwhile authors assert the originality of their work routinely so editors can be forgiven for treating this claim with scepticism. Given all this and the extraordinarily conservative nature of human beings, including scientists, it would be an extraordinary phenomenon if the community of scientists, alone of all social communities, or editors and reviewers, alone of all members in these communities, were to welcome revolutionary ideas.

Is peer review the reason for rejection of innovative manuscripts?

One of the layers of quality control Horrobin discussed, largely instituted since the second world war,[17] was formal peer review of research applications and research reports. In 1989, when he presented his opinions at the First Peer Review Congress, Horrobin had been particularly scathing about the stifling effects of peer review on innovation[9] and his opinions had not changed in 2001.[18] In the first article, he argued that peer reviewers "should always be asking the question, 'Is this a possible innovation that should be encouraged because at some time it could lead to improvements in the treatment of patients?"' He discussed the creative tension between innovation and quality control, "between, on the one hand, originality, creativity, and profundity and, on the other, accuracy and reliability". Horrobin, who rightly equated peer review for journals with that for conference programmes and awarding of grants, felt that the balance had shifted so much to the side of quality control to the detriment of patient care "that innovative articles should be deliberately encouraged and more readily published than conventional ones".[9] In his most recent article on the topic,[18] he took advantage of the great increase in scientific interest in peer review in the intervening years, largely brought about by the peer review congresses,[19] which has demonstrated that peer review is not all it was cracked up to be.

He alleged that journals and, more importantly, grant giving organisations are largely uninterested in open evaluation and validation of peer review and asked whether the peer review process in academia and industry might be destroying rather than promoting innovation.[18]

Is peer review up to the task of reliably detecting and welcoming important innovations?

Bacchetti makes the point that in both editorial and grant peer review, it is common to find unwarranted, ignorant, nit-picking and spurious criticism of sound statistics, particularly in the areas of sample size and multiple comparisons.[20] He feels that "A pervasive factor is the desire to find something to criticize", criticism and conflict being overvalued in our culture.[21] Bacchetti also notes that those who study peer review concentrate on finding flaws and on completeness, rather than on whether the reviewer's judgement is correct.[20] He is merely adding to the growing evidence that peer review, when viewed as a test, has operating characteristics that are far too crude and depend far too much on individual bias, for us to expect it would invariably select all highly innovative articles that later turned out to be important.[19] Given that it may be years before the community has largely embraced these articles after eventual publication, how could we possibly expect otherwise? Editors are those who seem to be most enthusiastic about the virtues of peer review, and this may well be because of the immense material assistance given to them by the reviewers, and the way the system allows editors to share the blame for rejection.

What happens when we abandon peer review?

If editorial peer review is so hapless, why do we not abandon it? Peer review could never be blamed for delay or suppression of publication of innovations if it were to be abolished. The cold fusion story is a classic example of what happens when peer review is circumvented.[22] Stanley Pons and Martin Fleischmann, of the University of Utah, without going through the formality of peer review by a journal, announced at a press conference on 23 March 1989 that they had achieved nuclear fusion at room temperatures. Indeed, when they submitted their paper to *Nature*, publication was refused because three peer reviewers had serious criticisms of the work. The editor, John Maddox, announced this publicly, noting that Pons and Fleischmann had not done the "rudimentary control experiment of running their electrolytic cells with ordinary rather than heavy water … This glaring lapse from accepted practice is another casualty of people's need to be first with reports of discovery and with the patents that follow." This was not a popular view. When it was suggested that the rejection by *Nature* should lead to caution in the allocation of Utah state funds to cold fusion, Bud Scruggs, the governor's chief of staff, announced that "We are not going to let some English magazine decide how state money is handled".[22] A California congressman wrote that the anti-cold fusion faction consisted of "small, petty people without vision or curiosity."

Despite the unhappy results of *Nature's* peer review, in a matter of days and weeks, scientists all over the world were reporting confirmatory results from hasty experiments, usually without proper controls. The scientific reporters were equally gullible. The *Wall Street Journal*, which continued to give the story a ludicrously strong positive bias, on 12 April 1989 summed up any criticism of the Utah scientists as "the compulsive naysaying of the current national mood". The American Chemical Society, even though it had regulations requiring peer review to prevent the dissemination of specious findings at its meetings, waived its requirements for peer review in the case of proponents of cold fusion. The Electrochemical Society arranged a symposium calling only for "confirmation results".[23]

Gradually, solid evidence that there was nothing to cold fusion built up, and a report in October by the US Department of Defense concluded that there was no reason to believe in the phenomenon. In all sorts of ways, this process was a reinvention of the peer review that Pons and Fleischmann had, in their passion for their theory, so thoroughly flouted. But the cost had been enormous, not just in loss of public confidence, and loss of scientific reputations, but financial. For many months, at least US$1m was being spent every week on cold fusion in the United States, and perhaps the same again elsewhere.[22] All of this could have been saved if the peer review system at *Nature* had been allowed by the scientists to function normally.

Will this happen again?

Of course. It is always easy to claim suppression of ideas, and editors, who are charged with selecting the best manuscripts from those they receive, are constantly the object of accusations of "censorship" from their rejected authors. Yet, given the enormous numbers of scientific journals, there seem to be almost no barriers to eventual publication.[19,24] Even as those with original theories claim suppression of their ideas by journals, they are still able to publish, something Horrobin, for example, admitted.[18] It is obvious that what the authors really want is not merely publication, but publication in a specific journal with high prestige, and consequently strong competition for its pages – precisely those journals where the editor has the hardest time making room for articles that are less than solid.

It is my experience that rejected authors will refuse to accept considerations of quality, lack of space, and so on in direct proportion to their passion for their ideas. They will assert that the reviewers are unfair, biased, old-fashioned, timid, and simply unable to comprehend the new paradigm-changing theory. They may even claim that they have no peers and that only they are able to judge the worth of their own theories, which apparently need little or no experimental evidence to back them up.

Taubes, discussing the public reaction to the scientific quarrel about cold fusion, vividly describes the position in which such people try to put the editor. "There was, of course, something of a catch-22 in this attitude: if you knew enough nuclear physics to understand why cold fusion appeared to be dead wrong, you were, by definition, sadly attached to the old paradigm, thus small, petty, and lacking in vision. If you knew little, nothing, or absolutely nothing about nuclear fusion … then you were considered a visionary."[22]

There are, moreover, other ways to get around peer review, one of which is self publication. Recently, Stephen Wolfram self-published a remarkable book, *A New Kind of Science*,[25] which envisages the universe as some sort of giant computer, and to understand it, we have to figure out the algorithms in its software – "digital physics". Wolfram is wealthy enough to bypass peer review. George Johnson writes:

> Had Dr Wolfram been more demonstrative in parceling out credit to those who share his vision (many are mentioned, in passing, in the book's copious notes), they might be lining up to provide testimonials. It's the kind of book some may wish they had written. Instead they were busy writing papers, shepherding them through the review process, presenting them in conferences, discussing them at seminars and workshops – going through the paces of normal science. That is how an idea progresses. But sometimes it takes a bombshell to bring it to center stage.[26]

Are innovative papers rejected more frequently than non-innovative papers?

Ernst and Resch attempted to answer the question of whether reviewers were biased against the unconventional, and found none in a randomised controlled trial, though, like others, they found interrater reliability very low. They concluded that peer review itself had inadequate validity,[27] so it is hard to know what to make of the study.

Ghali *et al.*[28] concentrated on articles they thought the editors might have considered to be innovative. They looked into whether "fast-tracked" articles, which were presumably thought by authors and editors to have particular importance, were rated by a panel of internists to be more important than matched controls. In a small series, they found this to be generally, but inconsistently, the case.

Originality is much prized by editors, and if this study has any bearing on innovation, it suggests that we are far from being able to recognise it reliably, as the findings of Fuchs and Sox suggest.[14]

The important question in this context is whether the rate of rejection by journals of truly innovative manuscripts is higher than the usual rejection rate for non-innovative reports. To this crucial question, we have no answer.

The difficulties to finding an answer are formidable. The first problem has to do with the definition of “innovative”, which is very much in the eye of the beholder. At my journal, *JAMA*, it is usual for authors to assert the originality of their work, indeed, we specifically ask authors to declare that its substance has not ever been published or been sent to another journal and is original – in some way innovative. Of these reports, and therefore of these authors, 90% are fated to be disappointed. So some 20 000 rejected authors every year are in a position to allege that *JAMA* frowns on innovation. We editors at *JAMA*, who above all are eager to publish truly original and exciting work, can calculate that over 10 years, around 36 000 manuscripts will have been rejected. We are acutely conscious that somewhere in those thousands of rejected manuscripts there may well be a work of extraordinary, paradigm-shifting genius, but which one? Such manuscripts do not come in to us marked “truly innovative”.

The next problem has to do with skewed expectations authors have about peer review and of journals. Many of these rejected authors allege that the process of selection was unfair. Indeed, an important and depressing fact about peer review is that the satisfaction of authors with the process is far more closely associated with acceptance of their manuscripts than with the quality of the reviews.[29] Given that we now know that peer review is a test with unvalidated characteristics and is, at best, an extremely blunt sword – one far too blunt to be able to make a reliable cut between the best 10% of articles, and the next best 10% – their complaint is often valid. The decision to publish has to be based on further considerations (for example, other articles accepted or recently published; the necessity of covering all of public health; available pages, etc.) that have little to do with science. So most rejected authors will end up confirmed in their bias that reviewers are incompetent and unjust.[29] Retrospective analysis of important papers ignores completely all those rejected authors who at the time felt strongly that they were shifting some paradigm or other and were later proved wrong. Even if these authors were foolish enough to complain publicly, no one would listen or care.

Moreover, a retrospective examination is unlikely to help much beyond what has already been described by Horrobin and Campanario, simply because in the final analysis they looked at work that had always ended up being published somewhere, and it is the innovative work that is never published that should most concern us. Moreover, the innovative work that they studied was work later validated by the scientific community – a process that might have taken 20 years or more. At that point, those who appear in the lists made by Horrobin and Campanario are in a strong position to make much of their initial rejection – and do. We hear little complaint from the authors of those works of extraordinarily great innovative significance that were immediately welcomed by reviewers, by editors, and by scientists at large.

What are the consequences?

Rejection will clearly delay dissemination of innovative work, and may well sap the morale of the authors. If it is at the grant proposal stage, the rejection may kill the idea for ever. It is in the field of pharmaceutical innovation that one might expect the biggest and most measurable effects in the biomedical field and also the most energetic efforts to remedy delays.

There has been a steady fall in the number of new, innovative drugs. Though there are contrarians who take a different view and remain optimistic,[30,31] the rash of mergers between pharmaceutical companies over the past few years seems partly to be a reaction to the paucity of important, innovative drugs in the development pipeline.[32–35] Indeed, when such mergers are discussed in the media, the new drugs each partner has in the development stage always figure prominently.

A recent editorial in *Nature* suggests as causes for the failure in the new drug pipeline: the possibility that, as the "easy" diseases have been tackled, the remaining complex diseases with many causes are harder to address; that company mergers cause so much delay and confusion that good ideas perish; and above all, that monolithic companies are oppressive environments for ambitious and innovative young researchers. To this I would add some others. Though developmental costs are unknown, because they are hidden from the public and mixed with promotional costs, and because companies have a great interest in inflating them, development is still an expensive undertaking. Rather than develop new molecules, it is much cheaper for a company to extend existing patents by legal, not so legal, and political means, or make tiny modifications to already successful drugs and market them as new advances.[27,36,37]

The US Constitution has from the eighteenth century given the Congress ability to "promote the progress of science and the useful arts by securing to authors and inventors the exclusive right to their respective writings and discoveries"[38] and Resnick has argued that at any rate in the case of DNA patenting, this does not harm science, and is "likely to promote rather than hamper scientific innovation and discovery".[39] I am less sanguine. The rush by companies to patent molecules, genes and so on, and to insist that all products of research be regarded as trade secrets, must surely have had a chilling effect on the free interchange necessary for rapid scientific advance.

Horrobin argued forcefully that the layering on of ethical and managerial controls to prevent the recurrence of bad events has stifled innovation.[9,18,40] It is Horrobin's thesis that for many of the major diseases of mankind, treatments are scarcely better than 30 years ago and this is because we have become inured to advances being tiny and have "lost our taste for innovation". Innovators are rare, and in a culture where elaborate controls have been set up to prevent unethical behaviours, they are pilloried as being suspect and likely to harm patients. Ethical committees exceed their mandate and fixate on trivia, themselves behaving unethically in not regarding innovation as the highest imperative, and in a culture that insists that the absence of harm is the highest priority, suffocating layers of control have brought advances to a halt, while enormously increasing the cost of the small progress that is being made.[40]

No one who has witnessed the recent dramatic shaming and blaming of innovative researchers in the United States whose experiments have gone wrong, resulting in harm to patients, can deny that Horrobin was right about our cultural approach. Doubters should read the series of five detailed articles on experiments at the Fred Hutchinson Cancer Research Center in Seattle, published in the *Seattle Times* in early 2001[41] and the new rules on disclosure of financial conflicts of interest introduced very shortly afterwards by the US Food and Drug Administration.[42]

If the bias against innovation exists, what can we do about it?

The short answer, given that it is human nature to resist change, might be that there is nothing to be done. Why should we expect a revolution in the behaviour of editors and reviewers when the communities they represent tend to be so antagonistic to revolutionary new ideas? That said, I believe it is useful to consider possible changes to encourage innovation.

Grants

In the United States, agencies awarding grants are very aware of the issue. I was on an advisory team for the National Science Foundation (NSF) in 1996, and we devoted much time and attention to ways to encourage high risk, high pay-off proposals more vigorously by means of small exploratory grants, awarded not by peer reviewers, but by NSF officers. In particular, we felt that special attention should go "to proposals that get widely disparate reviews, which may sometimes indicate a creative or innovative proposal that warrants funding despite, or almost because of, some negative reviews."[43] How successful this will be remains to be seen. Giving reviewers feedback on, say, citation rates of projects they have funded, possibly to compare with those that were not funded yet still completed, might be useful, but granting panels tend to be temporary, while citation rates for innovative work will tend to grow over the long term.

New Mechanisms of Publication

The web, and such initiatives as BioMedCentral, will remove many of the difficulties frustrated authors find in publishing. But publication of innovative ideas does not mean anyone will either read or accept them. Given the decades it has taken anyone to notice many paradigm-shifting notions when they have already been fully published, I cannot be optimistic that this will solve the problem.

Journals

I see this as being entirely in the hands of the editors of prestigious journals, who should be chosen partly for their originality and willingness to take risks. The ways by which they encourage the publication of really original work should make up one of the criteria on which they are assessed. Editors should understand they cannot possibly please everyone, and should never attempt to. They must select open and constructive reviewers, but they must not cede decisions about manuscripts to reviewers, and must look on their journal as a home for the original, unusual, and unsafe. Their task is not to be bomb proof, it is to stimulate and sew seeds and publicise scientific papers that are so worth people's attention that they will try to refute them.

In March 2002, scientists again claimed to have produced "table top" nuclear fusion, in a paper reporting deuterium–deuterium fusion.[44] A controversy erupted, both sides pointed out that the first paper had undergone, and the rival one reporting no effect was undergoing rigorous editorial peer review.[45,46]

Don Kennedy, the editor of *Science*, in dealing with this case, described well the problems an editor faces when a really controversial manuscript comes to a journal.[47] He recounts attempts made by other scientists to belittle the work, and efforts by administrators at the authors' institution to block publication. Responding to criticism that as an editor he should not go "forward with a paper attached to so much controversy", Kennedy writes:

> Well, that's what we do; our mission is to put interesting, potentially important science into public view after ensuring its quality as best we possibly can. After that, efforts at repetition and reinterpretation can take

place in the open. That's where it belongs, not in an alternative universe in which anonymity prevails, rumor leaks out, and facts stay inside … What we are very sure of is that publication is the right option, even – and perhaps especially – when there is some controversy.

In 1991, I wrote:

We editors interested in innovation, who, like Tennyson's Ancient Sage, "cleave ever to the sunnier side of doubt"[48] must be inured to the fact that we will usually get egg on our faces … for it is the duty of the editor to stick his neck out. In the uproar, he can comfort himself by remembering that his journal is an arena, not just a pulpit. The great Rudolf Virchow said: "In my journal, anyone can make a fool of himself."[49]

Editors must accept that if they are the ones who look foolish, this is all part of the job.

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